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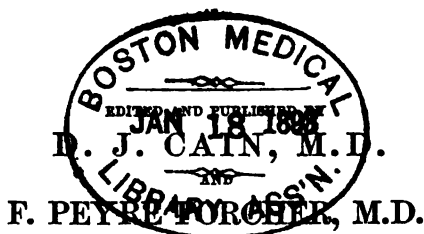
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THE
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Homo naturæ minister et interpres, tantum facit et intelligit, quantum de naturæ ordine, re vel mente observaverit; nec amplius scit aut protest.—BACON.

VOLUME IX.

CHARLES
JAMES, WILLIAM

STEAM-POWER P

INGER

GENERAL ALPHABETICAL INDEX

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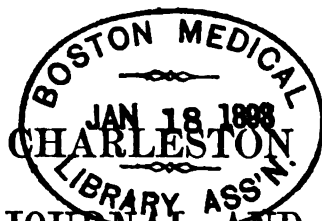
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[No. 1.

ARTICLE I.—*An Essay on the Mechanism and Management of Parturition, in the Shoulder Presentation.* By Wm. M. BOLING, M.D., of Montgomery, Alabama.

[Concluded from page 798 of last volume.]

I WILL speak of the main points in which the process, as it has occurred under my own observation in two cases,* differs from that in which the back of the child is primitively in front—selecting that of the right shoulder.

The head of the fœtus, it will be remembered, rests in the right iliac fossa, the breech in the left; its back is directed to the back of the mother, its abdomen to the pubes; its left side is directed upwards towards the fundus uteri, and the right downwards, towards the pelvic cavity.

One of the most important movements of the fœtus, for the favourable progress of the spontaneous evolution, is that in which the back of the child, instead of the side, is more or less fairly directed downwards, or towards the outlet—thus causing the back, instead of the abdomen, to be directed forwards, as regards the mother, on the expulsion of the body, and allowing of the curvature of the fœtus upon its anterior surface, instead of upon its side—so essential to its ready descent into the pelvic cavity. It is in the different direction of this movement, that the principal difference consists, in the mechanism of parturition in the first and second position. Thus, in the first position of the right shoulder, the left side of the fœtus, as the back is directed towards the

* Partial visceration had, to be sure, been performed; but though the process

pelvic cavity, turns forward and downward, while the right side rises somewhat, and is made to rest against the right side of the promontory of the sacrum. Now, in the second position, were the left side of the fœtus to turn forward in like manner, and the right, while rising, to be directed backwards to the side of the sacral promontory, the abdomen of the child would be turned downwards, or towards the outlet, instead of the back, with the effect, at a glance, sufficiently apparent, of rendering a spontaneous termination vastly more difficult, if not impossible, owing to the greater difficulty with which the fœtus may be flexed upon its posterior, than upon its anterior surface. It is a fortunate provision of nature, and as remarked by Cazeaux, "*il faut l'avouer, une chose bien remarquable, que nous retrouvons encore ici la loi générale, que nous avons vu régler tout l'accouchement naturel; quel que soit le rapport primitif du plan postérieur du fœtus, il vient en définitive se mettre en rapport avec les parties antérieures du bassin.*" On the contrary, in the second position, the left side, as the back turns downwards, or towards the outlet, turns slightly downwards, and obliquely backwards, so as to rest against the side of the sacral promontory, while the right side, becoming slightly elevated, is directed obliquely forward. Otherwise there is but little difference in the mechanism of the two positions. In the second, in the movement of rotation that brings the shoulder under the arch of the pubes, the head moves forward and from right to left, and the breech backwards, and from left to right; while, in the first position, the head moves forward and from left to right, and the breech backward, and from right to left. In the first position, the shoulder, as it makes its escape, rests at once, in part, as it were, against the left descending ramus of the pubes; but in the second position, the shoulder, as it first makes its escape, is more to the right, and by a somewhat more extended movement of rotation than takes place in the first position, and by which the head is made to rest more fairly over the pubes than in it, the acromion process is brought under the arch of the pubes, and the coracoid braced firmly against the left descending ramus. There the scapula remains fixed, while the back glides upon it, as it were, under the influence of the uterine contractions exerted principally upon the breech, till it is made to present in the direction necessary to admit of the curvature of the fœtus upon its anterior surface. The body thus favourably placed, descends readily into the pelvic cavity; the right side of the back protrudes through the vulvar opening; the back presents still more fairly its broad surface; its different parts, from above downwards, in succession escape; the

breech sweeps over the perinæum, and the legs are unfolded; all as in the first position. The head escapes, as in the second, or right sacro-acetabular position of the breech.

The mechanism is the same in the first or left cephalo-iliac position of the left shoulder, in which the abdomen is primarily in front, as in the second of the right, except, merely, in the direction of the movement of rotation.

It is fortunate, indeed, and by a wonderful adaptation of parts to the essential movements, that in the rotation of the fœtus which takes place prior to the escape of the shoulder, the head should be thrown forward instead of the breech. Were it otherwise, and should the head be rotated backward and the breech forward, the advantages gained by the escape of the shoulder under the pubic arch would be lost. The depth of the pelvic wall in the rear being greater than the length of the neck of the fœtus, even under the most energetic contractions, the shoulder would remain within the pelvis, braced against the anterior surface of the sacrum or sacro-iliac synchondrosis, while the breech would remain lodged upon the pubes. We can conceive it barely possible, however, that in a case in which the fœtus was very small in proportion to the size of the pelvis, the breech thus placed over the pubes might be forced into the pelvic cavity, and thrown out under the arch of the pubes.

Descent of the Arm. The effect of the descent, or expulsion, in advance, of the arm, in the presentation of the shoulder, was formerly regarded as a very serious complication; and in some of the older authors we are gravely directed to return the arm at once into the uterine cavity, as if such a procedure were generally practicable; while in others, directions are given for the amputation of the arm, or for wrenching it off, at the shoulder joint. "If the arm come too far out," observes M. De La Vauguion, who wrote towards the latter part of the seventeenth century, "and cannot be put in without great difficulty, by reason of its bigness, and it be certain the child is dead, it is then advisable to take it off as high as possible, beginning with an incision round it, and cutting through the bone with sharp pincers, as is practiced by Ambrose Paré; but I think it better to twist it once or twice round, for by this means it will easily separate from the body in the articulation, and there will be no danger of hurting the womb by the asperities of the bone."

Doctor Meigs speaks of a case which occurred a few years ago in France, in which a practitioner finding the arm, as he supposed, an impediment to delivery, amputated it at the shoulder joint, and yet the

child was born alive. The physician was prosecuted for maiming. He (Dr. Meigs) also speaks of a case of which he was cognizant, in which, though the arm was torn off, the child was born alive, with the end of the humerus projecting below the ragged and torn edges of the wound. The persons interested were made to believe that the lost arm had been destroyed by absorption. We can scarcely conceive of a degree of stupidity and ignorance equal to this. Many other cases may be found on record, in which this measure was adopted. Indeed, Doctor Rambotham alludes to a case, the particulars of which he quotes from the *London Lancet* for April, 1829, in which a practitioner at Cheneau, in the Department of Orme, France, finding the two arms protruding and livid, amputated them both, and yet the infant survived the mutilation, and was born alive, the wounds soon healing.

With others, the practice was adopted, on finding the arm prolapsed, of pulling forcibly upon it with the view of hastening delivery. Such a proceeding must, of course, be prejudicial in any case in which it might be proper to attempt to turn, or in which the child is alive, and can be of but little service indeed in advancing delivery by the evolution; for here the breech is the only moveable part, the head being lodged upon the pubes in such a way that no exertion upon the arm could bring it down into the pelvis. In one case, however, in which the operation of evisceration was performed by myself, and in which the pains had become feeble, their force seemed to be increased by the pressure upon the rectum and perinæum, caused by traction upon the arm, and it may possibly in some cases be useful in this way.

Again, by some of the older practitioners, as just remarked, the course was followed, or at least advised, as if generally feasible, of returning the prolapsed arm into the uterine cavity. "The hand or arm," says Vauguion, "must be thrust, as speedily as may be, into the womb;" and Burton, with this object in view, invented a kind of crutch, for the purpose of making pressure in the axilla, during the intervals from pain. Doctor Meigs quotes the following from the "*Woman's Booke*," of Thomas Rainbold, of the times of Queen Elizabeth: "And yf so be that it appears and comes forth first, the shoulder, as in the xi figure, then muste ye fayre and softlye thruste it backe again by the shoulders till such time as the head come forward." It would be in but a small proportion of cases, that, in pursuing such a course, the result here expected—the descent of the head—could reasonably be looked for; and even were such a practice advisable, the effect would be, it would seem, to cause the breech to descend, rather than the head. Indeed, a writer, even of recent date, (P. Cosgreave, in the *London Lancet* for

1828-9,) claims to have been so successful as never, in the shoulder presentation, to have lost a child by this practice of returning the arm. This, he says, he does in the absence of pain, and then retains it in the uterus till the evolution takes place, and the child is expelled by the natural powers. The number of cases in which he has been thus successful, is not given. Although the method might possibly succeed, in a few exceptional cases, any one at all familiar with obstetrical practice, and aware of the violence with which, generally, after the escape of the liquor amnii, the uterus contracts upon its contents, in the shoulder presentation, would hesitate to adopt or depend upon it, as a rule of practice. Not often would it be possible thus to return the arm; and in the few cases in which it might be otherwise, the state of relaxation of the uterus admitting it, would equally admit the performance, with facility, of the more certain operation of version; and which, moreover, it would be almost culpable to delay, at the risk of allowing the favourable moment to pass by.

Barlow recommends the accoucheur "to introduce a piece of sponge or other soft substance along the pelvis, during the absence of pain, and wedge or restrain the presenting hand or hands above the superior strait." Such a measure, of course, would be entirely inefficient, in a genuine shoulder presentation, though it might answer the intended purpose, in case of the mere descent of the hand with the head in a vertex presentation; and, indeed, these different presentations were apparently confused by Doctor Barlow, as was also the case, in all probability, by the author quoted before him.

Though at times we are shocked at the harsh measures, adopted by our predecessors, in cases in which the arm has prolapsed or protruded in advance, we are again not less amused at other methods of treatment recommended by them. For instance, various measures have been advised with the object of causing the fœtus, itself, voluntarily to retract the hand within the uterus—as pinching, pricking, the application of ice, &c. "If," says Heister, "the hand of the infant is perceived through the membranes at the mouth of the uterus, before the waters are discharged, it frequently withdraws that part, if the midwife pinches or hurts its fingers, and turns its head in the room of it, whereupon the natural birth succeeds."

Again, to procure a rectification of the position, various changes of posture, and movements of the body of the patient—some of them not only exceedingly uncomfortable and absurd, but painful and inconvenient—were recommended. The less objectionable measures of the kind were frequent changes of position from side to side, or from the side to the

back, resting upon the knees, &c.; but, in some instances, patients were made to jump, were shaken by assistants, and even suspended by the feet, with the head downwards. Smellie gives the particulars of a case to which he was called, in which a female practitioner, having failed by several of the methods above mentioned, to cause the child to retract its hand, she finally directed "the woman's husband to take hold of her legs, over his shoulders, and lift up her body three times, with her back to his and her head downwards—being of opinion, that, although the former methods failed of success, this would answer expectation."

Authors of a later period, generally—though Chailly, with a few others, seem still to admit the propriety of the amputation of the arm in some cases—taking a more correct view of the matter, it is believed, deny the necessity of such harsh measures as were formerly adopted. Indeed, having discovered that the importance of the descent of the arm had been overrated, some—while indulging in complacent congratulations on their superiority of knowledge to their predecessors—have gone so far, as to deny that it is any disadvantage whatever; nay, rather an advantage. The following remarks of Professor Meigs, taken from his valuable work on Obstetrics, may be very fairly regarded as representing the opinion of the profession, pretty generally, of the present time: "The hand and arm are merely prolapsed, and their prolapsion adds *nothing* to the difficulty of the case; indeed, their prolapsion serves as a means of guiding us in our diagnosis, and does *not at all* oppose the successful treatment of the labour. In the management of a pelvic presentation, I should, in general, prefer that the feet should not prolapse; in a shoulder presentation, it would be rather a favourable circumstance for the arm to prolapse." * * * "The arm actually is not in the way, for the hand of a practitioner, and the arm of a foetus at term, can never equal in size a circle sufficiently large for the head to pass through."

M. Cazeaux also remarks: "C'est la une circonstance a peu pres insignifiante, et qui même rend plus facile la version pelvienne." * * * "Ajoutons enfin, que cette main, ou plutot ce bras sert merveilleusement a diriger la rotation du tronc; qu'il offre une prise de plus pour les tractions qu'on exerce sur le tronc, et qu'enfin ou s'épargne l'operation, souvent trop penible, du degagement d'une epaule."

Now, while in no case would I view the descent of the arm as of sufficient importance to justify its amputation—an operation not without pain and danger to the mother, even though the child itself were known to be dead, (a conclusion which should not be hastily formed, from the lividity of the arm merely; for, owing to the pressure on the parts

above, and the constriction often occasioned by the os uteri, the arm very soon becomes greatly swollen, and much discoloured,)—there are certainly cases in which its presence in the vagina must embarrass considerably the efforts of the physician. For instance, in the operation of version, notwithstanding what M. Cazeaux says, could it possibly be otherwise, than that the presence of the arm of the child in the vagina must render more difficult and painful the introduction of the hand of the operator into the uterus? And, supposing his hand within the uterus, must not his arm, resting within the vagina, wedge, as it were, that of the child in its place, so as to render any elevation or movement of the chest, that might be desired, more difficult? Or, the feet being grasped, and the operation of version commenced, is it possible that the head and chest of the child can ascend, while the feet are drawn down—the arm of the foetus being wedged thus in the vagina by that of the operator—with the same facility that they could, were the arm in the uterus, and out of the way? It is really very difficult, it does seem to me, to understand how, as is asserted by M. Cazeaux, the descent of the arm can facilitate the operation of version. That it offers a means by which to make traction on the trunk, can be of no great service, because it is the breech, rather than the chest, upon which traction should be made after turning, evisceration, or in rendering assistance in cases in which there is a tendency to spontaneous evolution; but, even were this all otherwise, in the stage in which such traction might be desirable, the arm, if not previously expelled, could, of course, without difficulty, be withdrawn; and the operation of its disengagement, under such circumstances, should scarcely be considered or pronounced “trop pénible.”

But, though I cannot but regard the descent of the arm as an impediment, during an attempt to turn, or in any case in which turning might be proper, it is otherwise in cases in which, the time for version having passed, it may be proper to depend on spontaneous evolution for the delivery of the patient. Here, were the arm not protruded in advance, of course its volume would be added to that of the foetus in the cavity of the pelvis, at a time when the least additional bulk might offer a serious impediment to the favourable termination of the case. There are, then, cases in which the descent of the arm is a serious disadvantage; there are other cases, or a stage in other cases, in which the reverse obtains.

The complete expulsion of the arm through the vulvar opening, unquestionably indicates an unfavourable condition, for it will rarely indeed be found so situated, except in cases in which the liquor amnii

has been some time discharged, and the uterus so firmly—spasmodically, as it were—contracted upon its contents, as to preclude the safe performance of the operation of version. It was, probably, owing to difficulties encountered under such circumstances, that the ancients were led to look upon the descent of the arm with so much dread; to regard it, in short, as a separate presentation, rather than as a somewhat unimportant accident—a mere epiphenomenon of the shoulder presentation. They were, probably, deceived as to the chief cause of the difficulty, and would have found the shoulder presentation, with the arm not prolapsed, but a shade less troublesome, (some modern authors, it will be remembered, say more so.) than where it had escaped—supposing the shoulder itself to have descended equally low in the pelvis—in the operation of version; while, as already remarked, where the proper time for benefitting by this measure might have passed, it would, in reality, be a favourable circumstance, in the spontaneous evolution.

In delivery by spontaneous evolution, it will be found, on examination, that there are manifested the same stages or movements—or their analogues, it may, perhaps, more properly be said—as in delivery by the natural powers, in the vertex presentation, flexion, descent, rotation, extension or disengagement. M. P. Dubois, in the cases in which the abdomen of the child is primitively in relation with the pubes, has added another, that of external rotation; but it strikes me, unquestionably, from some mistaken view of the mechanism of the process.

Treatment. Although a knowledge of the possibility of spontaneous evolution, and a rational explanation of the process, must be a source of relief and satisfaction to the practitioner, in certain cases, it should never be forgotten that these are the exceptions, and that in all cases where it may be practicable without injury to the mother, it is his duty to anticipate the necessity of such a result, by the operation of version. Indeed, it has been truly observed, by more than one author, that it would have been better we should have ever remained ignorant of the fact, than to adopt, as a general rule of practice, a reliance upon it. "What practical application," says Doctor Churchill, "can be made of our knowledge of the occurrence of spontaneous expulsion? I am afraid not much. I am satisfied that we ought not to wait for it in any case in which turning can be easily accomplished; because, if it do not occur, the operation will be rendered tenfold more difficult, from the greater depression of the child and more energetic action of the uterus." And, says Cazeaux—"En consultant, en effet, les faits publiés, même en réfléchissant seulement au mécanisme suivant lequel cette expulsion a lieu, on voit qu'elle doit exposer la femme à un travail des plus douloureux,

et des plus longs, et le fœtus a une pression tellement violente que sa mort doit en être souvent la conséquence”—invariably, almost, the consequence, it might rather be said. Even Denman himself urges the impropriety of a dependence on spontaneous evolution, and advises turning in every case in which this operation may be performed with safety to the mother, or will give us a better chance of saving the child.

Having ascertained the existence of a shoulder presentation, we must be guided, in the management of the case, chiefly by the extent to which the labour may have advanced, the general condition of the patient, the rupture or not of the membranes, and the state of the os uteri and other soft parts. Although delivery by the operation of version is much the most desirable termination—where it is possible, with safety to the mother—of such cases, and this at an early period, however important it may be, no effort of the kind should be made, till the soft parts are in a proper condition, and all the other requisites for its safe accomplishment are present.

In all cases in which the soft parts are well relaxed, and the os uteri fully dilated or dilatable, and the labour has not advanced so far, or the membranes been ruptured so long, and the uterus so forcibly contracted upon its contents, as to render the operation impracticable, without the exertion of a considerable degree of force, we should proceed to deliver at once by version. Where the membranes are ruptured, immediate action is the more imperatively called for, to avoid the increased difficulty to the operation, which would be occasioned by the complete escape of the liquor amnii, and the rigid contraction of the uterus upon its contents.

Where the membranes are not ruptured at the time the case is first seen—the soft parts being in the favourable condition above described—should we, for any reason, be induced to defer, for a while, the operation, we should, at least, proceed immediately to action, as soon as this occurs. A great advantage is gained, in the facility with which the operation may be performed, by managing so as to operate while the liquor amnii is still retained. Indeed, to secure this advantage to the full extent, it is preferable, where it can be done, to commence the operation before the rupture of the membranes, and to rupture them after the introduction of the hand; as by proceeding thus the fluid will be prevented from escaping, by the presence of the forearm in the vagina and os uteri, during the version of the child. The advantage spoken of has been well illustrated, by comparing the operation thus performed to the movement of a body in a vessel of water. Where the liquor amnii has escaped, the body is more or less firmly embraced by the

uterus itself, which, in such a condition of the parts, clings to and turns with the child, often, as if forming a part of it, with the effect, it must be evident, of rendering the operation much more difficult; while, where it is still retained, the child can be made to revolve with facility; floating, as it were, in a mass of fluid, which prevents, partially, even the contact of the uterine parietes with it.

Where the os uteri is undilated and undilatable, and the soft parts, generally, rigid and unyielding, we should at once resort to such measures as may be calculated to prevent a premature rupture of the membranes, and to secure an early relaxation. Free bleeding once, or more than once, according to the condition of the patient, is one of the best, and the first remedy that should be resorted to. It not only has, in itself, a considerable tendency to relax, but it is a good preparative for the favourable operation of other remedies, and places the system in a favourable condition to prevent bad results from a protracted labour, or the violence necessarily incident to the operation. Doctor Dewees has warmly recommended venesection *ad deliquium*—the patient, with this view, being placed in the erect position—as a preparative for the operation of version, and advises that it should be performed prior to reaction. This, no doubt, in many cases, would facilitate the operation, but bleeding to syncope does not always insure dilatation of the os uteri; and as this, or, at least, a perfectly relaxed and dilatable condition of this part should be regarded as an essential preparative, we should not, by any means, determine, before-hand, to turn immediately after the bleeding, though in some cases it might be proper to do so. The following very unfavourable case, given by Cazeaux, very well exemplifies the efficacy of the remedy:

“Chez une dame de la rue du Four-Saint-Germain, aupres de laquelle j'avais été appelé en consultation par M. le Docteur Treves, j'eus l'occasion de mettre pour la première fois en pratique le conseil de l'accoucheur Americain. L'enfant se presentait par l'épaule gauche; par suite d'une erreur de diagnostique, le siegle ergoté avait été administré, et l'uterus était tellement resserré sur le tronc de l'enfant, que l'introduction de la main fut complètement impossible. Je fis lever la femme et la fis soutenir par deux aides. La veine fut ouverte, et je laissai couler le sang, jusqu'au moment où la malade se sentit défaillir. Aussitôt elle fut replacée sur son lit, et la version fut fait sans difficulté.”

Opium, or some one of its preparations, immediately after the first bleeding, should be given in a full dose, with the view not only of allaying irritation and favouring relaxation, but also of diminishing the force of the uterine contractions, and thus to lessen the chances of an

early rupture of the membranes, as well as to avoid, to some extent, the further pressure of the child into the pelvic cavity,—as this, also, in proportion to the degree must enhance the difficulty of the operation, when attempted. In many cases, it is true, the first effect of the opium will be, for a short time, to augment the force of the uterine contractions; but its sedative impression, in due time, is felt,—especially when the dose given is sufficiently large, and the pains then are soon diminished in frequency and force. The dose should be, according to circumstances, from about a half, to three-fourths of a grain of morphia, and repeated in the smaller quantity in four or six hours, if necessary. Dr. James Hamilton, of Edinburgh, spoke in the highest terms of the use of opium, in such cases, and advised it in the dose of eighty drops of the tincture.

Doctor Denman tells us that, “should the opium produce the effect for which it was given, it will be in about twenty minutes after its exhibition; when we are to consider the calm, or disposition to sleep, as affording us the most favourable opportunity for turning the child.” According to my own observation, so far as the operation may be influenced by the opium, this is the very worst period that could be chosen for it; for, under the primary stimulant action of the remedy, the uterine contractions will now be found more energetic in many cases, than before its administration. On the contrary, the favourable influence of the opium will not, I think, be fully exerted in less than an hour; and if the circumstances of the case will admit, a still longer period may be allowed with advantage, for its operation.

Some authors object to the use of opium, from an apprehension that hæmorrhage may result, for the want of contraction of the uterus after delivery, that its influence may produce. But this is a mere possible contingency, to be met by appropriate remedies, should it arise, and should have but little weight in deterring from the use of a remedy so important for the relief of an already existing difficulty.

The Tartrate of Antimony and Potassa may occasionally be used with benefit, in slightly nauseating doses,—say from the 16th to the 12th of a grain, every twenty or thirty minutes—for the purpose of inducing relaxation, especially in strong muscular and robust constitutions. It is quite a favourite remedy with some authors for this purpose, but though, as just remarked, it may be used with advantage at times, in conjunction with other remedies, it is really of secondary importance, as compared to several of them.

As a substitute for the Tartar Emetic, in all cases in which it might be appropriate, and more especially where there existed much febrile action, with a dry, tender and irritable condition of the soft parts, I think

the *Veratrum Viride*, introduced to the notice of the profession of late, by Doctor Norwood, of South-Carolina, might be used with advantage. It not only produces great relaxation, but with it the pulse may be reduced, controlled and regulated, with almost unerring certainty.

The warm bath is also a favourite remedy, for the purpose of inducing relaxation of the soft parts, and may generally be used with undoubted benefit; but, when the pains are energetic, the movements and exertions necessary, might so far increase them, as to cause a rupture of the membranes before such an event would be desirable—and in some instances, therefore, at least, it should be regarded as of questionable utility.

The *Balladonna* ointment has, of late years, been warmly recommended by some authors for the purpose of relaxing, or causing dilatation of the *os uteri*. I have never used the remedy, and am consequently unprepared to give an opinion, based upon personal observation; but the analogy upon which it is recommended, it strikes me, is imperfect. Dr. Meigs recommends it, but apparently, with a somewhat different view. "Much reliance," he says, "is to be placed in the power of the *Belladonna* ointment, applied to the *cervix uteri*, in which it often most speedily, induces a complete *local anæsthesia*."

The Tobacco Enema would, no doubt, be a powerful means for inducing relaxation, but it is both inconvenient and dangerous. Blundell, who suggests it, seems, himself, fearful of its administration.

Chloroform, as a preparative, is a remedy not even secondary to blood-letting and opium, and for a twofold purpose. In the first place, its full and decided influence, while aiding, very greatly, in bringing about a relaxed and favourable condition of the soft parts, will unquestionably, in a great majority of cases, have the effect of diminishing greatly the force of the uterine contractions, and thus retard the rupture of the membranes, till the desired condition is attained. For this purpose, however, the *anæsthesia* must be full and complete. An advantage resulting from the use of chloroform, and in a less degree from opium also, is the calming of the mental agitation of the patient, caused by the idea of a dreaded operation; "an agitation," as remarked by Robertson, "that not only magnifies the pain to be endured, but which, by awakening a more exquisite sensibility in the vulva and passage generally, opposes a powerful obstacle to the introduction of the operator's hand."

The period at which the *anæsthesia* should be induced, varies in different cases. Where, after the bleeding, and the administration of a full opiate, the uterine contractions continue frequent and energetic, so as to lead to apprehensions of a rupture of the membranes, while the soft parts still remain rigid and unyielding, the remedy should be adminis-

tered at an early period, and its influence kept up, unless some particular circumstance should require its suspension, till the parts having assumed a favourable condition, the operation may have been completed. Or rather, it might be preferable, in some cases, after the foetus has been turned, to suspend its administration during the expulsion. In other cases, where the uterine contractions are less forcible, the danger of a premature rupture of the membranes less, and the soft parts assuming a favourable state, the chloroform may be appropriately deferred till a short time prior to the operation, and administered then merely as an immediate preparative. Not only are the patient's sufferings diminished, or rendered null thereby, all voluntary and involuntary resistance on her part prevented and the soft parts relaxed, but the force of the uterine contractions, as just remarked, when the anæsthetic influence is full and complete, will be very greatly diminished. All know how greatly these, when at all violent, increase the difficulty of the operation—indeed often rendering its accomplishment impossible, without the exertion of unwarrantable and dangerous force. Even Professor Meigs, with all his prejudices against the use of anæsthetics, gives a hesitating assent to its administration, in cases such as are now under consideration. "If," says he, "there could be a case to render a complete anæsthesia by ether or chloroform a desirable condition for the patient and practitioner, this is the *case par excellence*."

From the representations of others, and without personal experience, one can scarcely form a correct estimate of the difficulties attending the operation of version, in many cases, even when the best has been done, and with fair success, to bring the parts into a favorable condition; much less where our efforts fail; and it is impossible to overrate the importance of the measures recommended for this purpose. When the uterus is acting with but moderate force, of course the difficulties are but slight; but where the organ is in powerful action, "drained of its waters and lashed into fury by a long period of unavailing irritation," as is often the case, even where it might be possible to introduce the hand, all power of exertion with it, or use of it, would be destroyed or lost. So violent is the compression, that the part becomes, as it were, completely paralyzed, so that not only motion but sensation is destroyed.

Where the membranes may have already given way, and the os uteri is contracted, and the vagina and perinæum are unyielding, as above mentioned, we have still to resort to the measures just enumerated, and with but a slight variation in their application. The bleeding, the opium, and the nauseants, are as manifestly indicated; and here, in no case, can there be any doubt as to the propriety of the warm bath. The opera-

tion should be performed as early as, with propriety, it can be done—that we may, if possible, have the benefit of even a portion of the liquor amnii; but it cannot properly be attempted, while the external soft parts are rigid, and the os uteri undilated. As soon as they begin to assume a favourable condition, and the other necessary remedies have been properly applied, the chloroform should be administered, in hopes that the desired state of relaxation may be completed by it, and the operation performed as soon as this takes place. Often, indeed, it must be remembered, the os uteri remains undilated for some time after it has become dilatable, owing to the fact, that the part of the child in advance, is with difficulty pressed down against the opening. Under such circumstances, generally, the circle of the os uteri is thick, soft, and yields readily in any direction, to the finger pressed against it within.

Where the membranes are not only ruptured, but the shoulder of the child pressed deeply and firmly into the pelvic cavity, and the uterine contractions frequent and energetic, in but a few cases is it probable, that we shall be able to turn, without the exertion of a degree of force, that, for the safety of the mother, would not be justifiable.* Neverthe-

* An unjustifiable degree of force—a degree of force that must, without doubt, in some cases, prove fatal to the mother—is employed in the operation of turning, in some instances it is to be feared, even at the present day, though no respectable authority, I believe, could now be found, who would sanction the practice of many of the most authoritative writers of the last century. I quote a few paragraphs in exemplification. The first is from La Motte.

“La matrice étoit si étroitement appliquée sur l'enfant, qu'il n'étoit, possible d'introduire ni mes doigts, ni ma main dans la matrice, pour aller en chercher les pieds; L'épaule fermoit trop exactement le passage, joint à l'extrême grosseur du bras et à l'étroitesse du vagin. Tous ces obstacles, qui me paraissoient comme invincibles, me déterminèrent, après une courte réflexion, à tordre et arracher ce bras; ce que je fis en deux coups de main, ne doutant pas qu'après l'extraction de cette partie, étronçonnée, je n'eusse une entière liberté, à mettre à exécution le dessein que j'avais toujours, d'aller chercher les pieds. Mais quelque liberté, que me put donner cette extraction, je n'en eus pas encore assez pour exécuter mon intention, quoique la malade fut sans douleur, ce qu'étoit encore un grand avantage, tant pour elle que pour moi, car quand je voulais forcer ma main à entrer à côté de ce moignon d'épaule, que je ne pouvais faire retrograder, par les raisons que j'ai dites, je souffrais une si violente douleur, qu'elle étoit suivie d'une impuissance absolue de remuer aucun de mes doigts, à cause que la compression, que toutes les parties en général souffraient causait un étranglement aux nerfs de ma main, qui interceptant le cours des esprits, en sorte que ces parties tombaient dans un engourdissement paralytique, ce qui m'obligea à retirer ma main plusieurs fois, afin qu'en procurant le cours aux esprits je puisse y rendre sa première vigueur, après quoi je retouruais à l'ouvrage, comme auparavant, jusqu'à ce qu'enfin j'eusse forcé ce passage. Alors j'introduisis ma main dans la matrice. * * * * Je crus

less, the remedies enumerated should be resorted to in proper order, and as the particular circumstances of the case may require, and when the soft parts become relaxed, the attempt made. Even should success not attend the attempt to operate, the measures resorted to will serve to improve the general condition of the patient, and prepare her better for delivery by the spontaneous evolution, or by the mutilation of the child.

But it must not be supposed that so severe and rigid a system of treatment, as would be implied by the formidable array of remedies enumerated, will be necessary in all cases, of even pretty firm contraction. Often, indeed, success will attend the judicious use of one or two; and only in the worst cases, will more be required. The known remedies of real utility, have all, I believe, been enumerated, though others have been recommended—some of them sufficiently amusing. Thus Chailly recommends frictions of the abdomen, with oil of sweet almonds and laudanum; and M. Champion advises the injection into the uterine cavity of decoctions of flaxseed and poppy heads, where the organ is so much contracted as to prevent the introduction of the hand and the turning of the child.

Should all our known available means fail to produce a suitable state of relaxation of the soft parts, and things remain, for instance, as in a

très certainement que je mourrais après cet accouchement, où j'épuisai, et ma science et mes forces, et après lequel je restai sans respiration, en sorte qu'il me fallut mettre sur un matelas devant un grand feu, et me frotter avec des linges chauds pendant plus d'une heure." Entertaining such serious apprehensions in regard to himself, what should have been his thoughts in regard to the patient, who after all, must have been the greatest sufferer?

The same author, in noticing another case of turning, remarks:—

"Je crus que cet accouchement serait le dernier de ma vie, tant j'étais las et épuisé, et j'eus besoin de plus de huit jours pour me remettre, de l'extrême fatigue, que j'avais souffert sans que je puisse m'aider pendant tout ce temps-là, des mains ni des bras, ne marchant, même, qu'avec peine."

The next is from Doctor Smellie; who, in speaking of the difficulties encountered in a case of version, remarks:—

"The hand and arm, last introduced, being likewise cramped, I was obliged to withdraw them, and I began to despair of succeeding without the assistance of the crotchet, but I resolved to make one more effort. My left hand being now pretty well recovered from the former fatigue, I introduced it as at first, and at last reached up to the fundus uteri." * * * * * "I was not able to raise my arms to my head for a day or two after this delivery."

Of another case, he says:—

"The weather was remarkably cold for the season of the year—there was very little fire, and yet I sweated so much, that I was obliged to throw off my waistcoat and wig, and put on my night gown, with a thin napkin on my head."

case mentioned by Collins, where "the mouth of the womb was absolutely as firm as a piece of thick leather, and embraced the arm of the child as tightly as a ligature could be applied, without cutting the part," what are we to do? The hope of relief by incision of the os uteri, might probably suggest itself. Smellie, indeed, in some cases of the kind adopted this expedient, but none of his patients recovered. How far the fatal result was attributable to the operation, however, is a doubtful question, for all of his cases must have been of the most dangerous character; and besides, that, as we know, frequently lacerations of the cervix uteri occur during labour, incisions through the part have been practiced in other cases, without any such result. In cases of schirrous engorgement, Madame Boivin recommends, as has been practised in some cases, she says, an incision through one of the borders of the uterine orifice, for the purpose of facilitating the introduction of the hand,

Having failed in the attempt to turn, or abandoned the hope of turning, we may wait a reasonable time, in hopes that the patient may be delivered by the spontaneous evolution; of course, resorting to such measures, as may be proper under the circumstances, to favour this end, The symptoms given, in the words of Doctor Douglass, which should

It would seem miraculous almost, indeed, that any woman should survive, after being made the subject of such violence.

Even by one of the authors of the article "*Version*," in the *Dictionnaire de Médecine*, the following case is given:—

"Les eaux étaient écoulées, et le bras, sorti depuis huit jours; l'utérus était fortement collé sur la surface du corps du fœtus; il repoussait ma main avec tant d'énergie, que je desespérai pendant quelque temps de parvenir à vaincre sa résistance; je ne parvins en effet à introduire la main, qu'en écrasant la poitrine du fœtus, qui était mort depuis quelques jours; mais dès que ma main eut pénétré dans l'utérus, je parvins, quoique avec peine, à saisir les pieds."

Surely there could have been no necessity here for inflicting upon the patient, the pain of introducing, against so much resistance, the hand into the cavity of the uterus, the child having been dead several days.

Were the operation of version the only hope or resource, then, as a dernier resort, such violent measures might possibly be considered justifiable; but even before the discovery of Doctor Denman in 1772, of the possibility of delivery in the shoulder presentation by the natural efforts, the method of delivery by evisceration, was available; and surely, in cases such as have been quoted, there could be but little objection to it, on account of the child, which, it could scarcely be hoped, could survive, where such violent efforts might have to be made for its delivery; while, to the mother, both the pain and the danger, must be greatly diminished by adopting it. After his discovery, Doctor Denman seems to have employed much less force, in attempting the operation of version, than before—trusting that if moderate exertions failed, that spontaneous evolution would take place; and this failing, he assisted by means more consonant with the safety of the mother.

lead us to expect this termination, are the following: "If the arm of the foetus should be almost entirely protruded, with the shoulder pressing on the perinæum; if a considerable portion of the thorax be in the hollow of the sacrum, with the axilla low in the pelvis; if, with this disposition, the uterine efforts be still powerful, and if the thorax be forced sensibly lower during the pressure of each successive pain, the evolution may, with great confidence, be expected." I would add:—where the mother has little or no fever, and when the soft parts are cool, yielding and well lubricated. Other circumstances, also favouring the result in view, are, a pelvis of good size; a foetus of small size, premature, or in a state of partial decomposition; frequent former deliveries of full-sized children, &c. Circumstances of an opposite character, of course, are calculated to render the result doubtful.

By universal consent, ergot is proscribed in cases of shoulder presentation, but an exceptional case, in which it might be proper, might very possibly arise. Where, for instance, on account of the depth to which the thorax of the child has been forced into the pelvis, the idea of delivery, by version, has been abandoned, and the soft parts are well lubricated and dilatable—the child, with each successive pain, approaching or distending the outlet; should the pains then cease, it might not, I think, be improper to administer it. Of course, so long as a reasonable probability of terminating the case by version, remained, nothing could be more improper than the administration of this remedy, for it induces such a rigid and continuous contraction of the uterus upon its contents, as to preclude all hope, subsequently, of the operation, in most cases. Unfortunately, in consultation practice, it will often be found, that, from some mistaken view, or, more probably, from an error of diagnosis, this remedy has been improperly administered.

But how long are we justifiable in waiting for the termination of the case, by the natural efforts? Of this, we must judge mainly by the condition of the mother. When the soft parts, instead of remaining well lubricated, relaxed and cool, become hot, dry, rigid and irritable, and the patient herself is thirsty, feverish and restless; more especially, if, notwithstanding the continuance of energetic uterine contractions, the child—having been forced onward to a given point—ceases to advance, we should certainly wait no longer. The point has unquestionably been reached, at which the mother's condition becomes a matter of serious consideration. The vitality of the child at the time, and the hope of its ultimate safety—which, at best, is but slight indeed, in delivery by spontaneous evolution—alone could justify us in waiting so long. Where the child is known to be dead, it would be proper to decide on

delivery by art, at a much earlier period, and the course to proceed, is, to diminish the size of the child, by removing, partially or wholly, the abdominal and thoracic viscera.

As a preparatory measure, both to the operation of version and evisceration, the bladder should be emptied by the catheter; and where the rectum is loaded with feces, their removal should be effected by the administration of a mild enema; but the latter remedy should not, unnecessarily, be resorted to, because the stimulus to the rectum may be extended to the uterus, and increased uterine action thus induced.

In the presentation of the vertex, it would be improper to resort to the operation of version, after the head has passed the os uteri, because, in the attempt to elevate it above the superior strait, the neck of the organ being contracted on that of the child—a part smaller than the part to be elevated—the uterus, itself, would also be pushed upwards, and there would, therefore, be danger of laceration and its separation from its vaginal attachment. In the shoulder presentation, less apprehension of such a result need be felt, because the part still engaging in the os uteri, would, probably, be larger than any that may have passed it; but still the operation will seldom, indeed, be found practicable, after the os uteri is fully dilated and occupied by the shoulder, pressed low down in the pelvis.

Version. In other cases, the operation of version may be required on account of some accidental complication, endangering the life of the mother or child, such as hæmorrhage, convulsions, &c., even when the presentation and the position are the most favourable; but in the class of cases under consideration, it is urgently called for, on account of the presentation itself.

The operation is called *cephalic* version, when the head of the foetus is brought to the superior strait—an operation, of course, only applicable to transverse presentations, and which has been regarded by some as preferable, and especially adapted to such cases—and *pelvic*, or *podalic* version, when the feet, knees or breech are brought to this part. Each of these operations has had its advocates; but while it would not be proper—as seems to have been done by Madame La Chapelle—to exclude cephalic version entirely, from the recognized and legitimate operations in the shoulder presentation—in which more especially, as just remarked, has its advantages been urged—there can scarcely now remain a doubt; and the profession seems unanimous on the subject, that pelvic version should be preferred, in a vast majority of cases.

From the time of Hippocrates, to that of Celsus, cephalic version seems alone to have been recognized, and an attempt was made to re-

vive it by Flamand and Osiander—and its advantages particularly urged in the shoulder presentation—about the close of the last century.—Though Celsus notices the operation of pelvic version, and recommends it when the child is dead; and we are told by Actius, that Philomenes, in unfavourable presentations, followed the method of turning, by grasping and bringing down the feet—it is to Ambrose Paré, his pupil Guillemeau, Mauriceau and La Motte, that the profession is indebted, for the introduction of this operation into general use, so late as about the close of the sixteenth, and the early part of the seventeenth century.

Cephalic Version. It would be difficult to give precise and definite rules, by which to decide, as to the few exceptional instances in which this operation should be preferred, and much must be left to, and will depend on, the judgment and practical tact of the accoucheur, in each particular case. In deciding the question, we must bear in mind, that every effort to bring the head to the superior strait—should we fail in the attempt, and ultimately have to resort to pelvic version—will render the latter operation much more difficult, both by removing the feet further from the superior strait, and increasing the energy of uterine contractions. This objection, however, is of less force where the effort is made by external manipulation alone, according to the method of Wigand. The difficulty occasioned by the latter cause, will be much greater, of course, where the membranes are ruptured during the operation, or may have been already ruptured, and the indications, therefore, for the operation, ought to be very unequivocal indeed, to justify the attempt.

“To justify the method”—say the authors of the article *Version*, in the *Dictionnaire de Médecine*—“it is necessary that the head should be near the superior strait, the body easily and sufficiently moveable, elevated, and surrounded with a considerable quantity of liquor amnii; that the mobility should not be confined to the parts nearest the outlet, but communicable from the latter to the parts above; that there should not exist any particular circumstance or accident that might require, after the version, the immediate termination of the labour; and that the uterine contractions should be efficient,” &c.

In any case in which the arm is prolapsed, Cazeaux, very properly, I think, rejects this operation. He further remarks:—“Un seul cas nous paraît devoir encourager à insister sur les efforts de version cephalique. C'est celui où une présentation du tronc coïnciderait avec une vice de conformation du bassin. Alors en effet, dut on aller saisir la tête, et la ramener de vive force au détroit supérieur; il vaudrait mieux exposer l'enfant aux dangers de cette manœuvre, la femme à l'irritation qui pourrait en résulter, que de tirer sur l'extrémité pelvienne, s'exposer à

l'extension de la tête, arrêté au-dessus du détroit supérieur après la sortie du tronc, et a toutes les conséquences fâcheuses qui peuvent en résulter." I do not think, however, that he places the propriety of the practice beyond doubt, except when all the other circumstances are favourable to it.

The same author further remarks: "La version céphalique, étant surtout pratiquée dans l'intérêt de l'enfant, la mort ou la non viabilité du fœtus, nous paraissent devoir être une contre-indication formelle à cette opération." This, I conceive, is calculated to inculcate an erroneous practice. I do not think that, in such a case, we should be governed mainly in our decision, by the probable result to the child—because, under the circumstances, the difference in the chances for it, must be very slight indeed—but purely by the facility with which one or the other operation may probably be performed, considering only the safety of the mother. Only where the circumstances are such, excepting in the case just mentioned, of pelvic contraction—as to render it probable that cephalic version may be performed more easily than pelvic version, should it be preferred, where pelvic version may be performed more easily than the other—this facility, even for the child, will fully counterbalance the difference in the fatality as regards it, between the breech and vertex presentations, while the difference as regards the pain inflicted upon, and the danger to, the mother, are allowed their due weight.

Madame La Chapelle, on the other hand, recommends version by the feet, where the pelvis is moderately contracted, even in cases in which the vertex already presents. Against the adoption of such a practice, however, the strongest reasons may be urged. By such a course, without any adequate advantage, the chances for the safety of the child are diminished, in proportion as delivery by the feet is less safe than that by the vertex, without counting the dangers of the operation itself. Of course, too, in coming to a decision, it would be proper to bear in mind, the great difficulty generally met with, where the pelvis is contracted, in the extraction of the head, after the delivery of the body; a difficulty sufficient to justify a preference for cephalic version, at the risk of a subsequent necessity for the forceps, in any case in which it may be performed with equal facility. The same may be said in regard to such cases as might probably require the perforator. In either case, the difficulty would probably be less before, than after, the escape of the body.

The existence of any complication requiring immediate delivery, would be an insuperable objection to cephalic version, even in cases otherwise favourable for it; and even the possibility of such an occurrence in the progress of the case, should be allowed its weight. In such an event,

the patient would be placed in a much more unfavorable condition, for the immediate termination of the case by pelvic version, than in an original vertex presentation. In the latter case, much of the liquor amnii, even after the rupture of the membranes, is retained and surrounds the body, while, in the former, the efforts that have been necessary to bring the head to the superior strait, will have occasioned its complete discharge.

Where the shoulder—though presenting—instead of being at the centre of the superior strait, is found resting rather to the side of the pelvis in which the breech is lodged, while the head itself—as it must be in such a case—is lodged but in part upon the pelvic brim of the opposite side; and the other favourable circumstances enumerated, are present, it is not improbable, that it might be found easier to bring the head into the superior strait, than to turn by the feet; and, it being the mal-position of the child only, for which the operation is required, it might, under such circumstances, with propriety, be urged in preference, on account of the greater probability, that the life of the child may be preserved by it.

Such a case is a favourable one, in which to attempt the correction of the mal-position of the child, by external manipulation, according to the method of Wigand, and, of course, with a better prospect of success, if prior to the escape of the liquor amnii. Chailly considers the presence of what would seem somewhat incompatible conditions, necessary to success, in such an attempt, to wit: that the foetus must possess great freedom of motion, and the uterus contain but little amniotic fluid. In such an attempt—the patient having been previously placed in a suitable position, with care to relax the abdominal muscles—while with one hand applied upon the abdomen, the head is pressed downward, and across from the side at which it rests, towards the centre of the pelvis, with the other, in like manner, applied externally, the breech should be, at the same time, pressed upwards and directed towards the opposite side. The attempts to move the foetus thus, will probably succeed best if made during the intervals between the uterine contractions, though, whatever is gained should be maintained, while the organ is in action, by a continuation of the pressure.

Another method recommended, is that by position. The patient is made to rest on the side to which the head is directed, so that the gravitation of the breech towards the same side may cause the head to glide from the iliac fossa, in which it rests, towards the superior strait. In addition, a firm cushion may be placed under the part of the abdomen where the head is lodged, and pressure made with it, during the uterine

contractions. Of course, the two methods of operating, by external manipulation and by position, may be combined in any one case.

Another method, recommended, and perhaps more generally, of operating, is, while pressing the head towards the superior strait, with one hand applied to the abdomen to elevate the shoulder, with a couple of fingers of the other hand introduced into the vagina, and carry it as much as possible to the side of the pelvis in which rests the breech, instead of employing this hand in pressing the breech upwards, through the abdominal parietes. When the membranes are not ruptured already, in no case should this method of operating be attempted, prior to the complete dilatation of the os uteri, as it is more than probable that the membranes will be ruptured in the attempt; and, should the effort fail, pelvic version will be rendered much more difficult by the escape of the liquor amnii, which must take place, while the requisite dilatation is taking place.

D'Outrepoint, thinking it not probable that the head could be maintained at the superior strait, even if brought there, without the position of the entire body being previously changed, operated by elevating with his hand introduced into the uterus, the presenting part, in the intervals between the pains, till the entire body was moved, so that the head would fall into the superior strait. He directs then, that the hand be brought into the vagina, and with it, the head sustained in its situation during at least the continuance of one pain.

Busch proposed and advocated the method of operating directly upon the head itself, with the hand introduced into the uterus, instead of indirectly through the body. In operating in this manner, that hand should be chosen which may correspond, when introduced, with the side of the pelvis at which the head rests. Thus, if the head rests at the right side of the pelvis, the left hand must be used, and, *vice versa*. Supposing the head to be resting on the left side of the pelvis, as in the first position of either shoulder, the right hand should be introduced, (elevating and pressing the shoulder in the meantime to the right,) and passed up in such a way, if possible, in the left side of the pelvis, that the head may be received in the palm. After this, while with the fingers curved over it, we attempt to bring the head down and across towards the centre of the superior strait, with the other, the breech, as in the operation of version by external manipulation, should be pressed upwards, and towards the opposite side. Busch advises that the hand should be introduced prior to the rupture of the membranes; but the propriety seems questionable, of attempting the operation of cephalic version by the introduction of the hand, till a more exact knowledge of the

position has been ascertained, than can be learned before this has taken place.

Doctor Meigs gives, in his work on Obstetrics, the following case, which may serve to illustrate one method of operating. "Upon placing my hand over the right iliacus muscle, I distinctly felt the orbicular mass of the head of the child, under my palm. Introducing the fingers again, I waited till a pain came on. As soon as the bag of water became tense, from the pain, I pressed with my left hand the head out of the right iliac fossa, towards the chasm of the superior strait. I then ruptured the ovum, and exhorted the woman to bear down. I had the pleasure to find the head driven quite into the excavation, and to find it born after a few minutes."

As a general rule, in these different methods of operating, when the head is brought to the superior strait, it should be secured as well as possible in its position, by pressure through the abdominal parietes, and—where it has not been previously done—the membranes ruptured, so that the uterus may contract, and prevent its deviation. Labour may now go on as in a case of original vertex presentation, and the case must be left to the natural powers, to the same extent. On this account, cephalic version is manifestly improper in any case, in which, in conjunction with the mal-position, any complication may exist—as flooding, for example—requiring immediate delivery, seeing that in it, we have not the desirable control over the child, which the grasp of the feet gives us in pelvic version.

Cases may possibly occur, in which it may be proper to adopt one or the other of these methods of performing cephalic version, or in which several of them may be tried; such instances, however, are rare indeed; for, in a very great majority of cases, it may be repeated, the operation by the feet is the one that should be preferred.

Pelvic Version. In English practice, the obstetrical patient generally lies on the left side, and in this position, with the breech brought to the edge of the bed, the operation of version is performed. In France, and pretty generally in this country, it is preferred that the patient should rest on the back, with the breech brought over the margin of the bed—the coccyx and perinæum entirely free—while her feet are supported by an assistant, or are allowed to rest on a chair on each side, during the performance of the operation. Her shoulders should also be moderately elevated with pillows, and another assistant should be prepared to prevent her from suddenly receding, by pressure applied to the crests of the ilia, or to sustain the uterus, with his hands outspread upon the abdomen during the operation, as may be required. This I consider deci-

dedly the best position, not only as being more convenient, and giving to the obstetrician a more perfect command of the foetus; but, as enabling him to form, at once, a correct idea of the relations of its different points, with the mother. The chair in which the accoucheur is to be seated in front of the patient, should be of a proper height to render his position comfortable—for any thing like discomfort, or an unfavourable position, must interfere, more or less, with the success of the operation—which, even under the most favourable circumstances, is apt to be a fatiguing one. Dewees recommended that the operator should kneel on a pillow in front of the patient; and when the operation is protracted, if not at first adopted, this position may be assumed with relief in its progress. Where, however, the bed on which the patient rests is high, the standing position is, perhaps, the best, as leaving the movements of the operator more free and unrestrained. Various uncouth and inconvenient positions of the patient—as, for example, upon the knees and elbows—which were formerly recommended, have been pretty generally, if not entirely, abandoned. A folded sheet or blanket should be spread over the knees of the operator, to protect his clothes from the fluids that may be discharged.

The patient, of course, at the proper time, should be apprized of the necessity for the operation, and her acquiescence obtained; and so obviously proper does this appear, that although much surprised at the advice of M. P. Dubois and M. Chailly, not even to inform her of the intended operation, I cannot but deem it time misspent, even to discuss the question.

In all instances, where the circumstances of the case will safely permit, authors are agreed in advising a consultation, before such an operation is attempted; but where the propitious moment for its performance may be lost—as, the time immediately after the rupture of the membranes—by waiting, it should not be deferred for such a purpose; otherwise there would probably be cause for regret, that the practitioner lacked moral courage to act upon his own judgment and responsibility.

As the arm, in turning, may have to pass within the soft parts, as far almost as the elbow, it is better to remove the coat, so that no restraint whatever may be offered. Not only should the hand and fore-arm be well lubricated with lard or simple cerate, but, as advised by Roberton,* the vagina itself should be well filled with one of these substances, as a preparatory step. We are advised by authors not to oil the palmar surface of the hand, as, by so doing, the hold is rendered less secure, when the feet are grasped; but, as the body of the child is itself covered with

* Physiology and Diseases of Women, &c.

a substance not less slippery, and as the oiling of the whole hand facilitates its introduction into the uterus, I see no propriety in the advice. French writers, more especially, attach importance to this oiling of the hand, as a protection against infection.

Care should be taken to ascertain the exact position and relations of the child before commencing the operation, that the proper hand may be chosen and the necessity avoided, of withdrawing one and introducing the other, through an improper selection. Besides the additional suffering and danger to the patient, from the re-introduction of the hand, often indeed the operation is rendered more difficult, not merely from the increased action of the irritated uterus, but by the resistance offered at the entrance of the vagina itself. "No one," says Robertson—"who has had frequent occasion to carry his hand into the uterus, can be ignorant of the power of these muscles (the levator ani and sphincter vaginae) in resisting, under certain circumstances, the passage of the hand and arm through the os externum." * * * * * "Should the accoucheur withdraw his hand during the operation, he will find, on attempting to introduce it again, that although he may at first have passed the hand without resistance, these muscles are now irritated to action, and he will generally experience from them a powerful resistance." Where the operation is commenced prior to the evacuation of the liquor amnii, and the firm contraction of the uterus upon the child, even under the disadvantage of having introduced the less suitable hand for the operation, as it may be generally moved about freely in the organ, and as the child may be revolved with facility, we may often succeed without putting the patient to the pain of withdrawal and re-introduction of the hand; but where the liquor amnii is discharged, and the womb embracing firmly the body of the child, so much difficulty is met in the attempt to reach the feet, and to revolve the child, that it will generally be better to withdraw the hand, when the wrong one has at first been chosen, and introduce the other, rather than to make continued efforts to reach the feet, situated at an inconvenient point for the hand in use.

In turning, in all cases except in the presentations of the shoulder, a very general rule of practice—but one that may, with propriety, be deviated from in breech cases—is to use that hand which, placed in a state between supination and pronation, when introduced, will have the palm presented to the anterior surface of the child's body; but authors differ

tion;" and this is a most excellent rule to follow, whichever the shoulder presenting, or wherever the head may rest. He continues: "The rule is to use that hand whose palm, when open in the cavity of the womb, would look towards the face, or breast, or belly of the child—which, in this instance, would be the right hand—for it is clear that if the left were used, it would not apply the palm to the front of the infant, whether it were carried up before or behind the child's body.

I cannot perceive why, in this instance, the palm of the left hand, if carried up in front of the child's body, would not come as fairly upon its anterior surface, as that of the right, though it might not be directed so immediately to the knees or feet as the latter. In the first position of the right shoulder, Professor Meigs prefers the right hand; in the second, it would be as well, he says, "provided the patient were lying on the back, to use the left hand." In the first position of the left shoulder, the left hand, he says, "might be used in the dorsal decubitus, but not so conveniently as the right,"—while, in the second position of the left shoulder, he uses the left hand; for its fingers go out towards the pelvic extremity of the child, and its palm, in easy pronation, adapts itself to its anterior aspect."

Where the anterior aspect of the child is turned towards the pubes of the mother, Madame Boivin, introducing the hand in a state of pronation, prefers to use the right when the feet are to the right, and the left, when the feet are to the left of the uterus; which is contrary to the advice of others, who recommend the right hand, thus introduced, when the feet are in front and to the left, and the left hand, when they are in front, and to the right.

When the patient is resting on the side, as in the English method of operating, it would seem better, generally, to use the hand corresponding with the side on which she rests, as adapting itself better to the axis of the pelvic cavity, straits and uterus, than the other. Thus, when the patient is lying on the left side, which is the more usual position, the left hand would seem preferable, and when the right side, the right hand, Were the practice to be adopted of turning with the patient on the side, in determining the side on which she should rest, it might be a matter of some importance to be guided by the shoulder presenting and its position. Thus, in both instances in which the head is to the left side of the pelvis, that is, in the first position of each shoulder, it would seem better that the patient should rest on the left side; while in the cases in which the head of the child is to the right side of the pelvis, the operation could perhaps be more conveniently performed, with the patient resting on the right side. To be sure, in choosing the right hand, in

the second position of the right shoulder, in which the head is to the right, and the left hand in the first position of the left shoulder, in which the head is to the left, the palm in neither case would be so immediately directed to the abdomen of the child; but the advantage gained by thus following the course of the axis of the pelvic cavity, till the hand has reached the desired depth, would probably more than counterbalance any inconvenience arising from such a cause; while, after its introduction, it might be brought round to the proper situation. It may possibly, however, in some of these cases, where the womb is firmly contracted, be found necessary to deviate from this course, owing to the difficulty that may be experienced in bringing the hand round in front after its introduction, and pass it up directly to the feet, between the pubes and shoulder. The difficulty of reaching the feet in such an attempt may be somewhat diminished by pressing backwards the fundus uteri, so as to diminish the anterior obliquity of the organ.

When, says Robertson, "instead of the left, the right hand is used—the patient being on her left side—the moment it has passed, the flexures of the wrist, and of the palm, are found not to correspond with the axis of the uterus;" for somewhat similar reasons, the left hand is advised by Dr. F. H. Ramsbotham, and other English authorities, supposing always the patient to be lying on the left side. Merriman says: "in cases in which I have preferred using my right hand, I have been obliged to place my patient on her right side." The rule might, however, even with the patient on the side, be adopted without any decided advantage of using the hand corresponding with the shoulder presenting; but, as already remarked, I regard the position on the back as the most favourable for the operation.

Maygrier, having the patient on the back, uses the hand corresponding in name with the side in which the breech is lodged; for instance, in the first position of each shoulder, in which the breech is in the right iliac fossa, he uses the right hand, and the left in the second position of each shoulder, in which the breech is to the left. In the two positions in which the abdomen of the child is directed to the sacrum of the mother, he introduces the hand in a state of supination, and in the two in which the abdomen is directed towards the pubes, he introduces it in pronation.

Cazeaux uses the hand, corresponding in name with the shoulder that presents; in the presentation of the right shoulder, the right hand; in the presentation of the left shoulder, the left hand; and this, as being not only the simplest, but, also, I believe the best rule, is the one that I prefer.

The position of the patient having been arranged, and the hand adapted to the case properly lubricated, the fingers formed into a cone, with the thumb pressed into the palm, must be tenderly—as expressed by Blundell, with the fear of laceration of the womb, held in view—introduced into the vulva, and the hand pressed on, with a gently semi-rotatory motion, at first in the direction of the axis of the inferior strait, till it is lodged in the vagina, with the back presented to the anterior face of the scrotum. Following the direction of the cavity of the pelvis, the superior strait and uterus, in doing which the elbow must be slightly lowered; as the hand is pressed onwards, it must be advanced till the part presenting is reached, which should then be elevated, and carried, if possible, somewhat towards the side of the pelvis where the head rests, and where, as much as possible, it must be maintained by the wrist and forearm as the hand is advanced. This has the effect of bringing the feet nearer the centre of the superior strait. The hand must be advanced through the os uteri, in the most careful and gradual manner, with the precaution directed for its introduction into the vagina, and pressed on to the situation of the feet; being careful, where the membranes are not already ruptured, to preserve them, if possible, entire, till the proper point is reached. It is probably preferable too, as Madame La Chapelle directs, to lacerate them during the relaxation of the uterus, that less of the water may be forced out before the feet are grasped and brought down. Where, however, the hand is passed on between the uterine parietes and the membranes, and the latter only ruptured when the feet are reached, particular care should be taken to pass the hand through the opening made in them, so as to bring it in immediate contact with the foetus; otherwise the membranes, intervening between the hand and the feet, they will not only render the grasp less secure, but impede greatly the movements necessary to be accomplished. When the hand, thus in its advance towards the feet, between the membranes and the uterine parietes, may happen to come in contact with the placenta, the direction may be varied slightly, so as to avoid a separation of the latter; or, if the hand be already pretty well advanced, the membranes may be ruptured at the margin, and the hand passed at once within the sack. In all cases in which the rule is followed of using the hand corresponding with the shoulder presenting, following the curvature of the sacrum, it should be passed up behind the child. When the abdomen of the child is directed to the mother's sacrum, we thus find the feet at once; whereas, were we to pass the hand up between the child and the pubes, it would be brought in contact with the back, where the feet are never found, and it would be very difficult, without withdrawing it, to

carry it round the breech to the front of the child. Even when the abdomen of the child is in front, by carrying the hand up behind the child, it passes in the course of the axis of the cavity with less difficulty, and may be brought round the breech, to the feet in front, changing it in the meantime from a state of supination to pronation, with comparative ease.

Doctor Denman says:—"It is generally better to conduct the hand between the body of the child and the pubes, than between it and the sacrum, because in these presentations the feet lie most commonly towards the abdomen of the mother;" and the same statement is made by Burns and some others. Generally, if a proper examination be made, prior to commencing the operation, the exact situation of the feet may be ascertained; and, if possible, this should always be done, and the hand never introduced with this reliance upon the mere chance of finding the feet in a certain position. Moreover, the assertion, above alluded to, I think is erroneous; for the statistics of the shoulder presentation, so far as I have been able to ascertain, show that the positions in which the feet are lodged in the posterior part of the uterus, are more numerous than those in which they are found in front.

Where the vulvar opening is small, instead of introducing the fingers together, as a cone, it may be better to introduce them separately, pressing back the perinæum, for the admission of others by the first that are passed in. In following such a method, during the introduction of the fingers, the hand rests in a state between pronation and supination, but it must immediately after be brought to the latter position, so that the back may rest against the sacrum, and adopt itself better to the axis of the pelvis.

Antoine Dubois, Moreau, and some others, direct that the hand should be passed into the vagina during the uterine contractions; and there is this advantage in such a course, that the hand once lodged in the vagina, may, as soon as the pain subsides, be passed immediately into the uterus. Capuron chooses this time, "*pendant lequel, ce canal,*" (the vagina)—"*est poussé un peu en bas, et vient, pour ainsi dire, embrasser la main qui se présente.*" It is said, too, that the woman's attention being occupied with the pain attending the contraction, she notices less the inconvenience arising from the introduction of the hand. This may be so in some instances, but with many women the sensibility of the vagina is very much exalted during the time of the pains, so that instead of bearing the introduction of the hand quietly, a mere examination causes them great discomfort, much more than in the intervals; and they often attribute the uterine pains themselves to this cause, or believe

them to be—as in reality they probably are—augmented thereby. In some instances, by an examination, during the contraction of the uterus, all sense or feeling of pain seems to be concentrated to the points that the finger touches. Generally, the intervals between the pains will be found better than the time of the pains, for introducing the hand into the vagina.

But, whether the hand be first introduced into the vagina during the pain, or in the interval, it is only during the latter that it can with propriety be passed into the uterus. Where the os uteri is not dilated, but merely dilatable, the fingers must be brought together, with the thumb in the palm, and passed carefully in; or it may be even necessary to pass them through the circle of the os uteri, one by one, and thus to effect its dilatation. Some advantage may, at times, be gained, by alternately separating the fingers a little, so as to dilate the os uteri, and then approximating them, and pressing the hand onward while they are in the latter state, assisting, at the same time, by a gentle movement of rotation. Where the waters have been long evacuated, the great difficulty will be, not the dilatation of the os uteri—except in cases in which it happens to be preternaturally rigid—but the passage of the hand to the feet, between the uterine parietes and the body of the child. *In the intervals* between the pains, the hand must be advanced till the feet are found, and all effort must cease, and the hand be allowed to remain passive, for fear of injuring the uterus, or even, perhaps, causing laceration during the contractions. As the hand in the uterus is advanced, the fundus uteri should be supported externally by an assistant; or, what is better, by the other hand of the operator, as in the latter case the two hands can be made to act more consentaneously. Where it can be conveniently done, we may follow the formal directions of authors, to grasp the feet, by passing the thumb round one external malleolus, and the three outer fingers round the other, with the index placed between the ankles; but, as observed by Cazeaux, we cannot always do exactly as we might wish, and it is only necessary to feel assured that they are held securely. The feet being grasped, we must also remember to *bring them down, and turn the child only during the intervals*; but this being accomplished, should it be necessary to complete the delivery, *the extraction should be performed only during the pains*. Otherwise, the uterus being suddenly evacuated while in a relaxed state, hæmorrhage might result; or the chin, separated from the breast, for want of proper pressure upon the head, might lodge upon the margin of the pelvic brim; or even if not, the occipito-mental, instead of the occipito-frontal diameter of the head, be made to correspond with the

oblique diameter of the pelvis, and delay and difficulty thus produced. Besides, very considerable traction might be necessary to deliver the child, while the uterus is relaxed, and comparatively but little during its contractions. While drawing down the breech, by the feet, the turning may be very much facilitated, by pressing the head toward the fundus uteri, with the other hand applied externally to the abdomen.

The appearance or shape of the abdomen after the feet are brought into the vagina, will enable us generally to form a correct opinion as to whether the version is complete, and, consequently, as to whether the traction is yet to be made in the intervals, or during the pains. If the uterus still remain disproportionately large in its transverse diameter, the version is probably not yet complete; but, if otherwise, and its longitudinal diameter has proportionately increased, we may judge that it is. We may also judge by the effect of the pains, which will cause the child to descend, if the version be complete, but will have but little effect, if it should still remain in its transverse position.

When the part grasped has been brought down, should there be much difficulty in completing the operation, it may be facilitated by pressing the shoulder from the superior strait, by a couple of fingers introduced into the vagina, while traction is made on the feet by a fillet previously applied. Dr. Blundell says he has met with resistance in drawing down the feet, "in consequence of the breech becoming seated over the front of the pelvis, above the pubes;" but it is difficult to conceive how any obstruction could be thus produced, at least when both feet are grasped. He directs pressure above the pubes, back toward the promontory of the sacrum, while traction is made on the feet to overcome the difficulty.

The object of turning in the shoulder presentation, is merely the correction of the mal-position, and not immediate delivery, as a general rule. The child being placid in such a position, that its expulsion with safety may be accomplished by the natural powers, it is better to trust the termination of the case to nature, at least in a great measure, rather than attempt immediate delivery by drawing on the legs. We thus avoid the inconveniences and delay just referred to, that might arise from the separation of the chin from the sternum; and, moreover, by the gradual expulsion of the child, the soft parts are better dilated and prepared for the easy escape of the head, after its descent into the cavity of the pelvis—a matter of very great importance as regards the safety of the child, which is endangered by any considerable delay, after pressure on the funis commences, by the head lodged in the pelvis. A very considerable delay prior to this time, is of much less importance than even a

brief period of delay after it. With the view of insuring the preparatory dilatation of the soft parts, for the escape of the head, Dr. Wm. Hunter advised even "to sustain the child at the hips as long as you can, they being next to the head the largest and most unyielding part."

Dr. Dewees says:—"The whole act of turning should be considered as one of necessity rather than of choice; therefore where it is proper to commence with it, it is, we believe, always proper to finish with it, and not to trust the delivery to the powers of nature, after having brought the feet into the vagina, as some recommend." But this injunction holds good only where, for the safety of the mother or child, the immediate removal of the latter is required, as in cases of hæmorrhage or convulsions, and is not applicable to the cases under consideration, which generally may be left to terminate as an original footling presentation.

In all cases in which, without any great degree of extra effort, it is possible to secure both feet, and, more especially, where, from any complication, immediate delivery might be necessary, it is perhaps better to do so, because the command thus gained over the child is much greater than where we have but one. Nevertheless, the advantage is not sufficient to justify us in protracting materially the operation, or increasing the pain or danger to the patient, as the version may generally be very well completed with but one. Indeed there are authors who advocate the method of turning in such cases by one foot alone in preference, and there are certain advantages, no doubt, attending this method of proceeding. Heister was satisfied to turn with one foot, and Dr. Merriman says, "as it is sometimes difficult to lay hold of both feet, we must, therefore, be content with one." Burns does not deem it necessary to bring down both, and Chailly, who may be regarded as also representing the views of M. P. Dubois, directs that whenever the hand has reached the inferior extremities, whatever is met with, whether a knee or a foot, should be brought down, and he seems to regard it almost as a matter of indifference, whether one foot, or knee, or both, or which knee or foot. Dr. Collins, too, of Dublin, and Dr. Simpson, of Edinburgh, both, as a general rule, deem it sufficient to bring down but one foot, as the operation may be thus very well accomplished; but Dr. Radford, of Manchester, England, urges the practice on the following very plausible grounds:

"The results of practice," he remarks, "prove what might be inferred by reasoning, that the child's life is much more frequently preserved, in those cases in which it presents the breech, than where the feet come down first." * * * "Is there then no practice which would enable

us to bring down a part approximating in its measurements to those of the breech presentation, which we have already stated to be so safe to the child, but which cannot be effected in turning operations! There is, and this practice consists in never bringing down more than one foot, in the manual operation of turning the child."

Drs. McClintock and Hardy, authors of "Practical Observations on Midwifery," ascribe to this method of operating "the low mortality which appears among the children, in our version cases," in the Dublin Lying-in Hospital; and influenced by somewhat similar views, Dr. Hunter advocated, at one time, the practice of bringing the breech to the superior strait. "Reduce it," he says, "to a perfect breech case, that it may come more gradually, on account of the head and navel string, least you strangle the child." But to this course there is not only a strong objection arising from the difficulty of this operation, but also from the fact that even if we succeed in bringing the breech to the superior strait, we do not retain the control over the child, through which, in case of necessity, we are able to terminate the labour at pleasure, as we may, when even but one of the feet is brought down; somewhat less promptly and surely to be sure, than where we have both in hand.

The following measurements of children, at the full period of utero-gestation, are taken from Churchill.

The circumference of that portion of the head, which presents in labour, is from twelve to thirteen and a quarter inches.

Do. of the breech, with the thighs flexed upon the abdomen, as in breech presentations, from twelve to thirteen and a half inches.

Do. of the breech, with one thigh turned upwards, towards the abdomen, the other extended, from eleven to twelve and a half inches.

Do. of the hips, the legs extended, as in feet presentation, from ten to eleven and a half inches.

Thus it is apparent, that in drawing down but one foot, where, from choice or necessity, the other may be left with the thigh flexed upon the abdomen, to compensate for any difficulties or inconveniences that the seizure of but the one may entail, we have a better prospect of a living child, in the greater dilatation of the soft parts thus effected, preparatory to the escape of the head, upon the easy exit of which, by avoiding pressure upon the funis, safety so much depends.

Cazeaux advises, when the foot thus brought down alone, is the one in front, or corresponding with the pubes, to terminate the labour without searching for the other; but where it is the one corresponding with the perinæum, having secured it with a fillet, to introduce the hand, and search for the other,—and, seemingly, for the following reasons. "En

tirant spécialement, sur le pied antérieur on facilite la rotation du plan antérieur du fœtus vers les lombes de la mère, et on repousse, plus facilement les parties en arrière, c'est à dire qu' on se rapproche autant que possible de la direction de l'axe du détroit supérieur, que les parties ont à franchir." Here, I believe, the advice should only bear on such cases as may demand immediate delivery; for, in other cases, having brought one foot down, in the shoulder presentation, whichever it may be, it will be unnecessary to return the hand to search for the other, for, by retaining the one at the outlet, and, perhaps, exerting gentle traction upon it, the effect must be, to throw the shoulder out of the way, and to bring the breech in its place; and this, in reality, is all, generally, that is desired. We may trust the rest, mainly, to the efforts of nature.

To be sure, should we—influenced by the reasoning of Doctor Radford—decide, in any case, to bring down but one foot, it would not be a matter of indifference—setting aside convenience merely—which should be selected. It should be an object, when but one foot is brought down, to select that one which, when the back of the child is directed forward, will allow it to retain this position; but when the abdomen of the child is in front, to select that one which, on being drawn on, will tend to cause the fœtus to revolve on its longer axis, so as to throw the occiput forward and the face in the hollow of the sacrum, as the head descends into the pelvis, with the view of facilitating its escape. In like manner, too, when both feet are brought down, the traction should be principally exerted on one or the other, according to the presentation and position, with the view of effecting the same object. For instance, should we, in the first position of the right shoulder presentation, grasp and bring down the left foot—which, too, would probably be somewhat more remote from the outlet, than the right—the infant would be made to revolve in such a way, that the face would be brought forward, instead of the back; a circumstance to be, if possible, avoided, as being calculated to render the escape of the head so much more tedious and difficult; whereas, by bringing down the right foot, or making traction principally on it, the head would be brought into the pelvis, still retaining its primitive relation with the mother. Again, in the second position of the left shoulder, in which, as in the first of the right, the face of the child is directed towards the back of the mother, were we to draw down the right foot, instead of the left, the child would be made to revolve, so as to throw the face forward, which here, again, should be avoided; while, by drawing down the left, or drawing principally upon it, no rotation is produced, and the proper relations are preserved. But, in the first position of the left shoulder, and in the second of the right—in both of

which the face of the child is primitively directed towards the pubes of the mother—it should be an object, in turning, to throw the face to the sacrum, and bring the occiput forward. With this view, in the former, we grasp and draw upon the right foot, or principally upon it; and in the latter, we bring down the left foot, or draw principally upon it, as the case may be; in both instances, with the effect of causing the child to revolve—or producing a tendency at least to such a movement—so as to throw the face into the hollow of the sacrum, when the head descends into the pelvic cavity.

A simple rule to be guided by is this: In the two positions, in which the face of the child is, already, directed towards the sacrum of the mother—to wit: the first of the right shoulder, and the second of the left—we do not wish to cause the child to revolve, therefore, we bring directly down, or draw principally upon, as the case may be, the foot nearest the outlet, or the one corresponding with the shoulder presenting. On the other hand, in the two positions in which the occiput of the child is directed towards the sacrum, we do wish to make the child revolve, while turning, so that its face may be brought to this point, therefore, we pass the foot nearest the outlet, and grasp or draw mainly upon the one nearest the fundus uteri—the one that does not belong to the same side, as the presenting shoulder.

The propriety of certain directions, given by Madame Boivin, in regard to this part of the operation, cannot, I think, be regarded otherwise than as questionable. Thus, in turning, in the first position of the right shoulder, she says: “On fait des tractions plus fortes sur le pied gauche, pour déterminer la rotation de la partie inférieure du tronc”—and in the second position of the left, she directs: “tirer pendant quelques instants, sur l’extrémité droite, pour engager la hanche de ce côté à descendre la première au devant de la symphyse sacro-iliac gauche, comme dans la première position des pieds.” The tendency of such a method of proceeding, it seems to me would be, of necessity almost, to rotate the fœtus, so as to direct the face towards the pubes.

When but one foot is brought down, of course more caution is necessary in drawing upon it than where we have command of both, for fear of injuring, perhaps even of dislocating, the thigh; and more especially must care be observed in cases in which the leg belonging to the side presenting, is the one brought down; for here, in drawing down, the thigh is abducted, a dislocation is much more easily produced than when the

It may not be practicable, in all cases, to pursue the course above recommended, as to the foot to be brought down in preference, or to be principally drawn upon; and even when it is so, we may possibly fail to place the fetus in the most favourable position relative to the mother, for the ready escape of the head, to wit: with the face towards the hollow of the sacrum; and where such a result is not obtained, in some cases it may be well to impress a proper movement of rotation, by acting upon the legs or thighs after they have been withdrawn; a very moderate force, however, only should be used, otherwise the neck of the child might be dislocated, owing to the head being held by the uterus, contracted firmly upon it, so that it cannot revolve with the body. Such an accident would be more likely to occur where the liquor amnii has been long discharged, while the rotation could, of course, be effected with greater facility in cases in which a portion of this fluid might yet be retained.

Another accident that may possibly happen, calculated to retard delivery from this rotation of the child, is the decussation of the neck by the arm, owing to the arm not revolving with the body. M. Dugès speaks of two forms of this accident. When the body is revolved, if the arm be pressed down by the side, and fails to revolve with the body, it will first cross the back, and then, not descending with the body, as it is drawn down, naturally, of course, lodges under the occiput as the head engages in the pelvic cavity. This form of the accident is recognized by the inferior angle of the scapula being found very near the spine. The second form occurs where the arm at the time of the rotation of the body is raised by the side of the head, from which it is pressed down behind the occiput. Such an accident as this, though described by Dugès, must, from its mechanism, be very rare indeed. When it occurs, the inferior angle of the scapula is found at a greater distance than natural from the spine. In the first form, the arm must be brought down over the back before the head can well be delivered; in the second, it has to be pushed up over the head, according to Dugès, and then brought down over the face. But where such an accident is likely to happen, it would be better to desist from any attempt to rotate the body; allowing the head to descend into the pelvic cavity with the face to the pubes, and then, if necessary, to adopt the manœuvre of Madame La Chapelle which will hereafter be spoken of more particularly, for bringing it into the hollow of the sacrum, as less difficult than the management of the arm in its mal position. Still I think, that traction on the child's body, when the uterus is not in action, has more to do in causing the decussation than the rotation of the body.

Velpeau gives what appears a somewhat singular piece of advice in regard to those cases in which the back of the child is primitively in front—the first position of the right shoulder, and the second of the left, to wit—with the view of preventing the back from being directed towards the sacrum of the mother, *after* the version, to change the position of the child, so as to give it this direction *prior* to the operation. Now, where the operation is performed according to the method preferred, in the present paper, it is not probable that where the back looks towards the pubes of the mother prior to the turning, it will be directed towards the sacrum after it; but even admitting such to be a common result, by adopting the course advised by Velpeau, we voluntarily undertake a difficulty greater than any that may probably arise from its omission. Can any difficulty that might result from the transit of the head through the pelvis, with the face to the pubes instead of the sacrum be for a moment compared to such as would probably be met in attempting, completely, as it were, to reverse the position of the child, still in the uterine cavity; and this, too, as a preliminary step merely to turning? Here is his proceeding, as given by Cayeaux, from whom I quote:

“ Dans la seconde position de l'épaule gauche, il cherche à convertir en première de l'épaule gauche, en faisant passer la tête au-dessus des pubis si elle était d'abord plus rapprochée de l'arc antérieure du bassin; et au-dessus de l'angle sacro-vertébral, si elle était d'abord plus près de la symphyse sacro-iliaque droite.”

Unquestionably it would be better to encounter all the risks and difficulties of the transit of the head, with the face to the pubes, than to attempt such an operation. But even where the head has descended into the pelvis in this unfavourable position, the face may be rotated into the hollow of the sacrum, in many cases, by pressure exerted with a couple of fingers of each hand on one side of the forehead, and on the opposite side of the occiput; or by adopting the method suggested by Madame La Chapelle, which is accomplished by introducing the hand behind the occiput, and passing the fingers round the opposite cheek to the mouth, into which one may be inserted, and the face turned into the hollow of the sacrum. Where the face is directed to the right of the pubes, the right hand should be preferred, and the left when it is directed to the left. Either may be used indifferently, when it looks directly forward. Care, of course, must be observed to move the body, with the rotation of the head.

It is true, that in the vertex presentation, such a change as would, prior to the operation, direct the back of the child towards the spine of the mother, in those cases in which it is at first directed towards the

pubes, might be, without difficulty, accomplished, and would have the favourable effect of causing the face to fall into the hollow of the sacrum, in the transit of the head through the pelvis, after the operation; but it would be a matter of great difficulty, and certainly, as shown, not attended with a similar result in the best mode of version in the shoulder presentation. Moreover, even in the vertex presentation, the change could, in all probability, be effected, more readily and with less pain to the mother, after the feet have been brought down, than before.

In turning with both feet, we should at least bear in mind the possibility of the case being one of twins, and guard against bringing down a foot of each.

It has been recommended by Burton, Breen, Delpech and others, in turning, to grasp and turn by the knees in preference to the feet, for the reason, principally, that to reach the latter, the hand has to traverse a greater extent of the uterus than it has to reach the former; but this would only apply in ordinary cases of version, and not to the operation as performed in the shoulder presentation; for generally in the latter, to reach the feet, the hand has to be advanced to a less extent than would be necessary to reach the knees; except, indeed, in cases in which the legs, instead of being flexed on the thighs, are extended along the abdomen. Where the knees might happen to be reached first, on the introduction of the hand, of course they might, without impropriety, be grasped and brought down rather than to extend the search for the feet.

The consequences of mistaking a hand for a foot, and drawing upon it in an attempt to turn, it is manifest would be very serious; hence the injunction, repeated by every writer, to be well assured by a careful examination of the limb, after it is within the hand, of its character.

Cazeaux, in one instance in which he was unable to reach the feet, but could, the breech, brought down the latter, by means of the index finger, curved as a hook, introduced into the rectum; while, with the other fingers, he pressed upon the hips.

Where the arm is prolapsed, it is advisable, in all cases, to secure it before turning, with a fillet, so that as it rises, with the head, as the feet are brought down, we may retain our control of it, and be able to keep it down against the side, and impress upon it any rotation we may deem it necessary to give the body. Otherwise it might embarrass the delivery of the head, by becoming crossed, as it were, under the occiput or chin. Moreau gives the particulars of a case in which this precaution, not only having been neglected, but the arm forced into the uterus prior to turning—after the child was extracted as far as the axillæ—great resistance to its further removal was met with; and he observes, “Nous

reconnûmes que le bras avait été refoulé non sur le plan antérieur, mais sur le plan postérieur, du fœtus ; qu'en remontant dans le bassin la partie supérieure de l'humerus s'était fracturée, et qu'un des fragmens, de l'os, ayant percé les muscles et la peau s'opposait à la sortie du membre."

In the operation of version, in the vertex presentation, it is important, that in bringing down the feet, the body of the child should be flexed upon its anterior surface ; for not only would the operation be rendered much more difficult and more painful to the mother by drawing the feet down along the back, but much more dangerous to the child, from probable injury or dislocation of the spine ; but, in the shoulder presentation, as in the method directed, the feet are brought down laterally, there is but little risk of falling into such an error. In the plates of Heister, given to illustrate the operation of version in the vertex presentation, the feet are represented as being so grasped, that they must of necessity be brought down in this unfavourable manner.

The preceding remarks on the operation of version being of a somewhat general character, it may be proper to speak, briefly, of the particular method of proceeding in the different positions of the shoulders.

In the first position of the right shoulder presentation, the right hand in preference should be introduced, and the shoulder elevated and carried towards the left iliac fossa, and also pressed slightly forward over the pubes. The hand then directed rather towards the right sacro-iliac synchondrosis, will reach the feet, which may be seized and brought down in the manner directed, drawing principally upon the right leg. The head generally, I think, will escape, as in the first or left sacro-acetabular position of the breech. Something, however, in regard to this point, will depend on the extent to which labour may have advanced prior to the operation ; for should the movement of rotation, which throws the breech round to the sacro-iliac synchondrosis, have been completed, the head may escape as in the second position of the breech ; and similar variations, owing to the same circumstance, may occur in the manner of the escape of the head, in the other positions. Authors (Maygrier, for instance) tell us, that in the first position of the right shoulder, and in the first position of the left, after version, the labour is terminated as in the second of the breech, while in the second position of each it is terminated as in the first of the breech.

In the second position of the right shoulder, in which the face of the child is directed towards the pubes of the mother, the right hand should again be preferred. In passing it into the uterus, the shoulder should be elevated and carried towards the right iliac fossa. This brings the

feet nearer the centre of the superior strait. The hand should now be directed towards the left iliac fossa, passed round the hips, brought forward to where the feet are—changing it from supination to pronation—the feet grasped and brought down, drawing principally upon the left or upper foot—the face being forward—with the view of throwing the face of the child towards the sacrum of the mother. In that event, the head will probably again escape, as in the first position of the breech.

In the first position of the left shoulder—in which the face of the child is directed towards the pubes—with the *left* hand introduced, the shoulder should be elevated and carried somewhat to the left, so as to bring the feet nearer the centre of the superior strait. The hand should now be glided towards the right iliac fossa, along the back of the child, and around the breech—changing from supination to pronation—to where the feet are to be found in front, and the feet grasped and withdrawn, exerting the principal traction upon the right—the one most remote from the outlet—with the view of causing the child so to revolve as to throw the face towards the sacrum. The head generally escapes, as in the second or right sacro-acetabular position of the breech.

In the second position of the left shoulder, in which the face of the child looks towards the sacrum of the mother, with the left hand introduced, the shoulder should be elevated, pressed forward over the pubes and carried towards the right iliac fossa. The feet thus being brought somewhat nearer the centre of the superior strait, may be grasped by the hand—carried for that purpose above the left sacro-iliac synchondrosis—and brought down, by drawing principally upon the left or lower foot, so as not to cause rotation or any alteration of the relations of the front of the child with the mother, as its face is already directed towards the sacrum. The head generally escapes as in the second position of the breech; though where the operation is not performed till after the movement of rotation has taken place, which throws the head round over the pubes, it will probably escape as in the first position of the breech.

M. Cazeaux, whose method of proceeding, in the several positions of the shoulder presentation, is very much the same as that given above, says, in regard to it: “Nous devons convenir cependant, que dans le cas où le plan dorsal du fœtus est tourné en avant, elle expose après l'évolution du fœtus, à le diriger en arrière,” with, of course, all the consequent inconveniences. This is an admission, however, which, it seems to me, facts do not require, if the directions, which he himself gives, are followed, in the operation: to wit, in introducing the hand, to carry the

shoulder as much as possible to the side at which rests the head, then to seize the feet, and, instead of causing the child to revolve anteriorly upon itself, as in version in the vertex presentation, to bring them down directly into the vagina; and more especially, if proper care is observed, to draw principally upon one or the other foot, according to the directions already given. "Il n'est pas nécessaire," he says, "comme dans les positions du sommet, et de la face, de fléchir le fœtus dans le sens de la flexion naturelle, mais on peut immédiatement tirer sur les pieds et les trainer dans l'excavation." To be sure, should we cause the child to revolve upon itself anteriorly, according to the method of some accoucheurs, (a thing somewhat difficult to accomplish, and indeed, I think, impossible, without pursuing a course different from that directed by Cazeaux himself,) by carrying the shoulder, in its elevation, towards the iliac fossa in which rests the breech, instead of carrying it to the opposite side—thus bringing the head itself at first nearer and then throwing it entirely across the superior strait, while the breech is first elevated in the uterus, and then the feet grasped and brought down—the back of the child, though directed towards the pubes prior to turning, may be directed towards the sacrum after the operation; in which case, the first position of the right shoulder will probably be changed into the fourth of the breech, and the second position of the left shoulder to the third of the breech.

As this is the method of version recommended and practiced by many accoucheurs, it is right, perhaps, that more direct reference should be made to it, notwithstanding that I deem it of much more difficult performance, and can conceive of no circumstances under which it should be preferred. Here is the description of the operation, briefly given, by Janin, who adopts it. In the first position of the right shoulder, "le côté droit du fœtus étant en bas, il faut introduire la main droite, refouler à droite de la mère, et pelotonner le fœtus sur sa face antérieure, de manière à ramener ses extrémités abdominales du côté gauche du bassin. Cela fait, on parcourt tout le côté droit du fœtus, et on arrive bien tôt sur les pieds, qu'on degage, l'un après l'autre. On ramène ensuite en première des pieds et on termine comme dans cette position." Moreau also adopts this method of operating, and in his fifty-third plate an excellent illustration of it is given in the second position of the left shoulder presentation. The head, it will be seen, is thrown from the right iliac fossa, across the superior strait to the left, and then ascends to the fundus uteri, where the breech had first to be thrown, while the feet are brought down.

There is some difference among authors, as to the facility with which

the operation of version may be performed, according as the abdomen of the child, and therefore the feet, may be directed towards the sacrum or pubes. Professor Miller, of Louisville, for instance, says it may be performed more easily when the abdomen of the child corresponds with the pubes, and the feet are therefore contained in the front of the uterus, than where the abdomen of the child being directed towards the sacrum of the mother, they are contained in the posterior part of the uterus. I cannot understand, however, why or how this should be the case. Where the feet are in the rear of the uterus, the hand, on being introduced in a state of supination, adapts itself to the curvature of the sacrum; whereas, where the feet are in front, the hand has either to be introduced, at first in a state of pronation, or to be brought to this state, in which it does not so well adapt itself to the curvature of the sacrum or axes of the pelvis, with additional effort after its introduction. Moreover—notwithstanding the remark of Cazeaux, but recently quoted—in the positions in which the feet are in the rear of the uterus, where, in the operation, they are brought down directly, the head passes naturally through the pelvis, with the face in the hollow of the sacrum; whereas, in those cases in which the feet are in the front of the uterus, more care is necessary, in bringing them down, to rotate the abdomen towards the sacrum, otherwise this has to be accomplished after the feet are brought down, or other difficulties encountered in the delivery.

Moreau, too, says that the operation is more easily performed in the cases in which the abdomen of the child is in front; because, says he, there is no necessity for impressing a movement of rotation on the trunk; which is in reality the case, according to his method of operating, as already explained. Maygrier, on the other hand, who operates by bringing the feet directly down, considers the operation more difficult, in those cases in which the feet are in front, than in those in which they are in the back part of the uterus. I do not feel entirely satisfied that I understand which of the two methods in question is adopted by Professor Miller; if either strictly.

In the further management of the case, after the delivery of the body, such assistance as might be necessary to facilitate the escape of the arm and the exit of the head, in the several presentations of the breech, or after turning in other cases, will be proper in the shoulder presentation.

Where the case has advanced so far as to render an attempt at ver-

alive, at least, and the condition of the patient will warrant it, such an attempt should be made, before deciding on the mutilation of the child. With the hand passed up and lodged in the hollow of the sacrum, so that the fingers may be curved round the breech, we may draw down on this part during the contractions; or perhaps we may be able to operate to better advantage with a couple of fingers curved in the duplication of the trunk. The blunt hook also might be used, but not with safety to the child without great caution. Doctors Robert Lee, Peu, Monteggia and Chailly recommend the above plan of proceeding, as offering the only chance, under such circumstances, of safety to the infant.

Evisceration. The operation of version being found impracticable in any case, or improper, and the spontaneous evolution failing to take place, prior to the development of symptoms calculated to lead to apprehensions for the safety of the mother, our only alternative is the mutilation of the fœtus; and the preferable method is, I think, to diminish its volume by the removal of the viscera, so as to allow of its escape under the expulsive action, or its withdrawal by the aid of the finger, the crotchet, or the blunt hook, as recommended, first by Celsus, and somewhat recently by Dr. Clark. It is true, that by a few French practitioners, the Caesarean operation has been proposed under such circumstances, when the child is alive; but the rule among British and American practitioners, that when necessary the child should be sacrificed to the safety of the mother, should always govern us in cases like the present; and only, as in any other presentation, where such deformity of the pelvis exists, as may render the removal even of a mutilated child, with safety to the mother, improbable or impossible, should we decide on the adoption of so formidable a measure.

In commencing the operation of evisceration, an opening must, in the first place, be made in the most attainable intercostal space, with some perforating instrument—(and Smellie's scissors is, perhaps, the best)—so that one or more fingers may be passed into the thoracic cavity, and the heart, lungs, and thymus gland torn forth; this may, perhaps, suffice to reduce the volume of the fœtus sufficiently to allow of its deli-

by pulling upon the arm, or the upper part of the chest, as the head resting over the pubes cannot descend. Our efforts will be the more successful the nearer they are made to the breech—the only movable part. We may often succeed with the finger alone, introduced through the opening by which the viscera have been removed, and directed towards the breech; and as this is the safest instrument, it should have the preference. Failing with the finger, the crotchet may be used, hooking it upon the sacrum, or upon one of the ilia, or the blunt hook may be passed round the small of the back and the breech drawn down. Care, of course, must be observed, in using the iron instrument with one hand, properly to protect the soft parts of the mother with the other. Peu suggested the passing of a fillet over the body of the child by means of a perforated blunt hook, for the purpose of drawing down the breech; and, once applied, no doubt the traction might be more safely made with it, though the case to which it would seem more peculiarly appropriate—if applicable at all—would be one in which the infant being still alive, the spontaneous evolution was progressing tardily.

Decapitation. Celsus recommended, in some cases, the separation of the head from the body, and the removal first of the body and then the head; and some recent authors, Dubois, Chailly, Collins and Cazeaux, for instance, seem to favour the practice in preference to evisceration. Such an operation, it appears to me, though it may diminish somewhat the existing difficulty, must be attended with others that are avoided in the operation of evisceration; and there are but few cases indeed, in which any good reason can be given for preferring it; for generally where it is practicable, the other operation, safer and more certain, may be done as well. Only in cases where the neck alone is the accessible point, with no part of the chest within reach of the perforator, it appears to me, could there even be a question as to the preference of evisceration. We once, says Dr. Collins, "saw a delivery effected, as above described, and the greatest difficulty was experienced in the extraction of the head. It was necessary to introduce the hand to bring it into the vagina, and then it had to be lessened before it could be removed." Often the forceps, or crotchet, would have to be employed. In the operation of evisceration, we still retain through the extremities a control over the head.

Where the arm is prolapsed, less difficulty will be met in the delivery after decapitation than where it is not, as through it we have, at least, some control over the body. The description of the operation, given by Cazeaux, shows it to be more difficult and painful than the one for which

he prefers it. "Il s'assure d'abord du lieu qu'occupe le cou de l'enfant; puis, une main, (la gauche quand la tête est à droit, et la droite quand elle est à gauche,) *est introduite tout entière* dans l'utérus, et, l'indicateur s'appliquant en forme de crotchet sur la région cervicale, s'efforce de la tirer en bas pour la rendre plus facilement accessible. Lorsque le doigt ne suffit pas, on peut se servir avec avantage d'un crochet moussé, avec lequel on s'efforce de remplir le même office. L'autre main saisit de très longues ciseaux modérément courbés sur leur plat, à lames épaisses et bien tranchantes, et les fait glisser sur la face palmaire de la main, introduite jusque sur le cou de l'enfant. Là, on écarte légèrement les lames; on cherche à engager une petite partie du cou entre elles, et par des petites incisions répétées, on divise successivement toute l'épaisseur du cou. Quand la décollation est achevée, on tire sur le bras, qui pend ordinairement dans le vagin. On extrait de trouc assez facilement, et on va à la recherche de la tête."

The method of operating with the instrument of the elder Ramsbotham—a hook resembling in shape the common blunt hook, with a cutting edge in its curvature—is, I think, preferable to the above. In operating with this instrument, the neck is first brought down as low as possible, in the pelvis, by the aid of a blunt hook, so as to facilitate the passage of the decapitator over it. The blunt hook may then be withdrawn, and a sawing motion, by moving the handle backward and forward, being given to the cutting hook, while moderate traction is also made on it, the decapitation is soon effected. The soft parts of the mother must be protected, by keeping a finger of the left hand in contact with the end of the hook, during the time the sawing motion is continued.

The operation recommended by Davis, of dividing the body through the vertebral column, offers no advantages over decapitation, and would seem to be entirely unnecessary, for when such an operation would be practicable, evisceration could be easily performed, and perfect control over the superior part of the body retained; which, as with the head in decapitation, would be lost by dividing the body through the middle.

In commencing the preceding essay, it was the intention of the author to speak merely, or principally, at least, of a few points connected with the *mechanism* of the process of parturition in the shoulder presentation; but, under the influence of the "*cacœthes scribendi*," being once begun, it has been extended to what may be deemed, perhaps, an unnecessary length.

ART. II.—M. CL. BERNARD, on the *Physiology of Absorption, and of Respiration*. Being a sequel to the report of his *Lectures*, prepared by the Junior Editor, Dr. F. PEYRE PORCHER, and published in the July number of this Journal. By WALTER F. ATLEE, M.D., of Pennsylvania.

PARIS, September 29th, 1853.

THE physical or chemical change produced in the blood by respiration, has been already considered. Independently of that, the animal heat, the secretions, and nervous irritability, are all kept up by respiration.

Between natural and artificial respiration, there is an immense difference. The chemical changes produced in the blood are the same. Le Gallois experimented, and concluded that life could be maintained by the aid of artificial respiration. An animal was placed under a glass, with a tube, in order to blow into the trachea, and the air contained was found to be changed as by natural respiration. But Brodie remarked that the animal became cold, and colder more rapidly, than if respiration had not been kept up. He concluded, from this, that it was not the respiration that maintained animal heat, but that, on the contrary, respiration cooled. And this is a fact. Magendie and Bernard have always found the blood in the left ventricle colder than in the right, and this is natural, for the air being colder than the animal, the blood must be cooled. This is the cause of the more rapid loss of temperature in the insufflated animal.

Moreover, the secretions are not kept up, and the irritability of the muscles, which we have shown to be altogether independent of the nerves, diminishes and disappears. And all this while the blood is becoming red, giving off carbonic acid and absorbing oxygen. The difference is owing to the nervous system. This had already been remarked by Brodie. And it is not because the brain is taken away, for, as is readily seen, there is still a connection with it by means of the pneumogastriacs, but because the spinal marrow is removed. In an organ that functions, there are always two things needed: excitation from external objects, and, also, an excitation received from the nerves. This nervous excitation wanting, heat, irritability, &c. will also be wanting—every time that the medulla oblongata is cut at the level of the pneumogastriacs, life is at once arrested. Fleurens called this spot, limited below by the origin of the pneumogastriacs, and above by that of the auditory nerves, the *nerf vital*. If you cut above this, respiratory efforts are made with the mouth. If, in

place of decapitating the animal, they had destroyed life in any other way, Brodie and Fleurens would have found the functions spoken of, to continue. When you kill an animal by poison and at once insufflate, things are found to be altogether different; or if you produce asphyxia, provided the heart still beats, you can reproduce the phenomena of life, if the medulla oblongata be intact. M. Bernard performed the experiment upon a dog—destroying life by poison—and then practising insufflation before the movements of the heart had ceased. The secretions of uterine, gastric juice, &c. were accomplished. The cornea was sensitive, and the calorification did not diminish. Again, by the augmentation or the diminution of the insufflation, there was a corresponding change in the secretions. Among the rest, the secretion of sugar, by the liver, was very much augmented, and the animal became diabetic, sugar making its appearance in the urine.

The first condition of respiration is the charging of blood with oxygen. But this is not all; this excitation, entirely physiological, brought upon the lungs by the air, is carried, by the pneumogastric nerves, to the medulla oblongata. This organ transmits the impression to the spinal marrow. This excitation warms the organs, that so much oxygen is in the blood, and the two, the nervous excitation and the arterial blood, make the organs accomplish their functions. Respiration prepares the blood, and the nervous system renders the organs *apt* to utilize such blood. When the pneumogastrics are cut, phenomena occur, in the explanation of which M. Bernard thinks the effect of the lungs on the nervous system has been too much neglected. He says that none of the theories for their explanation mention it. So soon as these nerves are cut, the respiration becomes shorter. The cause of death has been placed by some in the stomach, by others in the heart. In the lungs the change from venous to arterial blood is well accomplished. It is said that the change is slow, and that not enough air is introduced. But the contrary is true, and at such times more air is absorbed than at others. Their section, then, does not bring trouble into the mechanical respiration; but it is always mortal. A dog dies in three days—you find his lungs engorged with blood; but does this always exist? no, and hence we cannot give it as the cause, of death. Besides, birds always die from this section, and you never find this engorgement in them. In many other

and because there is a want of harmony between the efforts of the animal to respire and the resistance of the tissue. This harmony naturally exists in the lungs. You know that sometimes in the muscles there is that want, and they yield. When we exert our muscles, we feel when we can do no more. The nerves have been cut in horses, and then, when urged to exert themselves, they have done so far more than before, as to break their bones. Charles Bell made this experiment. The same thing takes place in the lungs—we make an inspiratory effort and we feel when we should do no more. When the pneumogastric nerves are cut, this feeling can no longer take place, and the effort is greater. M. Bernard, in experimenting upon a rabbit, found that when one nerve was cut, twenty cubic centimetres more of air were inspired, and that, when both were cut, thirty-two cubic centimetres. This forced dilatation causes, that the lungs contain more air, for they are obliged to follow the ribs and diaphragm. The lungs have a dilatibility, calculated upon a normal dilatation—a tear of the lungs results, and this tear takes place almost at once; almost *immediately* after the section of the nerves, you see an emphysema. This is the first phenomenon, to which is soon added an effusion of blood. It is almost invariably in the upper lobes that this is first seen. Then the lungs become, as it were, marbled by effusions of blood—the circulation is hindered. Of this blood, the fibrous part coagulates, and the serous fills the vesicles. In these cases the animal dies by a true asphyxia. The more young the animal, the more certainly is this the mode of death, for the tissue of their lungs is more friable, more easily broken.

However, sometimes we find no such lesions, and how are we to explain the result? At the commencement, the phenomena are the same; the effort of inspiration is greater, and more blood enters. When the animal is old, the lung is more resistant and does not break; such animals live longer, but they never live more than three days. The rabbits, upon which M. Bernard experimented, did not live twenty-four hours. When these lesions do not take place in the lungs, asphyxia does not; the blood is arterial to the last moment; the respiration becomes slower and slower, until the animal dies. How then does death happen? We must seek its cause elsewhere than in the lungs; we have, evidently, introduction of oxygen, and giving out of carbonic acid, and, until the last moment, there is a paralysis of all the other organs, and we must see how this happens. The nervous system is the bond of union of all the organs—the liver, the kidneys, the other organs of secretions—and then the muscles. Put some vinegar upon the tongue, and the sensation, carried by the nerve, is reflected upon the gland, and it

secretes—cut the lingual nerve and this does not occur, the gland does not secrete—it is paralyzed, because it does not receive the excitation. Just as the sapid body makes an impression upon the tongue, so does the air make an impression upon the lungs. The pneumogastric nerves carry this impression to the medulla oblongata, and thence, by the spinal cord, it is conveyed to the liver. But when these nerves are cut, the liver is paralyzed—the metamorphosis of the blood, and the production of sugar, are no longer accomplished. Take the liver of such an animal and you never find any sugar—the circulation in the liver is not arrested, but no sugar is produced; it has ceased to function—the liver is paralyzed. A principle then is wanting in the blood. After the liver has ceased to act, the urinary secretion is arrested, for there is, between the liver and the kidneys, the same communication as between the lungs and the liver, by means of the spinal cord; a modification, very considerable, of the blood, must result, and life of course must be very imperfectly maintained. The muscles no longer secrete, so to speak, their irritability. You know very well that the muscular contractibility exhausts itself, and that, when left at rest, it is again reproduced by the blood. But when the blood is altered, it cannot be. The animal becomes cold—the heart becomes paralyzed—its irritability exhausts itself; its contractions become very rare, and no longer push on the blood already much altered. At last the heart stops. You see then that there is a kind of chain, and that all hangs upon the respiratory act. When you cut the pneumogastric nerves, you cut the bond uniting the muscular system with the organic functions. If you galvanise the cut nerves, you can awaken the functions of the liver.

Connection between respiration and calorification. In warm and cold-blooded animals, increased temperature and frequent respiration, and diminished temperature and slow respiration, accompany each other. The temperature of animals is not so fixed as had been supposed. It is higher in the day than in the night, and at some periods of the day than at others. These variations do not go beyond one degree, and they coincide with variations in the respiration. It was at first thought that this heat was produced in the lungs. There is more heat in the blood of the arteries than of the veins, as, for example, in the carotid of the dog it measured 39° or $88\frac{1}{2}^{\circ}$, while in the jugular vein but 37° or 38° . This seemed to prove that the heat was obtained in the lungs

exceedingly difficult to experiment here. Hunter, Davy, and others, after their experiments, thought that the left blood was warmer than the other; one of them even pretended to find a difference between the left auricle and the ventricle, the blood having already cooled. Other experimenters, Magendie, Liebig, and Bernard himself, found the contrary. There is a difference in the result if the animal be alive or dead. Kill the animal as quickly as is possible; it requires time to open largely the thorax, and then the temperature of the left ventricle is the higher. Liebig, however, has shown this to be owing to a difference in their rate of cooling, the walls of the right being much more than those of the left. Put a thermometer in each ventricle, and put then the organ in water at 40° —the thermometer will stand at 40° also. Take it out, and leave it in a temperature of 16° , and in a short time there will be a difference of 6° in the two ventricles. This difference, then, is not owing to a difference in the temperature of the blood. This experiment on living animals is very difficult; the best way is to kill and try very quickly. In his experiments, M. Bernard has always found the right blood warmer than the left; the difference is very slight, and for its detection very delicate instruments are requisite. In a dog in digestion, the blood in the right ventricle was one-fourth of a degree warmer than in the left. We are authorized to conclude this, and it is of the greatest importance; the theory that heat is produced in the lungs must be then completely abandoned.

The cause of heat must not be sought for only in the physical phenomena of the body, it must be looked for also in the nervous system. Heat is not produced in the lungs, but in the other organs under the influence of the excitation communicated to them by respiration. The phenomena following the section of the pneumogastric nerves, have just been recounted. The modifications in the blood which take place in the liver, &c., under the influence of the excitation conveyed by these nerves from the lungs, is the cause of animal heat. The point of the body, where the blood from the liver unites with that from the kidneys and from the spleen, is the hottest. This is why the right ventricle is the hotter, for it receives the blood from the intestinal vena cava. The blood of the superior vena cava is less hot than that of the inferior; in inspiration, it is principally the former that enters the lungs; in expiration, the latter. The lungs then produce heat indirectly—first from the liver, after that from the kidneys, &c. All the organs, even the muscles, concur in this production of heat; it cannot be localized in any one. The skin produces heat, and yet when you examine a vein you find it cooler than the artery—but it would be far more so, did not the skin act

as it does. Varnish an animal, preventing thus the skin from performing its functions, and it becomes very cold. The arterial blood has its temperature increased in all the organs when they perform their functions, but the necessary loss of heat may be greater than this gain. An organ may produce much heat, but if the causes of the loss of this heat be greater, the temperature of the blood in the vein, of course, will be found diminished.

The influence of the nerves in the production of heat is so true, that you can by it, at will, produce an elevation or depression of temperature locally. It is perfectly shown by the experiment made by M. Bernard some years ago. He cut the cord of the sympathetic nerve, connecting its second and third cervical ganglia, and there was a wondrous difference in the temperature of the ears of the animal—a difference of two or three degrees. Here a change in the respiration, even should it occur, could not explain the phenomenon of the localization of the heat on one side of the face. If, in place of cutting the great sympathetic, the fifth pair be cut, just the contrary is seen—the side becoming colder in place of warmer, and the other side becomes warmer. This is because the one side is no longer under the influence of the excitation produced in the lungs during respiration; all of it, on the other hand, is concentrated in the portion of the nerve that has remained intact. After the section of the nerves going to an organ or to a limb, that organ or that limb is paralyzed, and the blood that issues from it is colder than before; the organ or limb itself is colder. If, in place of cutting the nerves, they be excited (by galvanism), the temperature ought to augment, and this is really the case. In a paralyzed limb, the hand had a temperature of $17^{\circ} 3$, the arm of $21^{\circ} 3$, the armpit of $26^{\circ} 1$. After having been galvanized, the temperatures were, respectively— 20° , $22^{\circ} 6$, and $27^{\circ} 1$. In the normal limb the temperature was $26^{\circ} 6$. Calorification, or animal heat, does not then take place during the arterialization of the blood, but in fact during the transformation of the arterial blood into venous. It is not during the absorption of oxygen, but during the transformation, the assimilation of oxygen in the organs. Animals become cold at the very time that they absorb most oxygen—that is to say while fasting—a proof of what has just been advanced. It is the contrary during digestion. In a dog fasting, the blood of the jugular vein was at $20^{\circ} 3$; while digesting, at $23^{\circ} 1$. In the same animal, while fasting, of 100 parts of oxygen, 74.5 were given off in carbonic acid, and 25.5 were absorbed; while digesting, 94.3 were given off and but 5.7 absorbed. The oxygen that remains in the economy is fixed in the tissues of the organs, and when these organs act, this oxygen is disengaged in

the form of carbonic acid. The animal heat is in relation with the quantity of carbonic acid thrown off, and not with the quantity of oxygen absorbed.

Continuing his examination of the phenomena of absorption, M. Bernard next commenced the absorption of aqueous bodies, beginning with water. The body can no more do without water than without oxygen. Every time a portion of the body, a muscle, a tendon, &c., is deprived of its water, it loses its properties. The organisms of some of the lowest orders of animals can be dried, and when again moistened their life recommences; water is then fundamental both for the constitution and for the play of the organs. The following table shows the quantity of water in each of the principal parts of the body:

Blood, - - - 0.81	Vitreous body, - - 0.98	Skin, - - - 0.57
Serum, - - - 0.90	Aqueous humour, - 0.98	Optic nerve, - - 0.70
Lymph, - - - 0.95	Nasal mucus, - - 0.93	Crystalline, - - 0.58
Gastric juice, - - 0.98	Sperm, - - - 0.90	Muscular flesh, - 0.77
Saliva, - - - 0.99	Urine, - - - 0.97	Bone, - - - 0.46
Pancreatic juice, - 0.91	Milk, - - - 0.93	Tendon, - - - 0.51
Bile, - - - 0.90	Colostrum, - - 0.92 (?)	Cartilage, - - - 0.70
Ceph. rachidian liquid, 0.98	Brain, - - - 0.80	Fibrine, - - - 0.81
Peritoneal liquid, } 0.95	Liver, - - - 0.68	Albumen, - - - 0.87
case of ascites, }	Retina, - - - 0.92	

We must inquire then how this water is preserved and renewed in so constant a manner; it is true that there are variations, but excessively slight for quantities so enormous. How is it that the tissues preserve water and do not yield it by exhalation? It must be considered as combined with the organic parts of the tissues. When you put them in water they do not take up any more; the water exists in the tissues, and is constantly renewed from the current without. If an animal be deprived of water, or if too much be given, no exaggeration or deprivation is found in the tissues. This property of the tissues, of saturating themselves with water, explains this fact, and if too much be taken into the economy the more is expelled. Water can be thrown artificially into the veins or arteries; if it be forced in, the animal soon dies, and in convulsions. In these cases there is an excess of water which cannot be eliminated; the blood loses at once its properties and does not circulate. The kidneys, that ought to work harder, are infiltrated and no longer act. The intestines, the spleen, and then the other organs, become infiltrated, and the animal dies from a general dropsy. A tube of glass was inserted into the artery of the kidney of a rat; when water was poured in, the circulation was soon arrested. The serum of an ox being used, it circulated some hours, and there was no

infiltration. This was done after death, and the same occurs during life.

Many cases have been cited of men who rendered more water than they drank. Ordinarily it is the contrary, for some is lost in transpiration, &c. This water must have its source, must be formed somewhere, and to account for it three hypothesis have been given. 1st. That there is an exaggerated absorption of the vapour of water. 2d. That it comes from the tissues. 3d. That it is formed at the expense of the oxygen and hydrogen taken into the system. In regard to the first, on weighing a diabetic patient before and after a bath, the difference was never more than two ounces, and this must be accounted for by the thickening of the skin. The second hypothesis cannot be true, for the man in such a case must get thinner, and this is not always seen. And, first, is the fact well observed that persons render more than they take? M. ——— made some experiments, and he always found that when more water was rendered than taken in, that his patients were not of good faith. In one case, in which he could rely, when he had given 186 ounces of water in solids and in liquids, 132 were passed in the urine. Another day, the quantities were 162 and 147; and, on others, 171 and 145, 166 and 138, and 195 and 147. M. Bernard thinks that in the observations given, of more water passed than taken in, there is such a cause of error as a celebrated physician found in his, namely, a neighbouring fountain. This question, of great interest, is not yet settled.

Numerous experiments have been made for the purpose of determining the quantity of water necessary to maintain life. Sanctorious passed the half of his life in the scales, examining what he took in and what he gave out. These experiments have been made in two ways—by observing what the body lost in solids and liquids, when none were given from the exterior; and again, by supplying the usual quantity of both. Dalton submitted himself to the latter, during fourteen days. Every day he took 91 oz. in solids and liquids. The urine was $48\frac{1}{2}$ oz., his excrements only 5 oz. Thus he must have lost $37\frac{1}{2}$ oz. by pulmonary and cutaneous transpiration, for he weighed the same at the expiration of the experiment. The quantity of carbon he lost from the lungs can be calculated at $10\frac{1}{2}$ oz., and the water at $20\frac{1}{2}$ oz.; the quantity of the former from the skin at $\frac{1}{2}$ oz. and of the latter at $6\frac{1}{2}$ oz. M. Chaussat, of Geneva, has made numerous experiments by the first method: namely, when no aliment is given. He observed both the diurnal and the integral loss. The diurnal loss of the first day was far greater than that of the others; on all the others it was almost the

same, with the exception of the last, when it was much greater, almost equalling that of the first. He found that an adult animal, entirely deprived of food, dies when it has lost the four-tenths of its weight; the only exception is when it is very fat, and then it may lose the five-tenths. Young animals die when but two-tenths are lost. Birds die very quickly, dogs more slowly; but the rule is the same for all—they die when the four tenths are lost. Now, what become of these animals when you supply them with water? They live longer, if you allow them to drink at their own discretion; but, if you force them to drink, they die much quicker—they become infiltrated, their blood is decomposed. In the animals thus submitted to abstinence, the blood remains the same—its principles in the same proportion, at all the periods. Magendie has shown this. The quantity is much less considerable, for they use their blood. It is only in the last days that, the blood being in too small a quantity, the organs cannot act, the liver no longer makes sugar, &c.; and thus the blood is no longer normal. The animal is then taken with diarrhoea, and it is by this diarrhoea that the animal loses so much in weight in the last days. His blood has acquired venomous properties, so to speak; and, injected into another animal, he dies in a day or two, as if a poison had been introduced into his system. Experiments have been made by Mr. Smith, with the same results. He observed, above all, that the happy influence of water in prolonging life is much more marked in cold-blooded animals than in warm. A frog lived twice as long—mammifers only some days longer. He has also calculated the quantity of water lost, proportionately to the weight of the animal. In the dog, each thousand grains of the animal, in twenty-four hours, loses, by the kidneys and intestinal secretions, 22.89 grains. By the lungs, the same quantity of the animal loses 16.04. Every thousand grains of the animal loses, then, during abstinence, every twenty-four hours, 38.49 grains of water.

In hybernating animals, a curious phenomenon has been observed—they lose in weight, but not regularly, as in forced abstinence, and sometimes they are found to have increased in weight. Four marmots were weighed before and after their hybernation, which commenced the 8th of January and finished the 26th of February. The first weighed 2,226 gr. and 2,078—his loss, therefore, was 148 grains. The second lost 139, another 58, and the fourth 57. The last two were much smaller than the others. There are oscillations in which an increase of weight can take place. In one of these animals, in two days, there was an increase of two grains. How is this? They take no food. It comes from the oxygen taken in by the lungs.

The absorption of gases is much more simple than that of liquids. In gases the cause had to be sought for in the properties of the blood, the membrane having no direct influence upon it; in liquids we must consider also the properties of the membrane, of the epithelium, which plays no part in the absorption of gaseous bodies. M. Bernard demonstrated this by six cœca of chickens, filled with blood from different parts of the body, bile, &c. A tube was put in each, and they were all placed in distilled water; the height of the ascension in the tubes showed the amount that entered. The one containing bile was soon very high. The membrane is very important, and is always constituted for the liquids to be brought in contact with it. This was shown by the following experiment upon a frog. He was placed in salt water and soon died; a true endosmose occurred, salt entering and the blood going out. The blood did not coagulate when drawn from the body, and was very salty to the taste. The animal died from an endosmose, that would not have occurred had his skin been organized for such a medium. There are other animals who cannot live when a morsel of sugar is dropped into the liquid, as infusoria. An analagous phenomenon is produced, owing to the thinness of their skin. In the absorption of liquids, then, it cannot be indifferent in what particular part of the body the substance be introduced.

The intestinal tube is intended to absorb the aliments. Nutritive baths and nutritive lavements are spoken of, but do they really exist? When an individual has taken a nutritive bath, his weight has increased; but is not the water in the skin the cause? Admitting that the nutritive matters do penetrate, do they nourish? When aliments, sugar, albumen, &c., are introduced under the skin of an animal, they are soon eliminated by the urine; thrown off as foreign bodies. If, on the contrary, they are injected into the stomach, they do not appear. In order that a substance fulfil its part in the organism, it is not sufficient that it enter, but that it enter in a particular way. M. Bernard performed the following experiments upon two rabbits, choosing those animals because the cellular tissue of the dog, the animal he generally makes use of, is much more dense, and in consequence the experiment would be much more difficult. Into the cellular tissue of one, by means of a syringe, he threw the half of a solution of sugar; the other half was injected into the stomach of the other. As it might be objected that the difference in the result was owing to the fact that the stomach absorbs more slowly, he introduced into each solution some yellow cyanuret of potassium. In a short time, in the urine of the first rabbit both the

substances appeared, while in the second the cyanuret of potassium was alone thrown off.

The digestion of aliments consists in their solution; but that does not suffice. In order that they may be definitely assimilated, they must pass through the liver; particularly albumen and sugar. If, in place of injecting the solution under the skin of the rabbit, it had been thrown into the *vena portæ*, the result would have been the same as when injected into the stomach. Nutritive baths are, then, a pure illusion; but it is not the same with nutritive injections, for the veins of the large intestines pass through the liver.

What are the phenomena of absorption—by what mechanism is it effected? They are about the same in all substances, but in some they are more simple than in others. Imbibition, a phenomenon entirely physical, is essential. We must know that if a substance be not imbibed by an organ, it cannot be absorbed. This happens in the oils; for though animal membranes generally absorb aqueous substances, they do not the oily. This has necessitated a preparation of the fats, and for this purpose there is an organ in the animal economy. Again, the current of imbibition must take place more in one direction than in another; for, evidently, without that there would be no movement. It is these phenomena, above all, that depend upon the nature of liquids, upon endosmose.

These two, imbibition and endosmose, are not sufficient; there must be transport. This is active, depending upon the circulation; the other two are physical and are accomplished as well in the dead as in the living, provided the substances be not changed. However, on this point it is necessary to be very careful; for, take a tube, place it so as to be perfectly horizontal, and put some water in each end. The water will come together; yet here there is nothing vital, nothing active. This phenomenon is called diffusion. Again, the diffusion is not the same for all substances. Put on one side a solution of sulphate of iron, and on the other one of prussiate of potash; they will meet together, but the blue will not be found in the centre of the tube; one has marched quicker than the other. This is very important; for, wishing to determine the rapidity of the circulation, from their experiments, some men have decided that the whole circulation was accomplished in thirty seconds. But, had they taken other substances, they would have had a different result for each one. The phenomenon of diffusion must, then, be considered, besides that of transport. If, into the stomach of a dog, prussiate of potash be introduced, and he be killed ten minutes

after, that substance will be found all over the canal, from the mouth to the anus. What can you admit here? Surely not that the peristaltic movements have transported it everywhere, for the aliments have not been chased. It is a phenomenon of diffusion, and takes place just as well in the dead as in the living animal. Again, put some in the mouth—you can find it in the cesophagus, so that finding some there is no sign of its having been swallowed.

M. Bernard commenced with absorption in the cellular tissue, for there its study is most easy, most simple; for it is freed from the element coming from the epithelium. These absorptions under the skin, or under the epidermis—for it is the same thing—are generally the result of accident, following scratches, wounds, &c. The epidermis is taken away very often, to cause the absorption of medicaments. These substances are absorbed with greater or less rapidity, but the rule is that absorption takes place here with very great rapidity. Experiments have been made to ascertain at what time, after the inoculation of the poison of the glanders, of the rot, &c., the ablation of the part frees the animal from danger. In some of them, conducted at Alfort, it was found that, at the end of an hour, the poison of the glanders had been absorbed. Others are absorbed much more rapidly; as, for instance, the rot (*la clavelé*); the destruction of the part five minutes after the inoculation was too late. For *la rage*, the time is not known. All these poisons are without any effect when introduced into the intestinal tube.

Can all soluble substances be absorbed in the cellular tissue? No, as can very easily be shown. The experiment performed by M. Bernard was the introduction under the skin, in distant parts of the body, of two substances, one, the lactate of iron, the other the cyanuret of potassium. Where the iron had been deposited, a Prussian blue was found, and only there in the whole body—showing that while the cyanuret had been absorbed and taken into the circulation, that the lactate had not been disturbed. Again, upon taking the membrane of an animal, and placing it between those two solutions, the same thing happened. The blue was formed on one side, and not on the other; the prussiate of potash was alone imbibed. There is, indeed, a double exchange; the water in which the ferruginous salt is in solution, passing, but the salt itself does *not*. It will be seen, then, that an iron salt must never be employed by the endermic method, for it will never enter the blood. The old law, "all soluble bodies are absorbed," is not true. The above experiment settles the question.

Other circumstances can make absorption vary, above all the rapidity of the absorption. The concentration of the liquid has a great effect.

Having introduced the same quantities of sugar, but dissolved in different quantities of water, it might be expected *à priori* that the more diluted would be the first absorbed, for the quantity is the same, spread over a larger surface. Just the contrary takes place. You can make a *gramme* of sugar pass in five minutes, or in three or four hours, just as its solution is concentrated. This shows how precise it is necessary to be in making experiments, and when you are as precise as in chemistry and physics, their results are equally precise.

Besides the modification arising from concentration, there is another from facility of imbibition. This is very readily shown by placing a dried membrane between different solutions. In such a case, between the chloride of sodium and the sulphate of soda, there is a difference of double. If these two substances be placed under the skin, the chloride will be absorbed far more quickly than the other.

Can physical bodies, mingled together, re-act on each other—for instance, will those two bodies, just mentioned, be imbibed exactly as when separate? They always act for themselves, acting exactly as if alone. They act just as they do separately, if there be no combination be it understood; one cannot *draw* the other. And this is very important; for it might be supposed then, when strychnine and opium, an excitant and sedative, are administered together, a neutralization would result. This is never the case; each preserves its properties intact.

Two liquids placed in contact have a tendency to mix together. This property of diffusion is possessed by some liquids to a much greater degree than by others; and the difference exists with or without an intervening membrane. A small instrument, consisting of a tube of glass, whose ends are bent upwards at right angles, is used to measure this property. The greater part of the horizontal portion being filled with distilled water, the liquids to be tested are poured into the upright extremities. M. Bernard made use of the two substances before made use of, and also of solutions of nitrate of silver, and of chloride of sodium. The chloride of sodium has a greater property of diffusion than the nitrate of silver. Those substances that march most rapidly thus, do the same when membranes are interposed, and also in the living animal. For these experiments, an instrument composed of a flask, with a bottle of animal membrane suspended in another much larger, and both covered by a globe to prevent evaporation, has been used. The central flask containing 12 grs. of dry chloride of sodium, and the larger one holding distilled water—at the end of forty-eight hours, not a particle of salt was in the former, but it contained 60 grs. of water. The chloride of sodium was replaced by the same quantity of dry, crystal-

lized sulphate of soda; at the end of ninety-six hours all had gone, and it was replaced by 120 grs. of water—twice as much as in the other case, but twice as much time was necessary. There are some substances, as prussic acid and nicotina, that escape with wonderful rapidity. These bodies, in the torrent of the circulation, go faster than the current of the blood. An experiment with this instrument shows most evidently the truth of what was advanced above, that when two substances are mingled, the one does not draw along the other. The common salt and the sulphate of soda being placed in the flask together, at the end of forty-eight hours the former was all gone, and one half of the latter was remaining. The membrane used here was a pericardium. Wishing to apply these facts, M. Bernard thought to see if a different effect would be produced on a living animal. He mingled salt and sugar, thinking that perhaps the latter would be more rapidly absorbed. At first, he supposed such to be the fact, and finding sugar in the urine much quicker in such cases, concluded that it had been drawn along. Mixing strychnine with sugar, he thought that perhaps the animal would die less quickly; but he always died in the five minutes, whether the strychnine was alone, or with salt, or with sugar. But why did salt always draw along the sugar, and it alone? It is because of the chemical change that occurs when these two bodies are mingled—a combination, known perfectly well by chemists, but which he had not thought of. It was not sugar that had been absorbed, but the new body formed. In such experiments, care must always be taken not to have a chemical mixture. In therapeutics, this is very important; for if such a mixture do not occur, the bodies will act separately.

Influence of endosmosis on the phenomenon of absorption. In endosmosis, there are always two currents, two movements—endosmosis and exosmosis—only there is an inequality. In the living animal, we have the phenomenon of exosmosis, without the other; the equivalent endosmosis does not exist. M. Bernard, under the skin of one rabbit, introduced a solution of common salt, and under that of a second one, sulphate of soda. The animal uttered cries of pain when the salt was injected, as it always does; for this substance has the property of exciting the nerves in a peculiar manner—it never occurs from the introduction of sulphate of soda. These liquids were not replaced by any blood; all was absorbed; and in their place, nothing appeared. Why is this double current not observed? Is the cause special to living animals? This cause should not be sought for first in the vitality; it should first be sought for in chemical or physical phenomena—for vitality means we know nothing about it. It depends on the movement of the blood, and on its com-

position. As soon as the solution has passed, it combines, chemically, with substances in the blood, and the current carries it away. Endosmose is but a tendency to equilibrium; here, it cannot be, for ever there is new blood. The *sortie* takes place, but far away, in special organs, the lungs, the kidneys, &c. Some practical deductions can be drawn from this. It has been supposed that saline purgatives acted by the water passing from the blood; but place some in a portion of intestine, separated from the rest of the tube by ligatures. In some cases, nothing whatever is produced, and all the salt is absorbed; in others, you do find some liquid, but that shows nothing; for, without the medicament, the inflammation and irritation produce that. Injected into the jugular vein, a salt purges better than when swallowed; and in some countries, for the sake of economy, horses are thus purged. Absorption, then, is simple imbibition, and endosmosis, as understood, is not applicable.

Absorption is diminished or increased by a change in the pressure of the air. When an individual is in compressed air, all the liquids of his body tend to go into the interior; in the contrary condition, they go towards the periphery. Now, as absorption takes place even from the periphery towards the centre, in compressed air, absorption should be more rapid, and rarified air should act in a contrary manner. Many experiments show that absorption is less active, when the atmospheric pressure is diminished; thus, it has been said, that after the application of a poison, that the absorption is prevented by the immediate application of a cupping glass. Liebig has made an experiment, entirely physical, to show the effect of compression. He took a curved tube, one end of which was covered by a membrane, and having introduced some water, poured mercury into the other extremity. The weight of the metal caused the imbibition to be far more rapid. Prussiate of potash, placed on one side of the diaphragm of a living animal, appeared on the other in forty minutes. When a galvanic current was made to pass, it appeared in seven; a compress, imbibed with iodide of potassium, was placed on one arm, and on the other one dipped in a solution of starch; a current of galvanism being passed through the body, in ten minutes the compresses became blue. This latter experiment, M. Bernard said, he had not repeated, and that it appeared to him very extraordinary.

As to the influence of the abstraction of blood upon absorption, Magendie has made many experiments, and these are the results. When a poison was administered to an animal, after having been bled, he always died quicker. When the contrary was done, blood being transfused, he died more slowly. It was judged, that in the one case, absorption was

quicken, and in the other, retard. Evidently this can be, for we enter into the conditions, just discussed, of greater or less pressure; but it ought to require more poison to destroy an animal with more blood, than another. The same quantity of poison that destroyed one animal, will not kill a larger one; the poison must be, in a certain proportion, in the blood, and at the same moment. A large quantity can be given, little by little, and it passes away, without destroying life. Ten grains of prussiate of potash in one hundred of water, will kill an animal; diminish the quantity of blood, he dies sooner; increase it, he does not die at all. In this case, the substance was injected into a vein, so that absorption had nothing to do with the result. When you bleed, you make the animal more susceptible to the poison; you make him smaller at the same time that you vary the atmospheric pressure.

In absorption, there are a certain number of mucous membranes to study; and, above all, that of the abdominal intestine. It is certain that in them we have the double current; we have the excretion, the going out of a liquid; and this excretion, or secretion, must be studied. This *going out* is not allied in a mathematical manner to the phenomenon we have observed in a membrane. In the cellular tissue, absorption is simply imbibition; here in the mucous membrane we have the vascular membrane as before, and moreover, we have small cells in great number around the vessel, and in immediate contact with it. What difference is there between absorption and excretion? First, the current is directly contrary, and that is a great thing. Glands consist of numbers of cells, surrounded by ramifying vessels; between the cells and vessels there is no direct communication, only juxtaposition. The mechanism of secretion has been much discussed. If it be imbibition, why is the current inverse to what it always is in imbibition? A theory very old, developed by Haller, is, that it is owing to the interior pressure exerted by the blood. This is disproved by the following experiment of Ludwig. In the salivary duct he placed a tube, to which he attached a barometer, or curved tube of mercury, to measure pressure. In the artery going to the gland he placed a corresponding tube or barometer. He then stimulated the gland by exciting the nerve, and made thus, as his barometers showed, a greater pressure in the duct than in the artery; still the secretion continued. It is not right, however, to generalize too much from this experiment made on the salivary glands; for in the kidneys, when you diminish the pressure of the blood, you diminish the flow of urine.

If the secretions of the mouth be examined, they are found to be very different from those in the intestines. The salivary glands pro-

duce liquids very dissimilar; that of the parotid is clear and limpid, that of the sub-lingual is very viscous, and that of the sub-maxillary is between the two. The parotid is influenced, above all, by mastication, the sub-lingual by deglutition, and the sub-maxillary by gustation.

Now, relating to the excretion of particular substances. When in the blood certain substances are introduced, that can circulate without danger to life, we find them in different secretions. There must be a difference in the glands, although anatomists cannot show any, between a cell of the pancreas and one of the parotid. The salivary glands eliminate some bodies and leave others. If iodide of potassium be introduced into the body, by the cellular tissue, or the stomach, or by a vein, in about one second it is found in the salivary glands. The slightest trace of iodine is thus eliminated. If on the contrary, prussiate of potash be introduced and sought for, it is never found there; it passes through them. Sugar never is excreted by the glands, and there is no analogy between it and the prussiate of potash; it has more with the iodide. Again, if you inject lactate of iron into the blood, and much may be injected without harm, and then draw away the saliva, you will not find a trace of it in that secretion. Now take two substances, of which one passes and the other does not, as in the iodide of iron—what will happen?—will the iodine make the other pass, or will the iron keep it in?—or will the gland decompose it and send out the one, holding in the other? The iodine is the stronger, and the iron goes out with it, drawn in its train. Thus in experiment, as for example, to see if mercury passes, we must see with what it is combined; for while as corrosive sublimate it may not, combined with iodine it may. No substance is eliminated with such facility by the glands as iodine; and although the products of the salivary glands are very different, yet there is no difference in the elimination of that substance. Every substance has its special organ of elimination.

The mucous membrane of the mouth and that of the glands is not continuous; it is said to be continued into the duct; but absorption is twenty times more rapid in the duct than in the mouth. Because the glands have for their special object the expulsion of certain bodies, it must not be supposed that they cannot absorb, for they do absorb and very rapidly. M. Bernard introduced into the parotid of a dog a tube, and into his jugular vein injected together iodide of potassium, prussiate of potash, and sugar. Of these three substances, thrown in at the same time, the first was the only one that appeared in the fluid issuing from the tube. While absorption then allows all to pass, and takes place in the ducts of glands as well as in the cellular tissue, excretion is

confined to particular organs for particular substances. When sugar is injected, none is ever found in the saliva. It has been said to be found there in cases of diabetes, but it never is when the saliva is pure ; it is owing to its being mixed with the mucous from the lungs, &c.

Do absorption and secretion take place simultaneously—are there two currents, as when water and sulphate of soda are separated by a membrane—or do they occur successively ? When strychnine was introduced into a gland, and the secretion of the gland at the same time excited, in place of almost instantaneous death, the animal lived eight or ten minutes. Nowhere is absorption so active as in the glands, but when secretion is made more active, it is very slow. The two actions are then evidently not instantaneous ; for if they were, one would be quickened with the other. When a gland secretes it does not absorb, and when it absorbs it does not secrete ; were not this the case, it would be ever absorbing its own secretion.

There are other phenomena to be examined in the mucous membrane of the mouth ; besides this absorption, there are *special* absorptions, as that of taste. “Savor could not be perceived if not absorbed ;” it has then been thought that there was an absorption. It must be instantaneous ; but this absorption has rather been admitted *à priori* than proved. If the chorda tympani be cut, we find that gustation has lost its instantaneity and to have become very obtuse. (The chorda tympani is a branch of the facial nerve that is attached to the gustatory.) After its section, if you put some sulphate of quinine upon the tongue of a dog, that before, had at once started away on its application, he only does so at the expiration of two minutes, and then the sensation seems obtuse. If then it be an absorption, we must admit that the nerve modifies it, and it would be a solitary example of such a thing. After the section of a nerve, absorption is never immediately changed ; it is only afterwards, when the inflammation takes place. There are in the tongue small muscles, that are inserted into its papillæ, and are supplied by the chorda tympani. These papillæ, to act, must put themselves in contact with the sapid body. The action then of the nerve is readily understood, and the phenomenon produced by its section. Again, to show that it is not a sapid absorption but a sapid touch, there is a sensation when the gustatory nerve is excited by galvanism. By it, at the anterior part of the tongue, you have a sour taste ; farther back it is better. If you admit absorption, you must say that the anterior parts of the organ absorbs sour substances, &c., which cannot be. Give a dog very sour soup, he will not eat it ; cut the nerve supplying the anterior part of the tongue, the lingual, and he will. Give him some

very bitter, and though he had before refused, he will eat it after the section of the glosso-pharyngeal.

M. Bernard did not speak of absorption in the cesophagus. It is only a passage, whose epithelium is very thick, and absorption takes place then so slowly that it has been said not to do so at all.

The study of the double movement of absorption and secretion, in the stomach, is very interesting. While in the mouth, the juices are always alkaline, in the stomach they are acid. This liquid is secreted by a gland, but one whose cells are not agglomerated, but disseminated all over the stomach. These small glands have two different shapes, but the cell is the same everywhere. The long finger-like gland is found above all, at the pylorus; the others are chiefly found at the extremity. It is supposed to be the long, finger-like gland that secretes the gastric juice. During abstinence, the gastric juice is not formed. Unlike the saliva, it allows almost every substance to pass; every liquid substance, for the gaseous pass off by the lungs. In this property of eliminating every substance, it approaches the urine. Having determined this property, M. Bernard endeavoured by it, to discover the real glands that secrete the gastric juice. For this purpose he injected into the blood of an animal a salt of iron and the prussiate of potash. Upon opening the stomach, he found prussian blue, but only upon the surface of the intestine; there was none in the glands themselves. If sugar be injected, a great quantity is found in the stomach. The same thing occurs in diabetes. Sugar has been vomited, after a meal of meat, by diabetic patients, and it has been said to have been made then in the stomach, but it is no such thing; it is a secretion. Urea is generally eliminated by the kidneys, but in certain cases, by the stomach, as urea, or as ammoniacal salts. If the kidneys of an animal be removed, after some time, you find their secretion in the stomach; it is only at the end of five or six days that you find it in the blood. So long as the animal is gay, the stomach performs that office, and it is only when that organ no longer acts, that the urine accumulates in the blood. The gastric juice is only secreted during eating. The stomach is never empty; it always contains some liquid; we cannot introduce substances into its small glands, but, after the experiments on the mouth, it should be admitted that they would absorb also.

Absorption in the stomach is effected on bodies very different, aqueous, alimentary, medicamental, &c. Its rapidity is very various; the longer an animal has fasted, the quicker the absorption; in full digestion it is almost null. When fasting, in repose, the mucous membrane has very little blood; it flows there in proportion as digestion takes place. When

it is pale, it absorbs very rapidly, and when red, very slowly. The pancreas, the salivary glands, &c., are also pale when in repose. When the salivary gland is not secreting, absorption is immediate, as we have seen by the introduction of strychnine, the animal being killed at once. It is the same with the stomach. These facts are in relation to what is seen everywhere in inflamed organs, and an organ that is secreting is in a state, so to speak, of transient inflammation, the phenomena it presents simulating all those of inflammation.

There are certain medicamental substances that undergo a change in the stomach before being absorbed, as for instance the salts of iron. When an insoluble salt of iron is introduced into the stomach, it is not absorbed, certainly, while insoluble. In chlorosis, it is said, the iron has diminished in the blood; give iron, and its symptoms cease. But, here the question is not, as to whether iron cures the chlorosis, but to know if chlorosis be owing to a want of iron, and again, if iron be absorbed. When blood is calcined, there rests a certain quantity of oxide of iron; there is also some in milk; there is certainly then iron in the body, and how does it get there? The blood of chlorotics has never been examined to show that it contains less iron than that of others; it is said, indeed, that it contains fewer globules, and the iron being in the globules, there must be less of it, but we should know if there be less in one globule. Suppose a chlorotic patient needs three grammes of iron, so soon as the quantity is introduced, he should be cured, but a very long time is necessary for the cure of these cases and sometimes the iron does cure them. On injecting iron into the stomach, M. Bernard has never, for that, found any more in the vena porta. Its absorption cannot be demonstrated, as that of the iodide of potassium; it requires new researches. Besides, the theory of the cure of chlorosis not being admitted, there is an action, entirely special, of the salts of iron. So soon as in the stomach, all the points touched by them are seen to be excited. A very distinguished physician declares chlorosis to be a vice of nutrition, and hence this is more in accordance with the results of experiments, for the iron acts as a stimulant. The question is not at all resolved.

Alimentary matters are albuminous, fatty and sugary. The albuminous, when introduced, are solid; hence, not absorbable at the moment when the stomach can absorb; afterwards the other movement of secretion prevents absorption. Sugar is absorbed, provided it is dissolved; fatty matters are not; with the gastric juice they act just as with water; when solid, they melt; that is all.

Do the nerves distributed to the stomach have an influence on absorp-

tion! On this point there are contradictory doctrines. Delpsch (!) cut the pneumogastric nerves of a horse, and immediately introduced into the stomach, a powder of nux vomica. The animal was not poisoned; but there is an objection to make here, for the section prevents the gastric juice to form, and hence the nux vomica was not acted upon; the strychnine, its active principle, was not extracted. Another experimenter found a globule of this salt fatal. M. Ballet introduced a dissolved extract into the stomach; at the expiration of some time the animal was poisoned; but he remarked that the absorption might have taken place in the intestines, after having passed there. He then tied the pylorus; the animal was not poisoned, and he concluded that absorption had not taken place in the stomach, but in the intestines, after the matters had passed there. These experiments repeated, above all at the Veterinary School of Turin, have shown the facts to be so, but they have been explained differently. Before the section of the nerves, the animal dies in a few minutes after the administration of the strychnine. In all the experiments conducted at Turin, the animal was killed by them the following day. On searching in the stomach, none of the poison was ever found. When they introduced, in the same way, prussiate of potash, they found it in the urine. Without, then, cutting the pneumogastric nerves, but only tying the pylorus, they found that the animal did not die from the poison. How then did the poisoning not take place! All these experiments have only been made hitherto in the horse. Now, this animal has, on one side of his stomach, a large surface covered with epithelium; the part secreting gastric juice is very small, and perhaps this anatomical fact accounts for the results in these experiments. That morning M. Bernard had tied the pylorus of a dog. His stomach was empty, and iodide of potassium had been introduced. The animal had vomited, so that, not much had been left, and the act of vomiting produces a viscid secretion on the surface of the stomach, hindering absorption. On testing his saliva for the iodine, it was found there. M. Bernard then introduced some strychnine; the animal vomiting, he introduced more. He said he thought that absorption took place after the ligature of the pylorus, but that there is a trouble in the elimination. Even if the animal be not poisoned, that does not prove that the poison is not absorbed; for some things do go directly to the kidneys, and there are cast out of the system, without having ever entered into the general circulation. This animal, two days after, was still alive; he had repeatedly vomited. M. Bernard left the subject for the time, as his experiments were not sufficiently numerous, taking it up again, as will be seen, before closing his course.

Absorption in the small intestines. It is necessary, in this place, to say something of the liver and the pancreas. The liver is ever the nearer to the pylorus; the ductus choledochus never changes; it is always at the commencement of the intestine; the duct of the pancreas changes. The biliary fluid is sometimes poured out in company with the pancreatic, and sometimes alone. Bile is one of the most complex bodies in the economy, it is a fluid altogether special, and not to be likened to any other in the economy. As regards the bodies eliminated, it differs from saliva and the pancreatic juice, and resembles more the secretion of the kidneys. It is never found in either of the former, but it is sometimes in the last. Many physiologists say that bile is only a fluid to be expelled from the body; that some of its components are excrementitious is no argument in their favour. In vertebrated animals it never contains sugar, but in the invertebrated there is normally sugar in the bile. Sugar cannot be considered excrementitious, hence the bile cannot be. The liver eliminates a great number of bodies above all the metals. There are a certain number of bodies, that, absorbed, localize themselves in the liver, as arsenic and mercury. Some bodies, when absorbed, are thrown into the bile and again absorbed, but there are others that are localized, concealed in the tissue of the liver, and sometimes iodide of potassium having been given, they are thrown off. Experiments to show this have been repeatedly made.

All those substances, iron, &c., that are not eliminated by the salivary glands, are not by the pancreas; hence it resembles them to some degree. M. Bernard brought a dog before the class in whose side a tube had been introduced to let out the pancreatic juice; some iodide of potassium was given, and its presence was sought for by the usual test in that secretion. The first did not give the blue colour, but there was an error in the experiment, the sulphuric acid he had made use of (to free the iodine) having transformed the starch into sugar. On trying again iodine was found, but he said it did not pass so quickly as in the salivary glands, and that in this respect the organs differed.

Below these ducts, from the liver and the pancreas, the substances contained in the intestine undergo all the changes necessary for their absorption; formerly it was said that in the small intestine the chyle was absorbed, and that it was found in the chyloferous vessels, but these are only lymphatics. The term chyme does not indicate any thing precise, nor does the word chyle. There are three kinds of substances entering, and the fatty matters alone enter the chyloferous vessels; all other alimentary principles find another way into the system.

Fecula, in the state of fecula, undergoes no change in the stomach;

in the duodenum it becomes sugar, &c., and as such is absorbed. As absorption takes place by a single order of vessels, the branches of the vena porta, and never by the other order, the chyliiferous. If you tie the portal vein, and then give sugar in its different forms, cane sugar or grape, you will always find it; in no case is any ever found in the chyliiferous vessels. Sugar is said to be found in the thoracic duct, but this sugar does not come from the intestines, but from the lymphatics of the liver. Sugar is never found there before those lymphatics have emptied themselves. M. Bernard has found sugar in lymph taken directly from the lymphatics of the liver, and again he has found sugar in the thoracic duct of an animal that had only eaten meat. Cane sugar must be changed; grape sugar can serve some purposes, but it is not so apt for nutrition as it is after a change. They undergo a second change in the liver. Substances are there a second time digested.

Albuminous matters are dissolved and absorbed only by the branches of the portal vein. To show this is difficult. To show that no sugar is absorbed by the chyliiferous vessels is very easy, for all you have to do is to test for sugar; but, in regard to albumen, how can it be told whether you have the albumen of the lymph, or that absorbed from the contents of the intestines? Cane sugar is so little assimilable, that the smallest quantity injected into the jugular vein is found in the urine. When thrown into the portal vein it is not so, it remains in the economy. This is precisely true also of albumen injected into the jugular vein; you find it in the urine, hence were it absorbed by the lymphatic system, which empties itself into the venous just in that region, it would of necessity be eliminated in the urine.

Chyle represents simply the fatty matters. We have seen that these matters cannot pass a membrane, they do not even moisten it. To be absorbed they must be modified, they must be rendered soluble. Until the pancreatic juice is poured into the intestine, none is ever found to be absorbed; after that its absorption goes on throughout the whole intestine, until all is taken up. In the rabbit, the pancreatic duct empties itself 35 or 40 centimeters below the biliary. It is seen that fatty bodies do not pass through the liver; the first organ they pass is the lungs. It was shown, long ago, by Magendie, that they cannot traverse the liver. A great number of analyses have been made of the blood before entering and coming out of that organ. If there be some before, there is none after; it rests there or is changed. There are several ways of absorption then for aliments; there is no chyme and chyle, as was believed in ancient times.

In other classes of vertebrated animals, birds, reptiles and fish, the

chyliferous vessels do not exist; that is to say, you find no chyle. Their lymphatics are found filled with a clear, limpid fluid; chyle is only found in the mammiferous. Why does it not exist in the others? We must admit that it is absorbed by the portal system. There is in these animals a system of veins altogether different, anatomically different, from that of the mammiferous. In the latter, the system of the vena porta is like a tree, completely isolated. It has no communication with the general system but by the hepatic veins. In those other classes, the tree of the vena porta communicates very largely with the general system, and thus the fatty matters can deviate.

Absorption cannot be said to be only effected in the small intestines, but it also takes place beyond that. The ancients thought the cæcum a second stomach, so to speak, and that it digested substances that had escaped the first. This is exaggerated. The comparison arose from its having the same reaction; the stomach is ever acid, and in the cæcum there is an analogous acid reaction. In the intestine the reaction is alkaline. It was imagined from this that the cæcum secreted a juice proper for digestion. These experiments had all been made on herbivorous animals, and the result is not the same on others; in the carnivorous the reaction of the cæcum is alkaline. Now-a-days the reason of this is known. Generally when an animal eats, much more is taken in than is necessary for alimentation. There is always much of this that is not absorbed, but after having undergone the usual changes, it arrives in the cæcum. Here this excess, and the substances refractory to absorption, rest some time. What happens? In the herbivorous, the aliments being sugary, fecula, &c., their putrifaction produces acetic acid. There is a great difference then in the stomach; the acid is a juice secreted from its walls, in the cæcum; it comes from the reaction of the substances contained. In carnivorous animals, the substances in the cæcum are albuminous, &c., and their putrifaction, as every one knows, produces ammonia and ammoniacal salts. This is so well known, that the reaction of the cæcum can be changed at will, by the change of food. If after having given too much food, you give too little, the animal can be nourished by what is in the cæcum. The fatty matters are not absorbed by the large intestines, but the sugary and albuminous are, as they are in the whole extent of the alimentary tube. Fatty matters are only absorbed in the small intestines after they have been properly prepared by the pancreatic juice. Nutritive injections can then, as was before said, be given with perfect propriety. Should they be of milk, of course only the sugary and albuminous parts will be absorbed, and not the fatty.

Why are not the walls of the stomach absorbed, since the stomachs of other animals are digested? It was formerly said to be an effect of vitality; and, it was said, kill an animal, and if you keep it warm some time, the stomach and the neighbouring parts will be digested. Gastric juice digests because it holds in solution an organic principle, pepsin. Meat, alive or dead, is digested perfectly well by this principle. M. Bernard introduced the hind parts of an eel and of a frog, both living, into a fistulous opening in the stomach of a dog, and they were absorbed and digested. The reason then cannot be that it is alive; it is owing to its epithelium, and when it exists, this active principle, pepsin, is not absorbed. When poison was introduced into the stomach of the dog, no harm followed, and all of it was afterwards found there. The poison was harmless, not because it was digested, but because it was not absorbed. The epithelium of the stomach is soft, yet not less than that of the hand, does it prevent substances from passing. The active principle of serpents does not exist in their blood, and this poison is fatal to themselves, though it has been said not. It exists in their glands only. In the stomach there is constantly a renewal of the epithelium; the pepsin is not found in the blood, but in the organ itself, which produces it. When you kill any animal the epithelium no longer renews itself, and it decomposes very quickly; the gastric juice then can act, for it can be imbibed—an imbibition that cannot take place during life, for it is prevented by the epithelium.

Action of the nervous system on absorption. The nervous system itself never absorbs, it never serves as conductor to a poison, &c. The following experiments were made with strychnine upon a limb, where you would have a vein, an artery, and a nerve. When the first two were cut, the nerve being left alone, its application produced no effect. Applied to a nerve itself, it produced nothing, and yet it is a substance acting on the nervous system, producing convulsions, &c. When the nerve was cut, the application of the poison produced death. When but the artery was left no effect was produced. The nervous system then has no influence on absorption in the cellular tissue. The nerve cut was a cerebro-spinal. In experiments recently made on the sympathetic, M. Bernard found that upon cutting a branch going to a part, that its calorification was increased, and absorption also, but, at the same time, the circulation was much more active. The nerve does not absorb, but it acts upon the circulation; it does not act directly, but indirectly, by rendering this more active. The influence then of the nervous system on the phenomena of absorption is null in the cellular tissue. Poisonous substances act just as well in a paralyzed limb as in another. It has,

however, been thought, that although this may be true for the external parts, it is not for the internal. Experiments have been made to show that the stomach cannot absorb after the section of the pneumogastric nerves. M. ———, of Alfort, experimented first upon a horse, and only says that the animal was insensible to the powder of *nux vomica* made into paste. This poison is not soluble, and could not act, therefore, on the animal. It is true that the poison did not act, but the secretion of the gastric juice is arrested, as every one knows, by the section of those nerves, and the gastric juice is necessary to dissolve the poison. After this solution of the difficulty, they injected a soluble salt of strychnia, and that proved fatal. This, however, did not prove that it was absorbed by the stomach, for it might have gone into the intestines. They then tied the pylorus, preventing thus the substance from passing, and the animal was not poisoned. Without cutting the nerves, the pylorus was simply tied, and the result was the same. It cannot, therefore, be put upon the section of the nerve, but upon the tying of the pylorus. It is impossible to admit that these nerves influence absorption. M. Bernard brought forward a rabbit whose pylorus he had tied a quarter of an hour previously, and whose pneumogastriacs had been cut previously to that. He had given him the cyanuret of potassium, and, upon examining his urine, there was none to be detected; the time, however, had been short. Why does the ligature of the pylorus produce this effect of preventing absorption? It is an effect entirely mechanical. The stomach of the horse is very small, and on the splenic side there is an epidermis so thick that there is no absorption. Liquid matters in the horse pass at once into the intestines, so that the surface from which absorption takes place is very extended. When you tie the pylorus it is far more limited. When you wish to kill by poison, you must give it so that there will be a certain quantity in the blood at a certain time. This is demonstrated again by the slowness of absorption, as was shown in the rabbit a moment ago. Here never enough poison is absorbed to kill, for enough is never at one time in the blood. If, in place of strychnine, you give prussic acid, the animal dies, for a much smaller quantity suffices. These are then perfectly mechanical phenomena that take place.

Absorption is a phenomenon of imbibition, having no need whatever of the cerebro-spinal system. The grand sympathetic has an influence, but not as nervous system; it is in causing to be borne circumstances influencing it. There is a kind of correlation between absorption and secretion, and pathological conditions can arise in default of this.

In all the parts of the body in which absorption has been studied, it

was always necessary to consider imbibition as passive. Once in the blood, the substance circulates, is put in contact with the nervous system of certain organs, and excites a movement of, from within, out. It was seen that never in the living animal do the two currents exist without the intermedium of the nervous system and the circulation. It is thus the action of purgatives must be understood. Sulphate of soda carried into the blood, is put in contact with the glands, and these secrete more. Without the connection established by the nerves between the two phenomena, it cannot be understood how life can continue.

There are still an infinity of things to elucidate.

ART. III.—*Medical Progress :—A Lecture Introductory to the Course on the Institutes and Practice of Medicine.* By SAMUEL HENRY DICKSON, M. D., LL.D. Med. Coll., State So. Ca.: Charleston, November, 1853.

ONCE more, gentlemen, I bid you welcome, and greet you with a cordial salutation. I see before me, with infinite pleasure, many familiar faces, and gladly anticipate the change, gradual, yet I trust not slow, which is to convert strangers among you into friends. I acknowledge the heavy responsibilities which belong to *my* position here, and pledge myself to the utmost exertion of my ability to sustain them. On your part, I expect a similar sentiment of obligation to be duly felt and acted on.

You have chosen a course of life full of hardship, sacrifice and difficulty. A toilsome pathway lies before you ; yet at every step of the ascent, you will find cheering compensation for the labourious effort incessantly necessary. Medicine has been well described by one of its illustrious cultivators—Prof. Christison—as “a restless profession, whose *history* is constant innovation and continual discovery ; and its pride that it cannot stand still, as the world advances in science and civilization.” He had just previously spoken of the law, as “an institution whose principle is stability, and its ruling passion resistance to change.” If we consider that theology abounds in stereotyped creeds, and regards all innovation as heresy or sacrilege, we shall comprehend the peculiarity of our position professionally as the emphatic advocates of progress. We are indeed bound to advance with every rising wave of knowledge ; for our science is so connected with, and dependent upon, all other sciences and all arts, that the very fundamental institutions

which are to govern us are receiving perpetual and instant modifications with every improvement of old, or discovery of new doctrines and principles around us, whether in physics or psychology.

I cannot perhaps make a better use of this our first meeting and introduction to each other, than to sketch a brief outline of the more recent movements in the department of our profession, to which I am specially attached; and it may well be premised that all movement tends to progress, though it may often take at first a wrong direction. This apparent paradox is easily explained by the consideration, that in exploring our way from the known outwards into the unknown, we shall not unfrequently be driven to the exhaustive method, by which the various errors into which we are likely to fall shall be examined and condemned in turn; their ruins, and the discoveries made during the investigation, serving to guide us to the correct truth at last.

Physiology, which, notwithstanding the argument of Prof. Bartlett, I cannot but regard as the essential foundation of scientific medicine, bids fair to offer to the hands of future builders a mass of solid and available material. The improvements made in the construction of the microscope, and the delicate yet profound inquiries of organic chemistry, have opened to us rich mines of information. Let me beseech you to look steadily and with eager eyes, into the book of nature, unfolded, page after page, by these two methods. If you should meet with any one who shall talk to you of the fallacies and uncertainties of optical instruments, and of the experiments of the laboratory, be prompt to reply to him that these fallacies and uncertainties must be properly understood and allowed no more than their just and fair appreciation. What we see actually exists; we must take care not to assume the existence of any thing that is not actually seen. A positive addition to our amount of knowledge of something that can never be lost or taken from us, is palpably and undeniably made by every increment of power in the lens—by every new result of observation and experience in the laboratory. It is when we begin prematurely to explain, to infer, to deduce conclusions from insufficient premises, and carry out hasty and conjectural analogies, that we go astray and fall into confusion and error.

The tree, however, is best known by its fruits; and in the domain of hygiene, we are already reaping a rich harvest from our physiological investigations. Enlightened as to the normal relations of dependence upon the objects which surround and affect us, we have done much to improve the conditions both of individuals and masses.

We have succeeded in convincing our clients of the advantage of

breathing pure air, drinking unpolluted water, removal from among them of offals and offensive matters, and promoting, in every way, cleanliness, order and comfort.

Model lodging-houses are now erected for the poor; baths are furnished them; cemeteries are banished from the haunts of living men, and domestic and public architecture controlled by proper rules for ventilation and the admission of light.

Chemistry and the microscope have been brought to bear upon the important inquiry, as to the alledged adulteration of articles of food and drink offered to the public, especially in large cities. The frightful truth has been established by the analytical sanitary commission of London, that "in purchasing any article of food or drink in that great metropolis"—and similar examination has given similar results in our own country—"the *rule* is, that you obtain an adulterated one, the genuine commodity being the *exception*; and that the articles used for adulteration are always of inferior quality, often worthless, frequently positively injurious, and not uncommonly even poisonous." Remember that this is affirmed not only of the conveniences and luxuries of life, but of its most essential necessities—of bread itself in all its varied forms; of flour and of milk; and rejoice with me in the reflection that the discovery of such evils cannot fail to secure their removal by exposure and punishment, or to effect at any rate their immediate and permanent diminution.

Quarantines, hitherto conducted, for the most part, with a tyrannical disregard of private rights and interests, are destined to undergo most beneficial modification. So great have been the abuses of the system, and such the indignation naturally excited by them, that there is some danger of its temporary subversion and abolition. We regret to read even in the proceedings of the French Academy of Medicine, and in a report to which are appended the names of Magendie, Louis and Londe, the entirely premature and erroneous assertion of the "inutility of quarantine:"—and we enter our protest against a conclusion so hasty and groundless, the result of views so narrow and partial, and so totally unworthy of the high authority which sanctions them.

The management of hospitals has been in every way improved, but especially in our detection of the risk of concentrating contagious and infectious cases in wards set apart for their reception. By diffusing them we avoid the development of their higher degrees of intensity, and the attendants and visitors are far less liable to their malignant attacks. We have even been led to doubt the alledged advantages of segregation in the pestilential epidemics in which it has been so long

customary, as in typhus, cholera and small pox, &c. and the subject is still under serious consideration.

The influence of locality on the production of disease, and the importance of removal, are now beginning to be properly appreciated. To New-York belongs the credit of depopulating and fencing in an "infected district:" a wise and excellent regulation. If it were universally followed, and promptly, on the invasion of local epidemics, I do not doubt that many valuable lives would be annually preserved.

In the examination of the effect of varied occupations upon the public health, each new discovery in the arts requires new researches. Thus, in the substitution of zinc for lead, it was at first supposed that we had got rid of the dangers of saturnine poisoning entirely; more recent experience, however, shows that the zinc wires used to fasten the corks of champagne bottles, and the oxides of that metal employed as paints, do produce a series of evils very similar to those occasioned by the salts of lead, though somewhat slower in development and less severe.

Lucifer matches are now a necessity of civilization, and are manufactured and used in infinite numbers; yet at a cost of great suffering and almost irremediable injury to the workmen engaged. They are liable—perhaps not in a large proportion, and only after some considerable time—to a malignant and gangrenous ulceration of the gums, cheek and jaws, from the material handled and inhaled. Let us hope that science will soon provide these unfortunate operatives with a sufficient protection or counter agent.

The new and beautiful art of daguerreotyping has also been accused of inflicting terrible calamities upon some of its cultivators. They suffer from the irritative properties of iodine inhaled in vapour; ophthalmia follows the exposure of the eye to it; vertigo is said to be produced by it. In one case, related by Wright, there was "absorption and atrophy of the testicles; and, in some instances, almost total extinction of the sexual appetite."

Pathology has already begun to derive obvious advantage, direct and indirect, from the better conducted investigations of Microscopic Anatomy and Organic Chemistry; and from the aid which they give in our studies of Comparative Anatomy and Physiology. From Bernard's investigations into the functions of the liver, the detection of a saccharine secretion which he ascertained to take place within it, and the marked influence of injury of a portion of the nervous system upon this secretion, we shall undoubtedly receive aid hereafter in the appreciation and treatment of maladies affecting the digestive system, in renal

diseases, and in the fevers of warm climates, in which the liver is supposed to be especially implicated. By defining clearly the true offices of this great organ, we shall acquire precise views of the modes in which its derangements are connected with varied forms of disease; points on which, as you are well aware, the most vague, contrasted, and exaggerated opinions have been entertained.

Marshall Hall—whatever be the judgment which shall be pronounced by posterity upon the doctrines which he has promulgated so earnestly and with so much enthusiasm, and advocated with a dogmatism aroused by unfair criticism, and therefore excusable—Marshall Hall deserves to be held in high respect for his acute and successful endeavours to deduce pathological principles first, and thereafter therapeutical practices, from discoveries in the anatomical arrangement and distribution, and the physiological functions, of the cerebro-spinal axis and nerves.

Dowler, on the banks of the Mississippi, with equal boldness and originality, but with opinions often discordant from those of our scientific Englishman, pursues a similar course of inquiry into the functions, sympathies, and disturbances of the nervous tissues, which cannot but be pregnant with valuable results; and Brown-Sequard, in his teachings on both sides of the Atlantic, maintains that we have too readily yielded to despair as to the capacity of the animal body to restore injured portions of the vertebral column or to recover from the paralyses from such injuries. Similar hopes as to restoration from cerebral softening are also, of late, held out to us by Durand Fardel as the result of careful and repeated observation.

Kluge, Lebert, Leidy, Burnett, and many others, are engaged in per-severing examinations of minute morbid structure and deposits, and we shall doubtless soon ascertain the intimate nature of cancer, tubercle and other diseased productions. Such knowledge, when obtained, must and will be made available, in due course of time, therapeutically.

The diagnosis of Leucocythemia, as a new disease, has been made by Prof. Hughes Bennet, of Edinburgh, and the morbid multiplication of the white corpuscles of the blood rendered evident under the microscope. Prof. Wood recently reports a case so diagnosed in Philadelphia.

Common psora has long been known as a parasitic affection. Vallex lately detected the presence of animalculæ by the lens, in a very troublesome form of pruritus, heretofore treated empirically and unsatisfactorily. The sufferings of the patient were at once relieved by the ready destruction of their living cause.

Whether *life* be only a modification of the ordinary forces of nature, as some are now disposed to contend, or, as has long been believed, a

force or principal altogether peculiar, is a question of difficult, and, as yet, unattained decision. But its manifestations are always important to recognize; its presence, ever influential, should be carefully observed and appreciated; and I am inclined to the opinion that a morbid vital force, connected with some extraneous form of organization, parasitic, irritative, injurious, perhaps malignant, will be discovered to be present and active in an increasing number of examples, as our means of apprehension, and our instruments of observation, are improved and multiplied. The conjecture of Linnæus has not been and never will be verified, so far as to establish the universal dependence of disease upon parasitic life; but enough is already known of their frequent connection to make us respect his prophetic sagacity. Animal growths upon vegetable structures, and vegetation flourishing on diseased animal tissues, are now seen to be not rare. The germinal property in some contagious cells, visible and palpable, and the analogous capacity of reproduction and self-multiplication inherent in and displayed by certain forms of epidemics, are fully recognized. Indeed we might well feel some astonishment at the slow and reluctant admission of this semi-individuality or parasitic vitalism in many pestilential maladies, when we reflect upon the ready assumption of what we call vital properties, by dead matter in an infinite diversity of conditions. Food eaten this morning by us has already, by mere solution, digestion, absorption and admixture with our living fluids, itself become living. The portions which are flowing along our veins, at this moment, in the transition state—just passing through the mysterious and incomprehensible change—may, in an hour or two, go to constitute the substance of the brain, the organ of immortal thought, or, received into a tissue of vessels scarcely less delicate or important, be deposited in ova or spermatozoa, one or both endowed with the germ-force, the highest attribute or characteristic or type, perhaps, of life-force. Surely we need not wonder, then, at the ready development of an indefinite extent and variety of morbid vitality under fostering contingencies.

Do not imagine for a moment that I am about to press upon you a system of parasitic pathology, or to add another to the long catalogue of theories of disease with which your course of study will be found encumbered. Nothing can be farther from my purpose, inclination and habits of thought. It is requisite, however, that you should know something of all these theories or hypotheses, and more especially of those whose influence you are destined to encounter in your future professional life. They constitute portions of our literature, and are engrafted even upon the severest forms of scientific medicine; they lie

upon the borders which divide almost imperceptibly an inevitable empiricism from a coarse and repulsive charlatanry. They force themselves upon our notice, and you will be mortified to discover how much they affect and interfere with your prospects of usefulness and success, and how often they take deep root in cultivated and refined society.

The most fashionable and widely prevailing of them all, you are familiar with, at least by name—Homœopathy—which has now included among its avowed followers certain persons of some distinction, and among them a professor in the time-honoured school of Edinburgh, venerable by age, and illustrated by the memories of Cullen, and the Monroes and Gregorys. All error must contain—if it live—some nucleus of indestructible truth; all success implies some mode and modicum of desert. So in this misty delusion, the eclectic finds a lesson of much value, as to the capacity of resistance to disease in the unaided constitution, and the paramount importance of a well regulated regimen and course of living, to which the boasted triumphs of homœopathy are doubtless owing. Its infinitesimal divisions, and absurd minuteness of dose, only serve to re-establish in its proper position the medicine expectante—too apt to be neglected or underrated by the modern practitioner, in the growing habit of heroic or impertinent medication. If it prompt you to a careful selection of cases in which transient disorder will pass away without more injury to the organism than will be incurred or risked by interference—and such cases undoubtedly present themselves from time to time and in considerable proportion—this charlatanism of the most ingenious and reputable of charlatans, Hahnemann, will not have existed in vain.

To Hydropathy you will have nothing to object but the absurd combination of universality and exclusiveness, urged for it by some of its professors, and you will derive from it many valuable suggestions. Currie, Bertini and Bell have dwelt ably and truthfully on the various topics of hydrology; and waters, pure and mineral, and medicated, and equally varied baths, have been always used freely in the treatment of disease, both by savage and civilized man. But it was left for a shrewd Silesian peasant to show how many varied methods of employing one remedial agent might be made efficient, and you will avail yourselves of his ascertained results, in all their diversity, with proper discrimination and prudence. The death of this extraordinary man at the age of fifty-four years, left his immense establishment almost without a head, for Priessnitz educated no pupil. It would be as idle to deny his merits as to attempt to disguise the defects of his character. He probably prescribed for a greater number of patients than ever fell under the care of

any other practitioner in any age or country. For some time before he set up the institution at Graeffenburg, to which all the world sent its large representation, he had been in extensive practice at home as an itinerant. There, it is calculated that he received not less than 47,000 sick people, of all nations and all ranks. He left a fortune, as we are told, of nearly two million of dollars; the greatest sum ever accumulated by any follower of the healing art by his practice. His successes were truly remarkable, and the nature and amount of testimony brought to prove his skill, attention and sagacity establish, beyond cavil, the greatness of his genius. His inveterate hostility to scientific medicine and physicians was only equalled by his tyrannical obstinacy of his insatiable avarice.

Oinopathy, or Oinothearpy—is the title of a new method of cure, instituted by Shroot, a German, and followed exclusively in an establishment set up by him near Graeffenburg, in opposition to Preissnitz. Shroot treated all diseases by wine and good diet, as his neighbour did by water alone. He also boasted of his wonderful successes, and all the influence of the millionaire empiric could not put him down. He was fond of receiving cases which hydropathy failed to benefit, and, we may readily believe, made many cures among them. The violent temper of the Silesian would not permit him to reciprocate this course. He obstinately refused to receive any one who had been prescribed for by his rival, and the mortification and annoyance to which he was subjected in the contest, is affirmed by one of his biographers, to have hastened his death.

The Medico-Botanic system—the Thomsonian—the Botanico-Eclectic, or whatever other name our American form of quackery may chose to be designated by, is one of the coarsest ever prevalent. We must be permitted to express our wonder at its prolonged existence, and our still greater surprise at the erection and endowment of colleges and schools devoted to its promotion. From even this you may learn something; the capacity of resistance to, or evasion of poisons in the animal economy; the uncertainty of therapeutical inferences from assumed pathological conditions; and the imperative necessity of great reserve in the ascription of effects to apparent and coincident causes. I have known fevers which the learned Clutterbuck would have regarded with much seeming reason as clear examples of phrenitis, and such as the ingenious Broussais would have logically argued to be violent gastro-enterites, treated with some of our most vehement local irritants and stimulants; yet without fatal issue.

It may interest you to know that while, merely struggling here in the

land of its birth for the protraction of its diminishing sway, among the less informed and less reflecting classes of the community, it has emerged into some notoriety in England, under the new and not inappropriate name of *Coffinism*. You will at once recognize it in this description, taken from the London Pharmaceutical Journal. "Coffinism is an open and impudent repudiation of medical science, philosophy and anatomy. There is no disguise or mummery in this system. The entire code of practice is in Dr. Coffin's book; the practitioners are shoemakers, chandlers, &c. The remedies are medicinal plants, some of which are powerful poisons. They are not administered in infinitesimal doses, but by the teaspoonful or penny-worth; and for the protection of the practitioners when *inquests* occur, it has been found expedient to establish a 'Defence Fund, to which all subscribe, and in the benefits of which each participates when in trouble.' The system is described as gaining ground extensively among a class of persons less able to take care of themselves than the wealthy dabblers in globules. A mother thinks her child looks dull; she takes him to a Coffinite cobbler, who administers a tea-spoonful of lobelia inflata; the child becomes worse—the dose is repeated; it dies—an inquest is held. The defence fund is brought into use; the jury is made to ascribe the death of the child to the physician, called in at the last extremity; the cobbler is acquitted, and retains the conviction that if the child had taken another dose of his medicine, it would have recovered."

Of Dyspathy, the transient opponent and contrast of the water-cure internally, there remains but one relic. Williams proposes to shorten the vexatious course of catarrh—one of the most annoying of ordinary afflictions—by total abstinence from all fluid ingesta. It may, perhaps, be said to survive, also, in its application to dropsy by some physicians, who have the sternness to refuse relief to the unquenchable thirst of that cruel disease.

Isopathy, one of the most ancient of medical superstitions, has lately been revived in a novel form. In the obsolete "doctrine of signatures," it was taught that, the physical or visible peculiarities of plants denoted their therapeutical application. The yellow celandine was hence used in jaundice; saffron is still vulgarly employed with similar vague reference to the colour; and liverwort derives its name from the same notions. Some wiseacre carried out the idea into the choice of articles of food, and prescribed, as specially appropriate diet, the healthy parts of animals to those in whom those parts or organs were supposed to be in morbid condition. So, liver, sound and well cooked, was to be the frequent meal of the hepatic invalid; rognons, a la champagne, the agree-

able and proper delicacy of the sufferer from renal affections; and tripe, I suppose, would be selected for the victims of intestinal and gastric maladies. Under this head, too, we may include the administration of pepsin—so called—to the dyspeptic, and pills of felel bovm, or ox gall, to the constipated. But, in a recent number of a French Medical Journal, we find the most extreme illustration of this notion. ‘A Prussian groom,” says the editor, “became weary of cleaning the king’s horses. Looking about him for some more honourable and lucrative profession, he casts his eyes upon medicine. Armed with a lancet he comes to Paris, the propitious nurse of chevaliers of industry. Without farther preparation he instals himself; and do you know what use he makes of his lancet? He bleeds, without doubt; yes! but to what end? For the purpose of procuring human blood, which he administers to the sick in form of pills; and he builds upon it we know not what unintelligible system, and makes, of this fine recipe, the basis of his materia medica.”

Among the curiosities in the department of medical literature, we may mention Metallo-therapie, the application of metals in the treatment of disease, upon the theory of a peculiar relevancy or specific adaptation. It is by no means to be confounded with electropathy, or electro-magnetic influences, so familiarly resorted to in various modes. “Anesthesia and amyosthenia,” says M. Salneuve, “are the indications of this method of cure.” It would seem, however, that there is nothing, in any case, tending to designate *à priori*, the eligibility of any one metal or combination of metals, rather than any other. As yet, it is pure matter of experiment and observation, requiring much time and patience. In a memoir, by M. S., upon “the treatment of certain nervous affections, by the metallic rings of Dr. Burg,” the inventor, we are told that the results are highly interesting, and abundantly compensate for the time and labour bestowed in their attainment. One case is given, of cure by laminated bell-metal; another, by English steel; a third, by virgin gold, which, it seems, acted exclusively on the morbid sensibility present, leaving, untouched, the coincident derangement of mobility. These examples, which are varied and numerous, include many forms of neuralgia and hysteria.

The most modern of all these systems, however, is that which is learnedly designated Kinesipathy, or—following the French derivation from the Greek roots, kinesi-therapie, invented recently by the Swedish gymnast, Ling, and advocated, in almost all languages, by Sonden, Georgii, Roth, Doherty, and Blundell. It has been improperly termed medical gymnastics; but this phrase gives an incorrect and insufficient view of it; for it includes, besides active exercise—general and partial—

an infinite variety of passive movements, pressure or compression, friction and percussion. So much of what is old and excellent, is found collected here, that you are prepared to approve, on their first announcement, the general doctrines and theory of Ling, which seem, indeed, on a careless view, little more than an expansion or extension, and a nice development in detail, of rules and practices long since familiar to the profession. Gymnastic exercises, irregular and specially regulated, are among the most ancient appliances of our divine art; frictions, instituted by Asclepiades, and commented on by Hippocrates, have a whole chapter devoted to them by Celsus, in his elegant work "*De Re Medica*." In modern times, friction by shampooing and other methods, has been particularly urged, in the treatment of disease, by Pugh and Grosvenor; compression and percussion, by Sir John Sinclair and Dr. Balfour. I have before me the description, with plates, of "a pulsator," or percussing instrument, invented by Dr. Gower, to act with precision upon diseased parts, more than a half century ago. This *passive* course has been elevated into a separate system, by Dr. Mayer, and dignified with a classical appellation—*Masso-therapic*. "*Massage*," as he calls its practical employment, "is destined," he predicts, "soon to become a serious art (*un art serieux*) and, for those who exercise it, a profession lucrative and esteemed."

But to return to Professor Ling. He does not advance, as so many of his predecessors have done, any claim to a method of universal cure. On the contrary, if I understand him, he proposes merely, in the words of one of his followers, "an addition to the resources of the healing art." It is not a remedy for all the ills that flesh is heir to. Other modes of treatment are required in fevers, epidemics, and acute diseases. After all this, however, the pretensions of the new system are wonderful and lofty, and require, as Malgaigne has it, "a very robust faith." "Life is to be considered," we are taught by Professor Ling, "as a whole, composed of three constituent parts—chemical, mechanical, and dynamic; or, in other words, vegetative, animal and spiritual life." "Health depends on the relative balance of these three functions or parts of the organization; sickness supervenes when the balance is overthrown. To cure sickness, to restore this balance, we must seek to increase the power of the agent, or part, whose operation is deficient. It is an error to attend, in preference, to one or the other of the fundamental forms while health exists; and it would be equally a mistake to neglect, in sickness, that form or function which calls for active aid. It would be wrong, for example, to endeavour to cure a fever by gymnastic operations; (though Herodicus thought otherwise;) for the animal function is already in ex-

cessive action, as may be learned from the accelerated circulation and the hurried breathing. Here, chemical treatment is indicated. No operation or function can take place in our organization, except by the reciprocal action of the three great fundamental forms of life. If *animal* life predominates, disease takes on the *vegetative* forms; if *vegetative* life be in excess, disease assumes the *psychical* type; and should the last named predominate, the disorder will put on the *animal* character. Thus, fear, joy or anger, either produce excess of muscular power or entirely take it away. Exercise produces a feeling of health, assists digestion and induces sleep. The chemical operation of spirituous liquors causes either great depression or great excitement."

All this, so far as it is intelligible, seems plausible enough; but in the application of the theory and in the details of the practice, kinesiopathy runs into the most extreme absurdities. For example—in Dr. Roth's book, the first class of disorders is enumerated as "congestions of the head—headaches, giddiness, &c.; and for these are prescribed, 1st, derivative movements of the legs and feet; 2nd, passive rotation of the feet with active, passive extension and flexion of the feet." In chronic inflammations of the larynx and windpipe, are recommended, "1st, a tolerable strong vibration with moderate pressure employed on both sides; 2nd, submaxillary vibration; 3d, double oblique lateral friction of the throat; 4th, point-vibration on the windpipe; 5th, double frictions from the throat down the shoulders and arms; 6th, longitudinal friction of the larynx, with three separated fingers; and, lastly, loud speaking, reading and singing."

I will only add that urethritis or gonorrhea is affirmed to be successfully treated by "percussions upon the sacrum applied from above downwards;" and that even opacity of the cornea is subjected to this mechanical or gymnastic management. "In a case of opaque cornea, Dr. Neumann employed the following measures: 1st, pressure on the supra-orbital region 20 or 30 times daily; 2nd, passive rotation of the head 8 or 10 times daily; 3d, frictions in the direction of the superior longitudinal sinus and transverse, and the beginning of the internal jugular vein; 4th, flexion of the trunk with resistance; (active passive;) 5th, active rotation of the lower extremities; 6th, percussion of the soles of the feet with a cylindrical piece of wood:"—a sort of revulsive bastinads, in the Turkish manner, I suppose.

I have one remark to offer upon all these methods of empiricism, which ought, I think, to obtain the attention of the profession. They seem to me to present themselves in the light of *protests*: Homœopathy, a mere return practically, amidst all its illusions, misty efforts at

reasoning and infinitesimal absurdities—a mere return to the medicine expectante, is a natural, shall we not acknowledge, an inevitable and not unprofitable reaction against heroic, reckless, and contra-stimulant doctrines and medication? And who can decide upon the comparative risk of the two extremes—the routine employment of the lancet on the one hand, antimonials, mercurials, &c, and on the other—the puerile nihilities of drops and globules of microscopic potency.

Hydropathy contains a well founded and forcible complaint against the neglect, daily growing greater, of one of nature's most available and accessible remedies. Electro-Botany or Thomsonianism is a blind and desperate attempt of ignorance to correct the errors of science, by substituting stimulants for excessive depletion, rendered fashionable by Clutterbuck, Rush and Broussais; and heat externally by steam, and internally by pepper, for the triple panacea of ice, gum water and leeches.

Kinesipathy is merely a fair demand for proper attention to those mechanical modes of relief which our predecessors valued and eulogized, but which we have permitted to fall into comparative, if not absolute desuetude.

Let us profit by the lessons thus intrusively urged upon us, and in the spirit of genuine and philosophical eclecticism, avail ourselves of all the resources which experience and observation have gathered for us; omitting nothing, rejecting nothing which may enable us to serve our suffering clients, or better to fulfil the onerous duties of which we have assumed the responsibility. Let us pity and enlighten, not revile or repel, the ignorant and vulgar among our opponents, while we hold ourselves ever ready to expose and denounce the knavery of the false and mercenary charlatan, whether in or out of the ranks of the profession.

I have said that even the errors of those who surround us can and ought to be made instructive. The spirit of experiment cannot safely be repressed, although it is so strongly apt to run into dogmatism or exclusiveness. The ingenuity of inventors is wild and wasteful to an extent scarcely credible; yet the results of its exercise, at whatever cost procured, are indispensable. How much time is thrown away, what an infinite sacrifice of labour, life and mind in useless and abortive inventions, can be seen in the records of all patent-offices, which, nevertheless, do not retain traces of one thousand part of the expenditure of physical and mental energy. We should remember that the sage and prudent eclectic is not likely to be a discoverer; this part of the office of the Therapeutist is almost of necessity left to the zealous empiric,

too often to the desperate and ignorant quack. Boerhaave introduced no new remedy, nor Sydenham, nor Cullen, nor Drake. We owe to Paracelsus the benefits of mercury and antimony, and the manufacture of paregoric; and to Dover, the valuable powder that bears his name—an almost equally happy hit; to an obscure Swiss, iodine; and to a Dentist our anesthetics.

Nor are we to learn the full value of any of our drugs from the scientific physician, impressed deeply and influenced always by the force of Chomel's first rule—"to take care to do his patient no harm." Fabius, always on the defensive, was not meant by nature to invade, conquer or annex new domains.

I do not eulogize, nor even excuse the recklessness with which our enterprising brethren sometimes administer new or virulent agents; but it must be admitted that such are our true pioneers, and that we must follow where they lead—not blindly, but with rational and cautious survey of the ground which they have traversed with head-long and daring enthusiasm. The more active the poison, the higher claim it had upon Stoerck; Hale risked his life to ascertain the effect of the direct introduction of medicines into the circulation; and Ellenberger has recently sacrificed himself in the search after antidotes. It is to such as these that we owe the important advantages resulting from the large doses of quina, and the promise of great benefit from the free use of opium in dysentery and puerperal peritonitis. To the rare courage of Green we are indebted for the best remedy in chronic laryngitis, and a very efficient aid in croup; Joubert has lately extended the application of nitrate of silver, as he affirms, successfully to the irritable glottis in whooping cough; and with a highly commendable boldness, Norwood has restored and raised to its proper place in our apparatus medicaminum, the *veratrum viride*. It is somewhat difficult to adjudge in instances of this sort the precise degree of merit, which must depend upon the train of thought, founded upon rational observation, which led to the result. Something may be due to accident doubtless, but we should never forget that the most fortunate accident will happen in vain to the dull, the timid or the inattentive.

We are becoming more and more familiar with the inductive method of deriving therapeutical inferences from physiological and pathological doctrines, and the discoveries of chemists and microscopists. We find a striking example of such induction, if it be established, among the honourable and diversified labours of Marshall Hall. Analyzing the complicated elements of one of our most terrible and unmanageable diseases, epilepsy, he came to the conclusion, that the convulsive coma

which forms its latest and severest manifestation, was owing to laryngismus or the spasmodic closure of the glottis; and suggested that great relief, if not absolute cure, almost certainly the rendering abortive each paroxysm, cutting off the last stage of the attack, thus converting the grand into the petit mal, would be obtained by any means that would keep open the air passages. Already we have authentic reports of several cases, in which tracheotomy was practised with this view, with results more or less satisfactory. Who will not envy this scientific philanthropist his ennobling triumph, if experience shall confirm the exulting anticipations thus excited! A farther step, in the same path of reasoning, led Watson to the idea, that the irritative contraction and closure of the glottis might be prevented by the action of nit. argent. upon this opening; and the employment of the strong solution by the sponge introduced in Green's method, is affirmed also to have been useful, palliatively, at least, as a substitute for the inconvenient operation above named.

It is not in place here to dwell upon the benefit derived from anæsthetics in surgery and obstetrics; but we must not omit to speak of the extension, inductively, of their exhibition in numerous forms of painful and spasmodic, irritative and ataxic affections. Thus, I have been gratified, beyond expression, at the relief afforded by chloroform to the sufferers from neuralgia, spasmodic cholics, certain headaches, rheumatism, and the tetanic agonies of spinal disease. We have accumulating notices, too, of its kindly influences in asthma, angina, pneumonia, delirium tremens, and even in true tetanus, and the indomitable hydrophobia.

We may enumerate here some curious instances, in which researches directed by theories erroneous, or, at least, unsettled and disputable, have led to valuable discoveries in therapeutics. Piorry, assuming that intermittents arise from splenic congestion, experiments upon all substances which resolve this congestion, and finds that common salt is a powerful remedy for such fevers; a statement strongly supported by Herrick and others.

Dundas, begging the question of the unity of fevers, and reasoning from the ready subjection of the malarious or periodical types to quina, whence they are well denominated by the French, quinine fevers, carries out the exhibition of the same remedy in the continued fevers of Great Britain, as he and his imitators affirm, with marked success. I need not tell you that, although I have given the name of Dundas as recently prominent in this suggestion, he was, long since, preceded in the advo-

cacy of the doctrine and practice by our countryman, Professor T. D. Mitchell, now or late of the Pennsylvania Medical College.

The unsustained hypothesis, that quina is specifically anti-periodic—a notion difficult to grasp or comprehend—has led to its use in neuralgias and other intermittent and recurrent disorders; often, without doubt, beneficially in greater or less degree. Its administration in yellow fever, consequent upon the confusion of this pestilence with the autumnal remittents of hot climates, is also alledged to be a most efficient method of cure, at least in certain localities, as maintained by Blair, Harrison, Porter, and others.

In this category of advantageous induction from vague theories of disease, we should not forget Dr. Rees' mode of treating rheumatism, by lemon juice, which so high an authority as Babington regards as "holding a prominent rank among the strides that have been made in the various departments of medical knowledge, since his pupilage." How it acts, chemically or otherwise, whether as an alkaline salt correcting an acid acrimony in the fluids, or 'as a vegetable acid, sedative and tranquillizing, we would gladly know; but have, as yet, ascertained nothing beyond the fact of its great utility.

Upon a more obvious and plausible hypothesis, Beneké has introduced the phosphate of lime into the treatment of scrofula, rachitis, and other cachexies, in which the calcareous element, so essential to many of the animal tissues, is supposed to be deficient. Professor Stone, of Louisiana, gives his weighty testimony in its favour. The same notion has led to its employment, as a means of promoting the union of reluctant fractures; and it is said to form an important adjutant to cod liver oil, in the cure of tubercular phthisis. Analogous reasoning, from induction, has given rise to the application of surgical measures to the relief of diseases hitherto treated by internal medicaments only or principally. Besides the tracheotomy of epileptics, already spoken of, this is exemplified in the recent practice of paracentesis by Trousseau, for the removal of the serous effusion in acute pleurisy; by the timely evacuation of the accumulating fluid, the condition of the patient is alledged to be greatly improved; and his hope of cure, in contingencies otherwise nearly desperate, very decidedly increased. The same may be said of Boquet's daring plan of procuring adhesion of the opposite surfaces of the peritoneum in abdominal dropsy, by the injection of stimulating fluids into the sac, as in the treatment of hydrocele. Many lives have been saved

of New-York, we are indebted, principally, for bringing this important topic clearly before the profession ; instructing us in the diagnosis of this serious affection, and constructing an instrument suitable and convenient for making the requisite incisions.

I mention, finally, before concluding this brief report of the inductive progress of therapeutics, the employment of collodion as a means of local compression in cutaneous diseases, and also in certain inflammations of soft parts, as of the mamma and testis, and in peritonitis, and in articular rheumatism. The insertion of glycerine into the ear in deafness, with indurated cerumen, and inunction in scarlatina, perhaps deserve to be added.

Let it be distinctly understood, in reference to the above detail of alleged progress, that I do not make myself in any degree responsible for the statements recited, nor do I even offer an avowal of full belief in the asserted facts. I have merely selected, from the best authorities, the records of those efforts which seem to me to promise most, and to be best sustained. You will become more and more aware, as you grow older, of the great uncertainty of testimony, and in our own profession, of the extreme difficulty of deciding upon the value of evidence, and of distinguishing between real and apparent truth. All statistics are received with a certain reserve, by writers in every department of social economy ; the impression is universal, that medical statistics are surrounded with greater vagueness, and therefore less worthy to be depended on, than those collected in any other quarter. Quetelet carries this opinion to the extent, I think, of an unjust severity, in his "Essay on Probabilities ;" in which, however, amidst his unduly harsh strictures, he tells us some wholesome truths. He speaks of medical statistics as "observations incomplete, incomparable, suspected, heaped up pell-mell, presented without discernment, or arranged so as to lead to the belief of the fact which it is wished to establish, and nearly always it is neglected to enquire whether the number of observations is sufficient to inspire confidence." A far more correct and fairer exposition of our insurmountable difficulties is conveyed by the learned Theophilus Thompson. "Numerism," he contends, "is only productive by the amount of *lumen siccum*, intellectual intuition, applied in the selection and appreciation of facts. Time is often lost in the collection of miscellaneous facts. There is an aristocracy of facts, as well as of races, and the mind should be taught to discern their prerogative dignity. The naturalist who cannot, or will not, see that one fact is often worth a thousand, as including them all within itself, and as it first makes all the others facts ; who has not the head to comprehend, the soul to reverence

a central observation or experiment, (what the Greeks, perhaps, would have called a 'proto-phenomenon,') will never receive an auspicious answer from the oracle of nature." Surely it does not need that I should remind you how few such observers there are, and how small a portion of our statistics are derived from such sources. No one would hesitate in making a choice, where the characters of the recorders or witnesses are known to him. I would prefer an impression recorded by Sydenham, or announced by Andral or Chomel, to a volume of tabular columns, even from the Corypheus of the numerical system, Louis. One page of results, ascertained and collated by Drake, would weigh more with me, than the contents of a dozen case books taken from the mass. But, where the men are unknown to us, we can attempt no such comparative appreciation. It only remains for us, then, to receive the miscellaneous recitals, and place them on record, for future examination, rejection, or confirmation and collation.

In thus accumulating testimony, it is, nevertheless, always desirable to obtain the details numerically, and in tabular arrangement. Admitting that these details are often of little value in themselves, and that they are always liable to colouring and misapprehension, it is surely more convenient to refer to them when definitely set down; in building, it is better to have our materials moulded and compressed, as brick are more easily handled than irregular lumps of clay, and stone, hewn into whatever form, preferable to shapeless masses of rock.

You will be interested to know whether any steps in advance have been taken of late in medical education. Perhaps no topic has been more discussed in England, and in our own country, than this; to what purpose, time must show. Colleges are multiplying rapidly enough, and sufficient diversity is exhibited in the arrangements which they institute for themselves. The recommendations of our national convention have been partially carried out by a few of the colleges; a very few have lengthened the term of collegiate study, but the majority regard any change of that sort as unadvisable. The change most to be deprecated, however, is surely an abridgment of the curriculum, or whole period of study. This is, however, virtually effected by the repetition of terms, a summer course of lectures following, almost without interval, that of the winter; thus allowing two sessions to succeed each other, terminated by examination and graduation of numerous candidates, within the year, as in more than one of our northern colleges. Preliminary instruction has of late received more of the attention it deserves, and schools are established for this purpose in several of our cities. There are two such in this place, whose prelections are as useful to the classes, as they are

honourable to the teachers. One is in operation in Boston, one in New York ; one, I believe, in Richmond, and one was, some time since, commenced in Knoxville.

Certain other novelties deserve mention under this head. Competition, by reduction of fees and expenses, rather than by increase of advantages or opportunities offered, is no recent offence against propriety ; two, if not three of the western colleges, have publicly cut down, by a sudden subtraction, more than half the cost of the course. But, we must not confound with this indecorum, the free school system of tuition in medicine, which, advocated a good while ago by one of the most distinguished northern professors, is now actually carried into operation, at Ann Arbor, in the University of Michigan. A large class attends, regularly, the course delivered in the medical department of that institution, by a respectable corps of professors, receiving no fees from them, but handsomely enough endowed by the State. The principle is a plausible one, and well adapted to please the masses, who, in certain sections of our country, intent upon a levelling equality, will hardly be satisfied until all distinctions of condition are removed by the interference of governments, and a modified agrarianism has become established with universal sway.

It is a novelty, and a vicious one, too, to see colleges founded by authority upon avowed grounds of exclusion and partizanship ; as Botanic, Homeopathic, and the like. Of course, I would not advocate any restriction upon the right to instruct and educate in any tenets whatever ; I simply object to the name and influence of the state being lent to aid such purposes. Still more seriously do I protest against their being fostered by endowment.

An effort—not yet successful, but very likely to prove so, from the respectability of the body which commenced it, and the energy and character engaged in pressing it—has been made to procure from the Legislature of our greatest State, New-York, the endowment, with full collegiate privileges of instruction, examination, and diploma, of all the Hospitals containing a certain large number of beds. This innovation, if it prevail, will work a most important change in our whole system. It would be very apt to become popular and to extend itself widely on every side.

I farther specify as new, the establishment of institutions for the education of female physicians. It is now a “fixed fact” that the profession of Medicine is open, in all its departments, to both sexes. Time will determine whether the female practitioner shall take charge, promiscuously, of all the several branches of our art, or confine herself

to the sphere, by no means a narrow one, of female diseases and calls for assistance. The former course I hold to be incompatible with the reverential reserve which modern civilization has built up between the sexes; in the latter, I really believe, woman will be fitly engaged in the fulfilment of her true mission, and most beneficially employed.

But I must bring these remarks to a close. Before I take my leave of you, however, I will beg your indulgence while I offer you a word of friendly warning and advice. You all know that I am no ascetic; on the contrary, I have been more than once censured, by the grave and wise, for my readiness to allow and approve of all varied forms of rational and temperate enjoyment. I seize the occasion the rather then, to urge upon you, emphatically, during your stay with us, a course of peculiar caution and prudence. You are here, most of you, at a great distance from your natural guardians. You are exposed to extraordinary, nay, as to many of you, unprecedented contingencies of temptation and opportunity—abounding, forcible, diversified and seducing; and the danger of being led astray is proportionally imminent. In this gay city every allurements is presented to the young and pleasure-loving—and who does not love pleasure?—every means of dissipation purposely scattered round you. Here—

“The gates of hell are open night and day—
Smooth the descent, and easy is the way!”

But the headlong and unwary youth who abandons himself to the guidance of his tempters, soon finds their promises of enjoyment, “a mockery, a delusion, and a snare.” While the *pleasures*, eagerly tasted, are fleeting as a breath, and even when at their highest point, mingled with many elements of bitterness and disgust, the *penalties* of sensuality are in most instances promptly exacted; and dejection, remorse, disease, and despair follow soon, and surely, the footsteps of riot and excess. The revel of the night ushers in a day of prostration and suffering; a transient and most deceitful relief is at hand in every thoroughfare, and at every crossing; the flagging pulse beats again with a fuller play, and the dizzy head regains its steadiness under the renewed excitement of the fatal draught, which, while it creates an intenser sensibility, and opens every avenue of desire, extinguishes the power of self-restraint, and lets loose the madman within. This, this is the moment of extreme peril: here has begun the downward course of many a deeply lamented, hopelessly lost son or brother, “quick to learn and wise to know,” in the language of poor Burns;

“Whose thoughtless follies laid him low,
And stained his name!”

May no one among you ever experience how difficult, nay, rather impossible, it is, in this harsh and censorious world, to efface such an early stain ; to rise from such a fall ; to recover ground, so hastily and unworthily lost ; to buy back the vigour of youthful health impaired ; the integrity of a constitution broken, it may be by a single impure indulgence ; and to regain the peace of mind, and cheerful self-approbation, belonging to a good conscience and a character above reproach.

Alas ! there is no Lethe, from which we may procure the fabled waters of happy forgetfulness. We exult to seize with almost insane urgency, the passing hour and devote it to pleasure ; the future we promise to dedicate to wisdom and virtue. But reflect ; the *past* is never entirely past ; nay, it is ever *present* to the tortured memory ; and one dark moment of guilty joy, may overshadow all your disappointed and dreary future. The lapse from social conviviality into debauch and gross intemperance, is so prompt to be made, so difficult to be avoided, that your only safety consists in the stern determination, while thus peculiarly exposed, to abstain, totally, from all such dangerous indulgence. At your age, the cup of Circe is full of devilish fascination and enticement ; to taste it, may be to die. Let me adjure you then, in the strong language of the Oriental teacher : " Avoid it ; pass not by it ; turn from it, and pass away ! "

I delight to think that if I can thus persuade you to pledge yourselves, each to his own constant mind, or all to each other, I shall have shed over your ~~distant~~ homes the sunshine of a genial contentment and well founded security. The father who has gone through the moral perils which now environ his absent son ; the mother and sisters, too pure even to imagine them, but dreading them none the less, for their ignorance and uncertainty, will rejoice with the feeling of assured reliance upon you.

Consider then thoughtfully ; for their dear sakes, and for your own, resolve with decision, and adhere firmly to all good resolutions. So shall you enjoy the present without envenoming the future, or rendering the past a spectre to haunt and torment you ; so shall you build up a reputation, on which trust and confidence will hereafter repose themselves readily and unreservedly ; so shall you in due time rejoin the beloved circle of the domestic fireside, without being doomed to disguise the ravages of debauchery, or hide the festering pangs of remorse ; and so shall you pass along your onward way, prepared for the performance of the highest and most responsible duties of a useful and happy life.

ART. IV.—*Treatment of Hæmorrhoids*: Application of Vienna Paste rendered easier by the hæmorrhoidal clamp of M. Jobert (de Lamballe.) By T. GALLARD, *Interne*.

OF the different methods which have been employed in the treatment of hæmorrhoids, two only have retained a prominent place in practice.* It is a long time since compression has been abandoned; and the ligature, either applied around the whole circumference of the tumor or including only a portion, is now very rarely practiced by surgeons. All know the intolerable pain of these two procedures. The choice remains now between cauterization and excision. Still, of these two general methods, certain procedures ought, in the majority of cases, to be preferred to all others. Thus, complete extirpation of a hæmorrhoidal tumor is no longer practiced. Simple incision is reserved for the cases where there is a small number of hæmorrhoids, and these voluminous and fluctuant; and even then, the danger of hemorrhage, and the propagation of inflammation to the mesenteric, and even to the portal veins, should render the surgeon careful when he resorts to the operation.

Excision with ligation of the divided vessels, performed as M. Jobert (de Lamballe) has advised, (*Gazette Medicale*, 1. 7, pp. 385, 1839,) has not the same inconvenience as total extirpation of the hæmorrhoidal tumor, or even the incision of a single fluctuating hæmorrhoid, for the surgeon only incises the tissue, essentially vascular, which constitutes the hæmorrhoid, having care to tie immediately each vessel which furnishes blood to the arteries or veins. This precaution obviates the danger of secondary hemorrhage; but there remains still inflammation, which of itself, in certain cases, may become very troublesome. M. Jobert has recourse to excision only in exceptional cases, for which cauterization cannot be practised, especially when the hæmorrhoids are high up within the rectum. For it is well known that the introduction of caustic profoundly within the intestine, is apt to give rise to contractions, which may, after a time, necessitate another operation, without mentioning the more immediate accidents which may occur from the inability to circumscribe its local effects. Therefore, although preferring cauteriza-

* We do not for a moment wish to conceal a new method which may arise as a consequence from the discovery of M. Pravaz. It is said that the perchloride of iron has already been successfully injected in varicose veins; and it is thought

tion in the majority of cases, M. Jobert has sometimes recourse to excision, which he performs in the following manner: The patient is made to make bearing-down efforts, which tend to protrude the tumor, which is then drawn as far as possible out of the anus, at the same time that the part within is seized as high as possible with the forceps, crotchet or hook, which enables him to operate on the parts brought thus into view, in the manner above described.

This method has the great advantage of obliterating the vessels, at the same time that it destroys the tumor which called for ablation. The cure is as radical as if extirpation was practiced, and it is not apt to be followed by hemorrhage or phlebitis. In all other cases, M. Jobert prefers cauterization. The question now arises, whether the actual or potential cautery should be employed? And if the latter, what substance should be preferred? The actual cautery, being too powerful in its action, ought to be reserved for certain chronic, fungous vegetating hemorrhoids, which give rise to so abundant a hemorrhage as to endanger the life of the patient. In these cases, the application of the red-hot iron procures immediate advantages in arresting the flow of blood, and modifies in a favorable manner the vitality, not only of the diseased tissues, but of the whole region. A short time since, M. Jobert, having to treat a case of hemorrhoids, which in consequence of their strangulation became gangrenous, applied the iron at a white heat with perfect success. We have seen him employ the actual cautery simultaneously with the Vienna paste in the case of a woman who had chronic hemorrhoids, of the character mentioned above. But, ordinarily, he uses the Vienna paste, which has the advantage of being easily prepared, and admits of being circumscribed to the surfaces with which it is brought in contact. The eschar which it forms, is sufficiently thick to disorganize the diseased tissues, without endangering the deeper seated; and it does not liquify like the *potessa fusa*. The surgeon can control its action better than that of any other caustic; but, however, its application requires several precautions which it is dangerous to neglect. Thus, the part to which the caustic is to be applied, must be retained without the anus, and isolated from the surrounding parts. Although the paste does not tend to spread, the blood which flows during its application, may run on the perineum or buttocks; which renders it, therefore, necessary to protect these parts from such an accident. The tumor must be seized as high as possible with the forceps or hooks, and drawn without the anus, as for excision. It is then confided to assistants, and the margin of the anus is surrounded with charpie and pieces of diachylon. It is not until these preparatory measures have been observed, that the Vienna paste should be applied

Some have even invented grooves, by means of which the paste can be confined to the circumference of the tumor.

With an instrument which he has just invented, to which he has given the name, hæmorrhoidal clamp, M. Jobert can dispense with assistants, hooks, diachylon, charpie, &c. If we figure to ourselves two concave blades of metal, (silver, or alloy,) or ivory, articulated together—(*Fig. 1*)—in such a manner that their concave borders, on approaching each other, describe an ellipse more or less elongated—(*Fig. 2*)—we will have an exact idea of this instrument, as simple as it is ingenious and easy of application. To say that it is executed by M. Charrière, under the direction of M. Jobert, (de Lamballe,) is enough to assure us that it will fulfil all the required conditions.

Fig. 1.

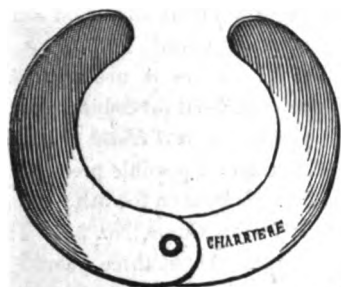


Fig. 2.

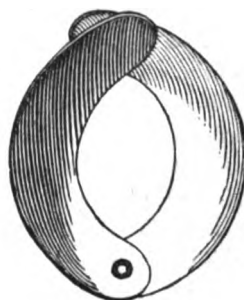
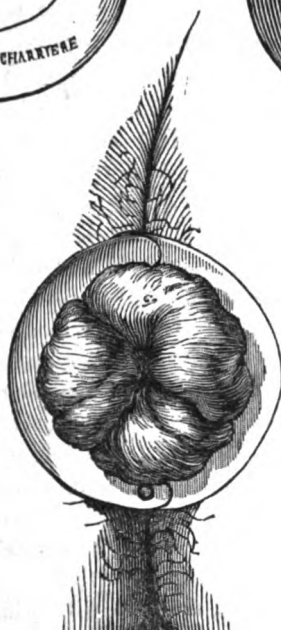


Fig. 3.



When the instrument is closed—the two external borders which form its outer circumference being a little elevated—it resembles, very much, those capsules used in chemical laboratories, and the resemblance is still more striking, when, instead of examining it isolated, it is viewed embracing a hæmorrhoidal tumor—(*fig. 3.*) As it is easy to procure instruments of all dimensions, and as the curvature of the two blades can be augmented or diminished at pleasure, it is possible to embrace tumors of all sizes.

If one, or several pediculated hæmorrhoids—separated from each other—are to be acted upon, a small clamp, less ellipsoidal and almost circular, is adjusted successively, to each, at their base, the blades of the clamps protecting the neighbouring parts while the paste is being applied. If there be but one tumor, or several so agglutinated that they cannot be separated from each other, a larger and more elongated clamp should be used, and the whole mass embraced at once.

But to what purpose are all these details? There is not a surgeon who is not familiar with all these varieties of manual procedures; and a short *resumé* of an operation, recently performed at *l'Hotel Dieu*, by M. Jobert himself, will be more instructive than all possible precepts.

Keller (Léon,) cook, æt 58, entered *l'Hotel Dieu* on the 9th September, 1853, in the service of M. Jobert, ward Saint Côme, No. 33. This man is of the bilio sanguineous temperament. About three years ago, he became affected with non-fluctuant hæmorrhoids, which, forming a tumor, have, for an indefinite time, become so prominent as to give rise to annoyance and pain. Several weeks before his admittance into the hospital, he received a fall, alighting on his buttocks. His tumor which, at the time, gave him no inconvenience, became, the next day, very voluminous, defecation was painful and more difficult than usual, and this state of things continuing longer than in preceding attacks, he decided to enter the hospital. It was ascertained that there existed around the circumference of the anus, a very red and prominent tumor, being, at least, $2\frac{1}{2}$ centimetres elevated above the margin of the orifice—(*fig. 3.*) This mass was formed by three distinct tumors, then adherent, the one with the other. There was no oozing of blood, but a whitish mucus. On the 15th September, the entire tumor was grasped at its base, between the two blades of a hæmorrhoidal clamp. (*fig. 3.*) A layer of Vienna paste was applied to the whole surface included within the grasp of the instrument, and maintained there during $4\frac{1}{2}$ minutes. After having removed the paste, the parts with which it was in contact were well washed with cold water; and the instrument was removed by seizing each blade with a pair of dressing forceps and separating them, as in figure 1. Cold lotions were applied on the following days.

The hæmorrhoidal mass assumed a blackish hue; on the 16th, it was shrunken and diminished in bulk; and on the 18th, portions could be removed with the forceps. Portions of the detached mass, agitated in water and carefully examined, presented no trace of organization. It was a shapeless mass, which, by washing, divided itself into tufts, similar to those which are formed by subjecting the cerebral substance to a jet of water.

On the 19th, another cauterization was performed in the same manner. And, on the 23rd, the eschar was completely detached, leaving only a slight vascular protuberance around the margin of the anus, to which the nitrate of silver was applied, and repeated on the 26th.

The cure was complete. The protuberance remaining does not exceed 4 millimetres in elevation. During the course of the treatment the patient's general condition was very satisfactory.

ART. V.—*Intermittent Fever treated by decoction of Gossypium herbaceum, or cotton-seed.* By PETERFIELD TRENT, M.D., Richmond, Va.

My attention was called, during the fall of 1851, to an article in the American Journal of Pharmacy, taken from the Charleston Medical Journal of May, 1850, in which article a decoction of cotton-seed was reported as having been successfully used in intermittents. The manner of preparation and mode of administration recommended was as follows:—To one pint of cotton-seed, put one quart of cold water and boil them to a pint; one gill of the tea to be given one hour previous to the expected chill. A dose of calomel, followed by oil, was also recommended to be given the preceding day or night.

Case 1st. Sept. 10th—called to see Mrs. H., aged 35 years, of bilious temperament and fine constitution; found her labouring under considerable fever and slight head-ache; was informed that she had a chill about two hours previous to my being sent for, and that this was the fourth chill she had had this week; I also learnt that she had taken quinine, but could not learn the amount she had taken. I ordered 20 grains of calomel to be taken immediately—this was, in six hours, to be followed by 3 ss. of castor oil. Next morning, at six o'clock, she was to take a gill of cotton-seed tea and remain in bed until I saw her, and if about the time the chill was expected she felt cool, she was to repeat the dose. Second visit—found the medicine had operated finely; no return of chill, but some sense of coolness experienced about the usual time the chills came on. My patient complained a good deal of the nausea produced by the tea. I ordered her to repeat the tea next morning, and not to rise until I saw her. Third visit—no return of chill; I did not visit her again.

Case 2d. Sept. 12th—called to see Mr. J. H., an Irishman, day labourer, aged 36 years, of robust constitution; was informed by Mr. H. that he had a chill early that morning, and that he had had one for about two weeks, alternately, daily, and every other day. I found him complaining of his head, and in a perspiration; I directed calomel and oil to be given, as directed in case No 1; he was, also, to take the cotton-seed tea in the like manner. Second visit—found Mr. H. better; a slight return of chill; he remarked my tea was rather hard to take, still the chill did not last so long as it had before I took it. To make the cure certain, I directed the tea to be repeated next day. I did not see him again, but learned that he had gone to work completely cured, and that although exposed to exciting causes, no return of chills had taken place.

Case 3d. Sept. 30th—called to see Mary, child of Mrs. W. aged 7 years; constitution very delicate, and very small for her age; was informed by Mrs. W. that her child had been complaining for some time of feeling chilly, and that she had noticed she was feverish and complained of headache. Upon examination of my little patient, I at once diagnosed the case to be intermittent fever. I prescribed 10 grains of calomel, to be followed in six hours by 3 ss. castor oil. One hour previous to her complaining of being chilly, she was to take half a gill of cotton-seed tea; and, if necessary, to repeat the dose about the usual time the chill came on. Second visit—no return of chill, the child had been made very sick by the tea; I directed a weak infusion of chamomile to be given her; she has had no return of her chills, though otherwise sick since.

Case 4th. Sept. 30th—called to see Reuben N., a cotton factory hand, aged 12 years, consumptive; was informed by Mrs. N. that her son had been complaining of feeling badly, and cold, for some time. That day, previous to my being sent for, he had left the factory and come home; soon after his arrival he was seized with a chill, which was followed by high fever and severe headache; no other pain complained of; a dose of oil had been given him the Sunday previous. I ordered 10 grains of calomel, to be followed by 3 ss. oil; a gill of cotton-seed tea to be taken one hour previous to the expected chill, and to remain in bed until I saw him again. Second visit—no return of chill, and no nausea had followed the use of the tea. To make sure, I ordered the tea to be repeated, in one half the previous dose, the following morning. Did not see him again, but understood he had left for the country, and had no return of chill.

Case 5th. Oct. 10th—sent for to see Rebecca N., sister of R. N., employed in the same factory; was informed by her mother that she had not been well for some time; the week previous to my seeing her she had stopped work, on account of her chills, and that she had given her calomel and quinine with apparent relief. I prescribed 10 grains calomel and 10 grains rhubarb, to be taken immediately. In the morning, to take the cotton-seed tea. Second visit—no return of chill, patient complained a good deal of nausea; I ordered her to remain in bed, and to take some soda water. Third visit—no nausea or return of chill.

REVIEW.

ARTICLE. VI.—*On the Sympathetic Nerve in Reflex Phenomena.* By HENRY F. CAMPBELL, of Georgia. *Transactions of the American Medical Association*, Vol. VI. 1853.

THE special object of the above paper, which we find in the sixth volume of the *Transactions of the American Medical Association*, will be explained by the following extract from the proceedings of the association at its annual meeting in 1853, to be found on page 49.

"Dr. CAMPBELL, of Georgia, submitted a paper, on a question of priority, in reference to the discovery of the reflex relations subsisting between the cerebro-spinal and sympathetic nerves."

"Referred to Committee on Publication."

As this is a direct claim set up to priority of discovery on an important physiological point, we deem it no transgression of literary or professional etiquette, to notice the "caveat" which Dr. Campbell has caused to be embodied in the *Transactions of the American Medical Association*, for the purpose of establishing precedence in relation to the discovery of certain physiological and pathological phenomena pertaining to the sympathetic nerve.

Dr. C. opens his subject by remarking: "In a recent number of the *Gazette Medicale* appear some remarks," (which he gathers from a translation in the *N. Orleans Medical Register*,) "by M. Cle. Bernard, on the reflex actions of the nervous system. In this he refers one order of phenomena to the sympathetic system of nerves, and illustrates, by experiments upon the frog, as well as by reference to many of the acts of nutrition and secretion, that such a relation exists between the cerebro-spinal and ganglionic system of nerves, as well as between the excitor and motory portions of the cerebro-spinal system. Or, in his own words "two kinds of nerves are requisite for a production of these reflex phenomena of organic life; the first transmits the impression to the nervous centres, the second to the viscera. With one order of these nervous filaments is always connected a ganglion of the great sympathetic. Example: the lingual nerve transmits the impression of the taste to the nervous centres; a special nerve then conveys a corresponding excitation to the submaxillary gland; on one of these nerves is situated a ganglion of the sympathetic, the submaxillary ganglion, &c." He gives several illustrations of this fact, and further, appears to be of the opinion that this set of phenomena are *identical* with those occurring in the cerebro-spinal system of nerves, denominated *excito-motory*, by Dr. Marshall Hall; but with this latter part of his paper we have nothing to do. It is only with that portion in which he appears to claim as his own, the suggestion of the theory that there does exist such a reflex relation between the sympathetic and the cerebro-spinal systems, and his presentation of it as an observation entirely new."

As we have not examined the article of M. Bernard, to which Dr. C. refers, we do not know positively what are the extent of his claims.

to originality, in reference to the point in question. One thing, however, we think can be clearly shown; and that is, that neither Dr. Campbell nor M. Bernard have any claims to priority in the discovery or demonstration of the reflex relations between the sympathetic and cerebro spinal systems of nerves.

One or two quotations from Dr. Campbell's "caveat" will suffice to show the nature of his claim to the priority of discovery; the first quotation runs thus: "A reference to a paper presented by us to the Medical Society of Augusta, Georgia, and published in the *Southern Medical and Surgical Journal*, on the Influence of Dentition in producing disease, will show that this subject was fully discussed by us, as early as June, 1850, and the whole argument upon which our theory of the mode in which dentition does produce certain morbid results (diarrhoea for instance) is based upon the supposed existence of such a reflex relation between the cerebro spinal and ganglionic system of nerves, as will be seen by the following: After referring the occurrence of convulsions, during dentition, to the reflex relations existing between certain nerves of the cerebro-spinal system, viz., the fifth pair as excitor, and the muscular branches of the spinal system as motory, we then endeavoured to account for the occurrence of diarrhoea, by establishing the existence of a similar relation between the cerebro-spinal and branches of the ganglionic system supplying the intestinal canal." In illustration, he then gives an extract from his article in the journal above alluded to, and concludes with the following remarks, which we quote entire: "In the above brief quotation, it will be observed that the doctrine of the reflex function between the cerebro-spinal and sympathetic systems is clearly enunciated, and not only is the physiological fact noted, but we there also have surmised the *transmission of permanent irritation*, or of paralysis, from the cerebro-spinal to the sympathetic system, giving rise to nervous aberrations in nutrition and secretion. This opinion we have held for several years, teaching to our classes that there existed between the cerebro-spinal and the ganglionic system of nerves, a relation *similar* to that between the sensitive and motor branches of the cerebro-spinal, and which Marshall Hall terms *excito-motory*; while we have termed that between the cerebro-spinal and sympathetic systems *excito-secretory*."

"As we have before indicated in this report, we do not feel authorised to lay full claim to the above theory, without further investigation of the subject, but with all due courtesy to that highly distinguished gentleman, we can say that we feel assured that these views are not original with M. Bernard, unless he entertained them previous to June, 1850. There may have been other similar observations, but until the publication of M. Bernard's, we had not noted them elsewhere than in our paper on dentition.

The following remarks, in relation to the functions of the great sympathetic nerve, will be found in the fifth volume of the *American Journal of the Medical Sciences*, (old series,) 1829, at page 215.

"On the Functions of the Great Sympathetic Nerve, and the illustration it affords of the Pathology of Amaurosis. Professor LARGEN-HECK considers the nerves of sense as those of vision, smell, and hear-

ng, have need of another order of nerves which do not possess sensibility of themselves, and are merely an accessory apparatus. Such are the ciliary nerves to the optic, and such the fifth pair to the olfactory nerve; the great sympathetic presides over the functions of vegetative life, but it is directly connected with all the organs of sense, and it is necessary to attribute to it an accessory part in the sensitive functions. *It forms a sort of conductor between vegetable and animal life.* The author applies these views to the pathology of the eye, and especially to amaurotic amblyopia and amaurosis. This malady is often a constitutional affection, which manifests itself sympathetically in the eye as an organic affection. The author demonstrates anatomically how the eye sympathizes with parts more or less remote, through the sympathetic nerve; its accounts for the action of the iris, and refutes the opinion that the excitation is produced by light on the retina, and propagated by the optic nerve to the iris. The dilatation of the pupil by belladonna explains this action of the ciliary nerves, and if fomentations or gargle be employed, the effects depend upon the great sympathetic and its irradiations in the iris. The nerves of the retina and optic nerve are of no importance in the motions of the iris, the irradiations of the great sympathetic nerve, which establish the connection between the eye and other organs more or less remote."

We should be glad to transcribe the whole article, but the above is sufficient for our purpose. The reader can examine it for himself. The article was taken from the *London Medical and Surgical Journal*, for July, 1829, into which it was copied from *Neue Bibliothek. für die Chirurgie, &c.*, tom. IV.

In the same volume of the *American Journal of the Medical Sciences*, at page 429, we find an able and somewhat elaborate review of a work bearing the following title:—*On the Structure, Function and Diseases of the Human Sympathetic Nerve.* By JOHN FRED. LOBSTEIN, Professor of Clinical Medicine and Pathological Anatomy in the Medical Faculty of Strasburg, &c. Paris, 1823. Illustrated with eight plates. pp. 174. 4to."

This work, which was written in Latin, was translated by our countryman, Professor Pancost, and published in Philadelphia in 1831. A notice of the translation will be found in Volume X of the *American Journal of the Medical Sciences*, (old series,) 1832, at page 205.

We shall offer a few extracts from the original works, as given by the reviewer.

The reviewer remarks, in reference to the sympathetic—"M. Lobstein, in common with Gall, Bichat, Broussais, Reil, &c., considers it not improbable that the ganglia which diversify the trunk of the sympathetic nerve, are the laboratories of the vital principle which maintain the tone, strength and energy of the organs over which they preside. The nature of the internal or egredient branches which run to the viscera, he considers yet an 'arcanum.' He rejects the idea of Scarpar, that the ganglia exist only for the mechanical use of the mixture and distribution of the filaments."

"He adduces arguments to prove that there is an *emission* and *retrogradation*, *exit* and *return* of the nervous principle, in the sympathetic as in the cerebral nerves, called the organs of the senses, to-wit, from

the trunk to the branches, and from the branches to the trunk ; with this difference, that in the normal state the impressions which arise in the viscera remain there, but in cases of disease, which he afterwards treats at some length, ascend into the brain, are perceived there, and excite a similar, though obscure, impression in the mind."

Lobstein again remarks :—

"There exists a relation between the sympathetic nerve and par vagum, viz: that one may take upon it the functions of the other ; for it is seen in the inferior vertebrated animals that the par vagum is the more prolific in branches distributed to the intestines, as the sympathetic nerve is less, and it is found that no sympathetic nerve exists in some invertebrated animals, and in which its functions are performed by the par vagum only ; whence it follows that the par vagum should be enumerated by the same law as the sympathetic, with the nerves of vegetative life."

The reflex action of the sympathetic is plainly set forth in the following remark of Lobstein, which, in connection with the last quotation, covers the entire ground in dispute.

"The sympathetic nerve forms an admirable chain of connection between the principal organs of the human body. *Whatever arises, or is perceived in one organ, is reflected immediately to the rest.*"

We shall make but one more quotation from the review of Lobstein's work, before passing on to other facts, if possible, bearing more directly upon the claim put forth in Dr. Campbell's "caveat."

"He considers the anastomosis of the sympathetic nerve with the fifth pair to account for the gritting of the teeth, and itching of the nose, in the verminous diseases of children. He considers the fascia communicans of Wrisberg, or the nervous loop between the par vagum and the semi-lunar ganglion, at the posterior part of the stomach, as the principal mode in which the connection is maintained, and sympathies exchanged between animal and vegetative, or nutrient life. This connection, which we believe is generally very little understood, he proposes to call the *abdomino-cephalic anastomosis.*"

We shall now give several quotations from *Jackson's Principles of Medicine*, published by Carey & Lea, Philadelphia, 1832. Dr. Jackson, after speaking of the anatomical distribution of the great sympathetic nerve, remarks : "Considerations based on the anatomical structure, lead to the following inferences, as to the functions of the ganglionic system. *a.* It is not independent of the cerebro-spinal system, but *derives its nervous activity from its connection with that system.** *b.* It is connected, throughout its whole extent, by the numerous nervous filaments passing from one ganglion to another, and uniting together in different plexuses. *c.* The organs of the neck, head, thorax, and abdomen, with the genital organs, which receive nervous filaments from this system, are placed in a communion of actions and impressions, which are transmitted from one to the other, and it is thus the principal instrument of the sympathies between those organs. *d.* Supplying the thoracic and abdominal viscera and genital organs, with nerves, and communicating with the cerebro-spinal nervous system, it is the medium of communication be-

* Only another form of expression for the function which Dr. C. calls *excito-secretory*.

tween these organs and the nervous system of relation. e. Supplying the abdominal and thoracic viscera and genital organs with numerous nerves, this system must be the chief agent in maintaining the exercise of their functions. *f.* From the quantity of nerves it distributes to the arteries, the closeness with which these vessels are invested with those nervous filaments, and which are lost in their coats, it must exercise an active agency over their circulation, and in this manner *influence the secretions and their nutrition. g.* The muscles that receive nervous filaments from this system, have this peculiarity, that they act without volition, or even consciousness. They must, consequently, receive the nervous stimulation for this purpose, from the ganglionic system."

Dr. Jackson remarks again :

"The connection between the *cerebro-spinal organs* and the *mucus tissue of the stomach and small intestines*, by which the action of the one is transmitted to the other, is most probably *effected through the nerves of the ganglionic system*, and the inoculation of the solar plexus with the *par vagum*."

"The irritations and morbid condition of the stomach exercise an active influence over all the organs embraced under the dominion of the ganglionic system, and the morbid condition of any of these viscera are *reflected* on the stomach. There is thus established a reciprocal sympathetic influence between the viscera and organic life, or those over which preside the ganglionic system of nerves."

After speaking of certain pathological actions, Dr. Jackson remarks :

"The above pathological phenomena exhibit : first, a close connection between the stomach and the brain, by which they mutually reflect their irritations on each other; and second, that the different organs to which the ganglionic system sends nerves, possesses a free communion in their actions, which is most extensive and active between those organs most abundantly supplied with nerves. The ganglionic system, from these facts, would appear to be the medium of the sympathies that bind together the viscera of the splanchnic cavities—the cranium, thorax, abdomen, and the genital organs."

One or two more quotations from the Principles of Medicine, will suffice. Dr. Jackson remarks :

"It was announced precedently that in the cerebro-spinal apparatus of the functions of relation, a central point existed, located in the medulla oblongata. To this point all the nerves of the apparatus are directed; towards it are transmitted all the impressions received by their expansions, or are excited in the cerebral structure, and from it are reflected again, as from a radiating focus, the movements of the nervous stimulation excited in the various organs with which it has relation.

"A similar central point seems to exist in the ganglionic apparatus or organic nervous system. This centre is the semilunar ganglion and solar plexus, seated in the dorsal portion of the epigastric region, and distributing nerves to the stomach, liver, spleen, small and large intestines, kidneys, and spermatic vessels, and diaphragm. All the impressions which reach the internal surfaces, all the excitations developed in them, and any of the viscera, are repeated in it, and

reflected into the other viscera, with which it is in communication. This centralization and redistribution of impressions and actions place the viscera in the closest relations with each other, make the impressions and modifications of actions induced in one, common to many, and constitute the ganglionic apparatus, the material organ of the sympathies, of which the semilunar ganglion and solar plexus, as possessing the most extensive connections, are the principal, central and governing portion."

"The communication formed by the par vagum or pneumogastric, between the centres—the medulla oblongata and semilunar ganglion—establishes the intimate relation and immediate connexion uniting the two apparatuses of nervous organs—the cerebro-spinal and ganglionic or organic. By this connexion impressions are *mutually reflected from the one apparatus into the other*; and consequently, the impressions of the viscera, especially those of the abdomen, which have no direct communication with the brain, reach that organ, *while those viscera experience, themselves, modifications from the influence of cerebral excitement.*"

"An irritation or stimulation, the excitation of the organic actions, awakes the activity of the transmitting faculty of the nervous tissue, and is conveyed and repeated in the nervous centres, distributing their mode of existence, and consequently, *through them is reflected into the other organs or tissues, with which these are in communication.*"

Were it necessary, we could go further and quote from Carpenter, Tyler, Smith, Paget, &c. and also cite the experiments of MM. Budge and Waller, absolutely *tracing the Origin of the sympathetic nerve into the spinal marrow.* (Archives Generales de Medicine, Sept. 1852.) We could also refer to a Monograph on the Phrasic Nerve and its Distribution, by Dr. Hubert Luschka, of the University of Tubingen, a notice of which may be found in the 24th number of these British and Foreign Med. Chir. Review. Although some of the experiments have been published since 1850, yet they are but confirmatory of the physiological and pathological views long since entertained and publicly taught by physiologists, both in this country and in Europe.

May we not appropriately adopt Dr. Campbell's own language, as used in his "caveat," and say: "In the above brief quotations, it will be observed that the doctrine of the reflex function between the cerebro-spinal and sympathetic systems is plainly enunciated, and not only is the physiological fact noted, but we there also have surmised the *transmission of permanent* irritation, or of paralysis from the cerebro-spinal to the sympathetic system, giving rise to various aberrations in nutrition and secretion."

While we cannot refrain from expressing our regret that Dr. Campbell should have so hastily, and upon such slight grounds, set up a claim of priority upon the point in question, yet we must admire the willingness with which he seems ready to admit that "there may have been other similar observations," and his announcement, that he does not feel authorized to lay full claim to the above theory, without further investigation of the subject." Upon further investigation, we feel assured he cannot fail to see his error, and will withdraw his claim to priority of discovery.

BIBLIOGRAPHICAL NOTICE.

ART. VII.—*Practical Observations on Aural Surgery, and the Nature and Treatment of Diseases of the Ear.* By WILLIAM R. WILDE, Surgeon to St. Mark's Hospital, &c., &c. Philadelphia: Blanchard and Lea. 1853. 8vo. pp. 475. (Through Mr. John Russell.)

THIS is a practical treatise, addressed wholly to the profession; giving the history, symptoms, causes, modes of treatment, and results of the most frequent and remarkable diseases of the ear. The author has laboured, as he expresses in his preface, "to expose error and establish truth; to lay down just principles for an accurate diagnosis of diseases of the ear; to rescue their treatment from empiricism, and found it upon the well established laws of modern pathology, practical surgery, and reasonable therapeutics."

In an art so long monopolized by empirics, many errors and abuses must necessarily exist; and had the author confined himself to the correction of these, he would have deserved well of the profession. But he attacks alike all who differ from him in practice or opinion, assailing them with coarse derision and unjustifiable abuse. In his introductory chapter he has chronologically reviewed all who have contributed to this department of surgery, and none has escaped his censorious criticism. Each is dragged forth as a victim, to receive a sentence of condemnation as suits his mood or fancy. Were this all, we could pardon Mr. Wilde, in consideration of the labour he has taken in chronicling so many interesting cases which presented themselves at St. Mark's Hospital, during the period of ten years. His registry looks imposing on paper, being a record of 200 cases selected from 2,385, with all the particulars neatly noted. But Mr. Wilde has done worse than ridiculing and defaming others. He has altered their words and distorted their meaning. In a recent number of the *Lancet*, there appeared an exposure of "Wilde's Aural Surgery;" where no less than twelve passages are pointed out, where Mr. Wilde has mis-quoted from a single work, ("The Nature and Treatment of Diseases of the Ear, By Dr. W. Kramer, of Berlin,") rendering the meaning the reverse, intended by their author. How many other misrepresentations this volume contains, we are unable to say; but when we find an author guilty of intentional misquotation in one instance, we cannot but infer that he will do so again, whenever it suits his purpose; and in condemning Mr. Wilde's acrimonious expressions and uncourteous behaviour, we must reject his testimony where it is unsupported by authority.

In the summer of 1851, the writer of this notice attended several of Mr. Wilde's lectures, and he was not favourably impressed with the man or his manner. Mr. Wilde's examination of cases were hastily made, and from his extraordinary behaviour, his patients were so terrified, that little could be gleaned from the previous history of their cases. If the short-hand writer—from whose notes Mr. Wilde asserts to have compiled his work—had given to the world these remarkable lectures, their author would be denounced in stronger terms than we have here used.

In these remarks, we have confined ourselves to a simple statement of facts, without commenting on what we were forced to condemn. As regards the merits of the work; it is systematically arranged, and would have impressed us favourably, if the author had confined himself to the "establishment of truth," and the exposition of his own peculiar views and opinions. Although, for reasons given, not a safe guide, this treatise is not likely to do much harm, for the author has not invented or introduced any new remedy or cure for deafness, but has contented himself with applying the rules of practice, recognized in the treatment of other diseases, to aural affections. He enters minutely into the mode of conducting an examination, and details the physical signs indicative of diseases, with a zeal which makes us lament still more the wrong purpose to which his talents have been sometimes applied.

ABSTRACTS

FROM FOREIGN AND DOMESTIC JOURNALS.

PRACTICAL MEDICINE.

Creasote Inhalation in Phthisis and Bronchitis. By THOMAS INMAN, M.D. (London Lancet.) SIR: Will you permit me to call the attention of the profession to a plan of treatment which I have found of great benefit in many diseases of the chest. I believe that it has been tried before, but I cannot at present lay my hand upon any account respecting it. I allude to the inhalation of the vapour of creasote combined with steam.

The first case in which I employed it was one of phthisis, in which the cough was peculiarly distressing, in consequence of the presence of necrosis and suppuration in both ears, which were attended with severe and constant throbbing pain. A host of opiates were tried, one after another, but had to be given up, from the sickness and headache they produced. I tried creasote inhalation on speculation; and it answered so remarkably well, that its use was continued to within a few days of death. The patient always expressed himself as very grateful for the relief it gave to the cough.

Encouraged by the success attending this, I determined to try it in other cases, some of chronic bronchitis, the majority of phthisis. In all I have found it most useful in allaying cough, and checking secretion and expectoration. By allaying the cough, and thus increasing the comfort of the patient, it seems in some degree to increase the strength; at any rate, it saves its diminution. It does not of itself appear to possess any curative power. As a palliative, it is useful in all cases; and I

have heard it spoken of most gratefully by a patient who only lived to use it for three or four days.

The mode of using it is very simple. I direct from four to ten drops of creasote to be placed in an old teapot, and a small quantity of boiling water to be poured over it. The spout is to be protected by a piece of flannel, and the steam inhaled through that until it begins to feel cool. Care must be taken, of course, not to put too much water in. This answers well enough in poor practice. In hospital and private practice more elegant apparatus may be used. An inhaler made by Weiss answers remarkably well. It consists of a closed tin can, holding about three pints; at the top are two large tubes which communicate with the interior; one is furnished with an ivory mouth-piece, to prevent the lips being burned by the hot metal; the other is turned upwards, and through this the creasote and water are readily introduced. The size of the tubes makes the inhalation of steam peculiarly easy. When this is employed I find that the temperature of the water should not exceed one hundred and fifty degrees. The immediate effects are a feeling of warmth about the throat, and a sensation "as if you had lungs under your ribs," followed by reduced irritability of the mucous membrane, and subsequently by evidence of the creasote having entered the blood.

It would take up too much space were I to detail all the cases in which I have employed it in hospital and private practice; one will suffice, and will serve to indicate the routine treatment I have adopted in phthisis.

A. B., seaman, aged 30, was admitted into the Northern Hospital with severe cough, dyspnoea, and profuse expectoration. He had been ill for three months, during which he had emaciated considerably. The pulse was one hundred and thirty, and there were copious night sweats. On examining the chest, I found solidification of the apex of the left lung, and coarse mucous rales all over the same side, both anteriorly and posteriorly. At the apex of the right lung there was tubular breathing in one spot, and prolonged expiration in another. The expectoration was of thick, semi-transparent mucus, free from air bubbles, and mixed here and there with grey tubercular patches. He was ordered to have creasote inhalation three or four times, and to take internally half an ounce of cod-liver oil, with twenty-five minims of tincture of iron, three times daily. The severity of the symptoms abated in the course of the week, and in six weeks the man was convalescent. The right lung now seems perfectly healthy; all traces of solidification have disappeared from the apex of the left; the respiration there, and over the whole of that lung, has become natural. The pulse has come steadily down to seventy-two. The man has gained flesh, and the capacity of the chest, as evidenced by the power to take in a deep inspiration, has signally increased.

I may mention, that the use of iron has been adopted in consequence of the effect it had on a little girl, who had been previously employing creasote inhalation and cod-liver oil for some weeks without any indication of improvement. In her case, the whole of the left, and the upper part of the right lung were implicated; but I could not detect

any cavities. The case seemed hopeless, when she began with the tincture of the sesquichloride, in addition to her other medicine. In two days an improvement was seen; in four it was confirmed; and, in less than six weeks after, she was running about the wards rosy and well, and has continued so up to the present time—about two months more.

Etheric Solution of Balsam of Tolu, in Chronic Bronchial Catarrh. By Dr. BOZIERE. (Medical Times and Gazette.) The author employs, in cases of bronchorrhœa and obstinate cough, also in cases of aphonia, from continued talking and singing, as well as in other chronic affections of the chest, æther. sulphur. 60 grmm.; bals. toluatan. pur. 20 grmm. He puts the mixture into a suitable vessel and makes the patient inhale the vapour every half hour for two or three minutes.

Strychnia in Lead Cholic. (New-York Medical Times, Aug. 1853.) Dr. SWEET called the attention of the New-York Medical and Surgical Society (Sept. 18, 1852) to a point in practice, which he brought before their notice a year or two ago—the use of strychnia in lead cholic, in moderate doses, say the sixteenth part of a grain, three times a day. This has become the settled practice in the New York Hospital. Relief is usually experienced in forty-eight hours; the bowels act, and the disease subsides. He recalled to mind one case, however, which went four days before relief was afforded. He also related the case of a young Englishman, a clerk in a drug store, who was admitted to the hospital a few weeks ago, with what was at first considered as ordinary cholic. After a time, however, the following facts were elicited. It appeared that, upon first opening the store in the morning, he had been in the habit of taking a glass of soda-water, which had remained over night in the lead pipe connected with the fountain. Strychnia was used in this case with great success. The doctor stated that, under the use of this drug, he had noticed twitching of the abdominal muscles before a passage from the bowels. He thinks that the disease is consequent upon paralysis of the intestines, and that strychnia, by acting upon the nerves, relieves it.

Dr. S. mentioned a second case which had occurred this summer, from drinking soda-water early in the morning, which had become impregnated with the lead poison by standing in the fountain over night. Also, an obscure case of this disease, now under treatment, in which the patient had suffered for seven or eight years. He was relieved by the use of strychnia, and is rapidly improving.

Dr. Bulkley remarked that he had employed the strychnia treatment in colica pictonum, and stated that since the first of August there had been five cases of the disease successfully treated with that remedy during his attendance in the New-York Hospital. He remarked, as a curious fact, that in ordinary paralytic cases the exhibition of strychnia will not affect the bowels.

Soothing Liniment—proposed by Dr. T. DOUGLASS, of Alexandria, Burke Co., Ga. (Southern Medical and Surgical Journal.) I beg leave

to present you, for examination and trial, a small specimen of a saponaceous compound which, on account of its very prompt and soothing qualities, I have honoured with the name of *Letheon Liniment*.

It is made by digesting a bar of fresh turpentine soap and four ounces of gum of camphor in a gallon of good alcohol for two weeks in the heat of the sun. It is then bottled up while hot, and one drachm of chloroform added to every four ounces, set in a cool place, and shaken occasionally while coagulating.

The turpentine affords the best means, in my opinion, of applying chloroform to the skin, because, by its adhesiveness it holds that volatile fluid longer and more firmly in contact with the surface than any other substance could do.

My mode of applying it, is to coat the part well with the liniment, and cover it immediately with a piece of good paper, which adheres firmly and produces a gentle burning, tingling sensation, which, in neuralgia, rheumatism, irritability of the stomach, cramp, colic, &c., is perfectly delightful.

Did I think more of money than of honour and humanity, I might possibly make this compound avail me something. But since I do not wish to become a competitor with all the host of pain-killers and extractors and eradicators in the country, you may, if you think it worthy of a trial by our noble profession, hand it over to Professor Dugas for publication in the Southern Medical and Surgical Journal.

Eighteen cases of Typhus Fever, treated by the free exhibition of Brandy, etc. By Dr. TODD. (Medical Times and Gazette.) We have recently watched, with great interest, a series of severe cases of typhus fever, under the care of Dr. Todd, in this hospital, in which an almost uniform plan of treatment, by means of the very free exhibition of stimulants, more especially brandy, has been resorted to with great success. Reflecting instructively, as these cases do, on one of the most important questions in the whole range of practical medicine, we hasten to bring their chief features before the attention of our readers. The series consists of eighteen cases; and as we cannot, of course, find space for the details of the whole, we shall content ourselves by giving a brief synopsis of them. The whole having occurred within the last few months, and several of them within a few weeks, they present, we believe, fair specimens of the form of fever lately and still prevalent in the Metropolis. They do not, however, comprise all which have been under Dr. Todd's care during the time referred to, but only those of well-marked typhus type, and which agreed in presenting the following symptoms previous to the commencement of the treatment: A copious eruption of scattered measles-like spots (mulberry or typhus rash;) bowels either confined or but slightly relaxed; great prostration of strength; delirium (in six cases, coma was present;) a small and very rapid pulse. It may be well to premise, that they were treated as is done in almost all general hospitals in the open wards, their beds being purposely arranged so as to occur at some distance from each other, in order to prevent the accumulation of contagious emanations. The treatment pursued consisted in administering, either every hour or every half-hour,

day and night, from half an ounce to an ounce of brandy, with a draught every second hour, containing *sp. æth. chlorici mx., ammoniæ carbonatis gr. v., aq. pur. 3j.* The patients were induced to drink as much strong beef tea as possible; the head was always shaved; and, in most, a blister was applied to the scalp.

Out of the whole eighteen cases, but one terminated fatally. The subject of it was very violently delirious on the day of her admission, and no account of her previous symptoms could be obtained. Death occurred on the third day afterwards. On making the autopsy, the brain was found to be slightly congested, and the grey matter was of a darker colour than usual. Peyer's patches in the small intestines were enlarged and very distinct, but not ulcerated. The spleen was enlarged, full of blood, and very soft; but all the other organs appeared to be in a normal condition.

Excluding, then, this fatal case, we will now examine the condition of the circulation in the remaining seventeen, more especially with regard to the influence of the treatment upon it. On the day that the administration of brandy, etc., was commenced, the pulse had, in five cases, a frequency of 136 per minute; in three, of 126; in seven, of from 120 to 126; and in one, of 116. After the measures above specified had been pursued for four days, the pulse had, in eight cases, fallen to 92; in five others, it had fallen below 92 on the fifth day; and, in the remaining four, to below 90 on the sixth. Again, taking the day on which treatment was commenced as our starting point, the skin, previously hot and dry, relaxed, and became moist and perspirable, on the fifth day, in nine cases; on the sixth day, in five cases; on the twelfth day, in one case; and in the remaining two the date of this crisis was not recorded.

The degree of success exhibited by the above facts is, we suspect, very considerably beyond that usually obtained in cases of so severe a type as those under consideration, and is very encouraging to a pursuance of a similar plan of treatment in future. That the success did really depend on the treatment, appeared to be conclusively evidenced in several cases, in which the pulse, progressively increasing in frequency up to the time that the brandy was ordered, steadily fell from that day forwards. The relapses of one or two, in consequence of the accidentally inefficient administration of the remedy, also afford important support to the same conclusion. In respect to the numerical age of the fever at which the brandy treatment was commenced, it varied so much in the different cases, that there does not appear to be any practical advantage in attempting to state it. In all, however, the first stage had passed, and low "typhus" symptoms had become fully developed. Dr. Todd is continuing the same plan of treatment on the fever patients now under his care, and hitherto with very pleasing results.

Two cases of Traumatic Tetanus treated by Ice. By B. D. CARPENTER, M.D., Suffolk County, Long Island. (*New-York Medical Times.*)—*Case 1.* August 22d, 1849—E. G., aged 16 years, of good constitution and habits, jumped from a fence on the stump of a twig some half inch in diameter, which made a wound in the ball of the right foot three-quarters of an inch deep. Twelve days after the

accident he complained of feeling lame and stiff during the night was awaked by a violent spasm; the next day complained of stiffness and soreness of the muscles of the neck and throat, and pain at the scrobiculus cordis; the following night, during sleep, was seized with spasm; and the next morning when I was sent for, I found him complaining of pain in the above region, great rigidity of the whole muscular system attended with difficulty in swallowing and constraint in moving the head and jaws, and in articulating. During the spasm, the body was curved backwards and thrown to one side, the dyspnoea was considerable, pulse full and slightly accelerated, skin warm and moist, bowels costive, urine scanty and high-coloured.

Administered a purgative, which was assisted by enemas. The patient was then put upon the free use of opium in the shape of Dover's Powder, and the bowels kept open by the use of cathartics and injections of 31 tincture assafoetida in half a pint of soap suds, repeated as often as the preceding one came away. This treatment was continued for four days, during which time he gradually grew worse. The tetanic rigidity and spasm increased until the sixth day; when, finding that he must die unless something farther could be done to allay the pain and extreme spasm, and viewing the difficulty as being an irritation of the spine, perhaps connected with congestion of the membrane covering the spinal marrow, I determined to apply ice to the head and the whole length of the spinal column, since the whole muscular system was affected. I did so, and in ten minutes had the satisfaction of seeing the pulse come down from 110 to 75, and all the urgent symptoms relieved; the rigidity was gone, and he had but one spasm after the ice was applied; his bowels were kept open, and assafoetida injections were continued twice a day, to allay the irritability of the nervous system, manifested by slight twitchings. No medicines were given by the mouth. The wound entirely healed, and in three days the patient was discharged cured; and his health since has been as perfect as before the attack.

Case 2. Aug. 11th, 1853—A. C. 21 years of age, a robust farmer, in good health, in assisting to remove some old lumber, stepped on the point of a rusty nail, which entered the hollow of the foot to the depth of three-quarters of an inch. The wound was not very sore, and was dressed with some simples by himself; and he remained at work moderately until the 16th, five days after the accident, when he complained in the afternoon of twitching in that foot and slight pain in the region of the wound and leg of that side. Was quiet the rest of the day, and retired early to bed, but slept none from restlessness, anxiety, and slight pains and twitching of the nervous system. On the 17th, felt some pain in the head and through from the lower end of the sternum to the back. I saw him at 6 P. M., and found him complaining of pain as above mentioned, which had gradually increased at the sternum, great rigidity of the muscles of the left side of the neck, accompanied with slight dyspnoea and some difficulty in swallowing. Even at this time there was present the peculiar expression of countenance found in tetanus. Pulse 100 and hard, bowels costive—had eaten nothing—the wound has not commenced to heal, and was covered slightly with a thin

serous discharge. Made a free incision into the wound, and dressed it with bread and milk poultice, to which tinct. opii. was added; ordered 10 grs. of calomel with 10 of rhœi, to be followed by pil. colocynth. comp. until the bowels were freely moved, and enema of tincture of assafetida, 3j. every three hours, or as often as the preceding one should be voided, large doses of Dover's Powder by the mouth, and to have the neck bathed in camphorated oil and tinct. opii. 18th, 7 A. M., found that the bowels had been freely moved, and that spasm of the whole muscular system had commenced. About 3 A. M., pain in the neck and at the sternum increased, and there was great rigidity of the muscular system generally; dyspnoea great, much difficulty in swallowing and articulation, jaws partially closed, entirely so during the spasm, pulse 120; indeed, all the symptoms increased in a marked degree, with slight delirium. Ordered quarter of a grain of morphine every hour, and to continue the assafetida injections. 6 P. M., all the symptoms greatly aggravated, pulse so small and frequent that it could not be counted, jaws closed, breathing extremely difficult, body almost constantly drawn backwards or forwards and to one side, face pale, skin moistened with clammy sweat, and perfect rigidity of muscular system. Had slept none for 48 hours. Applied ice to the head and whole length of spinal column; in 20 minutes the pulse came down to 100, the skin was covered with profuse perspiration, the muscular system relaxed; in short there was a perfect yielding of all the urgent symptoms, and the patient slept soundly and pleasantly for the succeeding two hours, during which time the breathing was natural, and there was neither tetanic rigidity nor spasm. When he awoke, there was still some delirium, the pain in the region of the sternum was very great, and for half an hour the tetanic rigidity and spasm were considerable. The ice was again applied, when the symptoms immediately yielded, and the patient (with the exception of short intervals) slept quietly the balance of the night.

17th, 6 A. M., the bowels were moved by the assafetida injections, the delirium had passed off, all the tetanic rigidity was gone. Pulse 80, breathing natural, but said there was great soreness of the chest and all the muscles of the body. Drank some soup, continued the ice and injections as before. 11 A. M., there was some slight twitching of the muscles, without rigidity; from this time the patient continued to improve without either tetanic rigidity or spasm, until, on the 25th, he was discharged cured, with the wound nearly healed.

The ice was applied from 10 to 30 minutes each time, with intervals of from 2 to 8 hours.

SURGERY.

New Instrument for Injecting the Perchloride of Iron in Cases of Nævus, etc. (Medical Times and Gazette.)—During the last two months we have witnessed several trials of the acid solution of the perchloride of iron, as an injection for producing the coagulation of the blood in nævus, etc. In more than one instance, very severe inflammation

of the part has followed its use, and in one, a *nævus* on the scalp, sloughing, and even exfoliation of a portion of bone resulted. There appeared, however, some reason to think, that the powerful agent had been too freely used, and that, had a much smaller quantity been injected, the promises made by its introducers, might possibly have been borne out. Other objections also applied to the method and instruments in ordinary use; they required that an opening should be made for the introduction of the syringe: and the flow of blood produced by the puncture, to say nothing of the smallness of the aperture, sometimes rendered the latter difficult of accomplishment. We have been shown, during the past week, an ingenious little instrument contrived by Mr. Ferguson, of Giltspur-street, which looks likely to aid very materially the efficient accomplishment of this procedure. It consists of a very small glass syringe, the point of which terminates in a fine platinum tube. This tube is encased in another one, about a quarter of an inch longer than the first, ending in a sharp trocar-like point, and having, near its extremity, an oblique opening in one side. By rotating the outer tube on the contained one, their apertures may be made to correspond or otherwise, at the will of the operator. Thus, then, the necessity for two instruments is quite done away with. The syringe having been charged, the operator rotates the outer tube, so as to conceal the orifice of the inner one entirely, and protect it from the ingress of blood. In this state the instrument is passed into the centre of the tumour, and, having been stirred about as much as may be thought desirable, the tube is turned back, so as to expose the orifice; and the piston being at the same moment depressed, a drop or two of the solution is squeezed out. It may be supposed, that the smallness of the whole instrument, and the diminutive size and oblique position of its aperture, will afford considerable guarantee against the injection of too large a quantity, while on its advantages, in ease of employment, it is scarcely necessary to insist.

Upon the Treatment of Vesico-Vaginal Fistula. By Dr. BERTHEZ. (From L'Union.) If the fistula be of moderate size, the author is convinced, that its closure may be brought about by cauterization. He has had, during the last ten years, six opportunities of bringing this method into practice, and of witnessing quick and permanent cicatrization of the wound. Before proceeding to cauterization, he clears out the bowels by mild medicines, and the bladder by the use of the catheter. He then introduces a thick, elastic canula into the urethra. The patient is put in the position as for the operation of lithotomy, and a speculum, composed of separate blades, is introduced into the vagina, so as to bring the fistulous aperture into view. The bladder is next

at their base to a right angle with the handle of an instrument, of such size as to be readily introduced within the interior of the speculum. If the first application does not suffice, it should be repeated after fourteen days. The granulations are often so abundant, that they require to be kept down by the use of the nitrate of silver. That the case should go on well, a fixed and non-stimulating diet must be directed. The patient must be kept in that position which prevents the urine from making its way to the abnormal opening; little fluid should be drunk, that the secretion from the kidneys may be inconsiderable; and a plug of wadding should be introduced into the vagina, and renewed twice or thrice a day. Cicatrization usually is complete in five or six weeks. If the opening is very large, cauterization has no effect; and the author then recommends the autoplatic operation of Jobert.

Spermatorrhœa Rings. (Boston Medical and Surgical Journal.) Quite a revolution has been effected in New England, in less than a year, by the use of a mechanical invention, instead of medication, in the treatment of a formidable malady. Heretofore, tonics, accompanied by a long series of auxiliary assistants, such as jaunts on horseback, sea bathing, a regular diet, besides innumerable preparations of drugs, have been prescribed to arrest the effects of spermatorrhœa, but rarely with any permanent success. It is a condition resulting, in most instances, from the indulgence of a pernicious vice. In prisons, self-pollution is nearly universal, and no ingenuity on the part of wardens or attending physicians has prevented it for any length of time. Some suffer intensely, and even die, from excessive indulgence in this vice. Schools, too, and colleges, are often the nurseries of this degrading habit, which carries many young men to an early grave, often without the true cause being suspected. The weak eyes and continued headaches so common among students at public institutions might in many instances be traced, if effort was made in the right direction, to this perpetual violation of a physiological law. The rings, which this journal was the first to announce, are a sure remedy for involuntary forms of the disease. Physicians are eminently successful with them. In the state prison at Charlestown, where Dr. Bemis has given them a thorough trial, we understand they have performed many cures. In private practice, also, testimony from the most reliable sources might be cited to strengthen the medical public's confidence in this simple and only effectual relief in these cases. Dr. Cheever has shown us another improvement of the instrument. It is far lighter than the former patterns, and the middle ring is better balanced in the centre of the large one. The simplicity of the adjustment to any sized organ makes it

Gazette.)—Chloroform having been administered, the boy was tied in the ordinary lithotomy position, a curved staff, with a rather deeper and wider groove than usual, was introduced into the bladder, and the stone was again distinctly felt. The staff being taken charge of by Mr. Paget, and held in the usual position, Mr. Lloyd next introduced into the rectum a metallic *speculum ani*, which as it differed somewhat from those in general use, we here figure. Its peculiarity consists in its having a constricted neck, by which its retention within the contracted sphincter is rendered easy. This instrument having been introduced with its aperture turned upwards to the front commissure of the gut, its handle was given in charge to an assistant, and Mr. Lloyd next proceeded to the incisions. The angle formed by the junction of the perineum and rectum was transfixed and divided with a narrow-bladed scalpel, the knife being pushed with its edge directly downwards into the median line of the perineum, about half an inch from the verge of the anus, allowed to enter the front wall of the rectum, at about three quarters of an inch up the gut, and then, by a single sweep, made to cut its way out downwards into the cavity of the speculum. The next step consisted in dissecting upwards and inwards to the membranous urethra, the operator keeping his left fore-finger in the wound, and feeling for the staff. Having found the groove in the latter, the finger was placed, in order to serve as a guide to the knife, on the anterior border of the prostate, at which point the knife was made to enter the groove, and by cutting *forwards*, division of the whole length of the membranous urethra was accomplished. A pair of small straight polypus forceps was next passed along the groove in the staff, into the neck of the bladder, and the dilatation of the prostatic urethra was accomplished by gently opening their blades, and then closing and re-opening them several times. During this process, which occupied perhaps nearly a minute, several gushes of urine escaped. The dilatation having been effected to such an extent that the fore-finger could be very readily passed, Mr. Lloyd withdrew the staff, leaving his finger as a guide, on which a pair of slightly curved extracting forceps were immediately introduced. The stone was at once found, but apparently, from its occupying the upper and front part of the bladder, it several times eluded the grasp of the forceps, but when once firmly seized, it was extracted with great ease. In order to prevent the roughness of the stone from injuring the important structures in the floor of the prostatic urethra, Mr. Lloyd endeavoured, as much as possible, to hold the blades of the forceps with their convexities directed upwards and downwards, while passing through that part. The calculus proved to be about the size of a small damson plum, being quite smooth on one side, in which its nucleus, (probably lithatic,) was displayed, but very rough on the other, from irregular phosphatic incrustations. Having examined the bladder with his finger, and ascertained that no other concretions were present, Mr. Lloyd introduced through the wound, into the bladder, a short gum elastic canula, and attached it by means of a suture to one edge of the incision. The quantity of blood lost had been unusually small, and the operation had occupied but a short space of time.

PHARMACY.

Tinctura Cinchonæ Ferrata. By SAMUEL SIMES. (American Journal of Pharmacy.) Huxham's Tincture, the official Compound Tincture of Peruvian Bark, cannot be combined with any of the ordinary chalybeates without an offensive decomposition, seriously affecting its apparent, if not its real characteristics. Those reactions were examined at the solicitation of Dr. J. F. Meigs, and a tincture prepared, so modified that it is no longer incompatible with the salts of iron. Sixteen grains of Ammonio Citrate of Iron are dissolved in each fluid ounce of this modified tincture, together constituting what has been denominated, for distinction, "Ferrated Tincture of Bark."

It is readily prepared by digesting in the Edinburgh Compound Tincture of Cinchona, sufficient hydrated sesquioxide of iron to completely eliminate the cincho-tannin, whether pure, oxidized, or combined. One ounce of hydrated sesquioxide, dried at a temperature not exceeding 130° Fahr., usually suffices for one gallon of the tincture. After filtration, the tannate and excess of oxide should be washed with boiling alcohol to remove any trace of alkaloid which may have been in combination with the tannin and precipitated with it. This alcoholic solution may be evaporated to dryness, the product dissolved in a little water acidulated with citric acid, and added to the filter along with the proper quantity of iron salt. It differs little in appearance from the ordinary Huxham's tincture, is exceedingly agreeable, and in teaspoonful doses has become a very energetic invigorative, admirably adapted for administration in those cases of weak and languid habits of children and females, where the body is in a pallid or flaccid state, and very susceptible of fatigue or morbid action.

The ferrated tincture is not solely dependent on the quinia and iron it contains for its value as a curative agent. The grateful, and by no means inefficient adjuvants, the orange peel and snake root, and the other proximate principles of cinchona, independent of quinia, are by no means to be overlooked, and cannot be replaced by salts of quinia and iron alone, however scientific their artificial combinations may appear.

Fine French brandy, employed as the menstrum, yields a still more grateful preparation, and should be substituted in all cases for the diluted alcohol, when the additional expense is no obstacle.

A quantitative examination of the cinchona alkaloids contained in this tincture, both before and after the elimination of the tannin, was made without appreciable difference in the results. A portion of the tincture as I have supplied it having been delivered to Professor Booth to determine its iron constituent with accuracy, elicited the following statement:

PHILADELPHIA, Aug. 3d, 1853.

DEAR SIR:—The Ferrated Tincture of Bark, which you submitted to me for analysis, yields 4.23 grains of sesquioxide of iron to the fluid ounce. Respectfully yours,

JAS. C. BOOTH.

MR. SAMUEL SIMES, Philadelphia.

The sesquioxide in 100 grains of the ammonio-citrate of iron was accurately determined to be 25.84 grains, consequently 4.23 grains of sesquioxide correspond with 16.37 grains of ammonio-citrate in each fluid ounce of the tincture examined. The slight discrepancy is probably due to some difference in graduated measures.

OBSTETRICS.

Uva Ursi, a substitute for Ergot. By E. G. HARRIS, M.D., of Fayette, Ala. (Southern Medical and Surgical Journal).—I wish to call the attention of the medical profession to Uva Ursi, as a substitute for Ergot, in producing uterine contraction. Whether it will produce all the effects, or answer all the indications of that drug, I am not able to say; but I do know that it will produce considerable uterine contraction when given during labour. Since December last, I have given it in five cases, in all of which it acted more efficiently than ergot. In three of these cases the pains had ceased entirely, from exhaustion of nervous energy. A strong decoction of uva ursi was given every ten minutes, and in thirty minutes the pains had increased considerably, and in from one to one and a half hours, the delivery was effected and the placenta expelled, the uterus contracting well, and no untoward symptoms taking place. In the other two cases the pains had not ceased, but were fast doing so. I gave them bountifully to two of my *new parturients*—one of them was delivered in fifty minutes, the placenta following in less than five; the other in an hour and twenty minutes, the placenta in ten. In all these cases the head occupied the superior strait at the time I commenced giving the medicine, in consequence, no doubt, of inefficient uterine contraction. The os uteri was in good condition in all the cases. Directly after the exhibition of the uva ursi, the pains would become strong and propulsive, lasting about the ordinary length of time; then going completely off, leaving the patient free from pain, except that produced by pressure upon the soft parts: the placenta soon followed; the uterus contracted well, and no hæmorrhage more than ordinary. There was little or no tonic contraction until after the expulsion of the placenta, when it was complete.

I am aware these five cases are not sufficient to establish its reputation as a therapeutic agent in labour, yet the result of the past encourages me to give it a further trial; and should its use in the future prove as successful in the hands of my professional brethren as it has in mine, I

does not produce that tonic contraction which is so painful to the mother, and at times so hazardous to the life of the child, until after the delivery is affected: this is generally far otherwise with ergot. More than once have I seen a young and healthy mother give birth to a well developed but dead infant—not from the poison being absorbed by the mother, and through the circulation destroying the child, but *alone* by the powerful tonic contraction of the uterus compressing the umbilical cord, arresting the circulation, &c. Often have I seen the placenta retained for hours, and in two cases for a day and night after it had been entirely detached from the uterus, by the firm and unyielding grasp of that organ—all brought about by the free administration of ergot.

My plan for giving it is as follows:

R̄. Uva ursi, a good article,	-	-	-	2 oz.
Boiling water,	-	-	-	1 pint.

Pour the water on the leaves in a pitcher or bowl, stir it till it becomes cool enough to drink, and give one-fourth as hot as it can be drunk every ten or fifteen minutes, until it has the desired effect. Attention should always be paid to the condition of the os uteri, dimensions of the pelvis, &c.

Stricture of the Fundus Uteri as a cause of dangerous hæmorrhage.
 Ry Dr. DEIG. (B. and. F. Med. Chirug. Rev.) Under this new title the author alludes to a discription of hæmorrhage that most experienced practitioners are familiar with as arising from partial contraction of the uterus. After the separation of the placenta, and while globular constriction of the uterus can be felt above the pubes, alarming hæmorrhage comes on. As there is a good contraction of the uterus, and pains, often of considerable severity, occur, this is the first inexplicable. On passing the hand through the widely opened os, we enter a tolerably spacious cavity filled with coagula, whose walls are firm and unyielding, and remain quite passive to the pressure of the hand. Pushing on farther, we perceive a more or less narrow, often cartilage-like stricture, separating the fundus from the body of the uterus. It is exactly the same as is observed in strong incarceration of the placenta, only, as a general rule, the contracted part is inclined to one side (usually the right,) and takes on a more oval form.

The means calculated to check hæmorrhage under other circumstances are here useless or mischievous. Increasing the pains by means of ergot, cold, external pressure, friction, &c., does injury by increasing the abnormal contraction, and by favouring the flow of blood from the non-contracted part. Of a more useful character are such means as tend to arrest hæmorrhage without exciting pain, as compression of the aorta (taking care, however, not to compress the fundus uteri,) acids, alum, cinnamon, ipecacuanha, warm astringent injections, &c. The most efficacious means, however, is the attacking the pathological condition, upon which the hæmorrhage depends by the employment of belladonna, in conjunction, or in quick succession, with, ipecacuanha, together with compression of the aorta. When on account of pressing danger, the operation of these means cannot be waited for, the painful, and often difficult procedure, of dilating the strictured part must be resorted to.

allowing the hand to remain within the fundus until the organ contracts in a normal manner.—*Monatsbericht für Geburtskunde*, Band ii., p. 1.

Nympho-Maniacal Hysteria. By M. Sandras. (Va. Med. and Surg. Jour. from Gazette des Hopitaux.) M. Sandras (*Traité prat. des mal. nerv.* T. i. p. 163) defines hysteria "an habitual nervous condition, in which occur, at a longer or shorter interval, paroxysms characterised by a peculiar sensation of strangulation, by oppression of the respiration, more or less acute pain in the head, and by clonic convulsions in all, or nearly all parts of the body." This rapid description, the only definition possible, omits a double phenomenon, which is very common in hysteria, and of which a patient in M. Grissolle's ward at La Pitié, presents a remarkable example. We refer to anæsthesia (absence of tactile sensibility,) and analgesia (absence of sensibility to pain. However rude the pressure exercised upon different portions of her body, she does not perceive it. A feather may be introduced into the nostrils or pharynx without exciting the slightest sensation. A pin may be passed through a fold of the skin without producing pain. This woman is therefore affected with anæsthesia and analgesia, but not in every point; a very circumscribed portion, the septum of the nasal fossæ is still sensible, and it is sufficient to pinch this between the fingers to occasion a convulsive paroxysm.

Every sensation may be decomposed into three elements, which are: the impression, the transmission of the impression, and the perception. The impression and its transmission are not indispensable, inconceivable as this appears at first. Do not those who have been subjected to amputations experience sensations referable to the limbs which they have lost? and are not sensations reduced to perceptions in hallucinations? In the particular case of which we are speaking, in hysterical anæsthesia, and analgesia, which of the three elements of sensation are wanting, or are they all absent?

The patient of M. Grissolle is interesting in another point of view; and here again we have to signalize an omission in M. Sandras' article on hysteria. It does not mention the libidinous, erotic, or nymphomaniacal form of hysteria; a form which is nevertheless common. Not only is M. Sandras silent in regard to the erotic form of hysteria, but he denies that hysterical women "are more desposed than others to take an active part in sexual intercourse; and he adds, that the contrary rather is true. T. i., p. 193.

In M. Grissolle's patient the erotic form is very marked. This woman's first action, when her paroxysms come on, is to seize upon the nearest man, and she moves the pelvis in a voluptuous manner throughout the whole duration of the attack. At the visit, when she is surrounded by the attendants, she becomes excited

M. Grisolles saw, some years ago, a case of hysteria in a very distinguished woman of fashion, thirty-five years of age, the widow of a man, who, having become the victim of satyriasis, subjected her to the rudest assaults. It cannot be doubted that the privation of sexual intercourse, succeeding such great abuse, was the cause of the hysteria in this case, and for this reason. This lady soon married a robust man, and under the influence of marriage the hysteria was dissipated.

Relatively to the treatment of this strange neurosis, the case we present is likely to modify the opinion of M. Sandras, who expresses himself in these terms:—"I have never witnessed a case in which the enjoyment, or even the abuse, of venereal pleasures, have produced a diminution or cure of hysteria." (T. i. p. 193.)

The seat, or rather the starting point of hysteria, is very obscure, and is the subject of much dispute; the majority of physicians and philosophers have placed it in the womb. Vain attempt at localization! Is it not true that hysteria has occurred in a woman without a womb, and also in men.

M. Grisolles has seen one example of hysteria in a woman in whom the uterus was absent. The anatomical fact was established by M. Chassaignac and himself. It was easy to be satisfied in regard to it by introducing a sound into the bladder and a finger into the rectum. On either side, could be felt two small bodies supposed to be ovaries. The hymen was intact, and behind it, instead of a vagina, there existed an opening of scarcely more than a line in depth. The external genital organs were normal. The mammae very large. Strangely enough, hysteria assumed the strongly characterized erotic form in this unfortunate, who (à cause de son extrême lubricité), was greatly sought after by the nurses of the hospital. Is it not clear, after this clause alone, that the cause of hysteria is not in the uterus, neither in venereal appetite?

Where then does it reside? We say simply, without entering into considerations which would carry us too far, that it varies, probably, in different cases; but, that wherever it may be, the morbid action, even if eccentric, inevitably rebounds upon the nervous centres, and from these irradiate the influences which produce the multiplied neuropathic phenomena which constitute the fantastic symptomatology of hysteria.

As to venereal desire, it arises in the central nervous system, in a point which we do not now seek to determine (apparently in the cerebellum), but which is affected, on the one hand, by the excitement of the imagination, and, on the other, by physiological or morbid excitement of the external organs of generation.

Is hysteria diathetic, that is to say, connected with a general condition of the economy, or does it consist in an organopathy limited to the nervous system? This is still a problem unsolved by the sagacity of pathologists.

EDITORIAL AND MISCELLANEOUS.

The People's Medical Gazette.

We have received two numbers of a monthly periodical, bearing the above title, which is edited by John Davis, M.D. It is published at Abbeville C. H., S. C., at \$1 *per annum*.

The propriety or impropriety, the policy or impolicy of imparting to the people such an amount of knowledge of medicine as will warrant them in either prescribing for the relief of diseases, generally, at their inception, or treating them throughout their whole course, has been the subject of repeated discussion.

The attempt to settle this point involves naturally, and at the very threshold, this question:—*Can* they receive such an amount of instruction, without undergoing a complete course of study, as will qualify them to undertake the treatment of diseases? We unhesitatingly answer, they *cannot*. Time after time have the Faculties of the Colleges been charged with granting diplomas to multitudes of young men to practice medicine, who are utterly unqualified for that purpose; and this allegation is made, notwithstanding the compliance, on the part of the students, with the requirements of the different Institutions, as regards length of time of study, attendance on two courses of Lectures, &c. Now, if this charge be well-founded, if this censure be merited, would it not be still more strongly applicable to the people, who, without other than the most superficial and imperfect knowledge, such as is contemplated to be imparted to them by Dr. Davis's scheme, would venture to cope with the ever-varying, and frequently complicated, conditions of disease. Dr. Davis says, that it is not his "intention to attempt to make doctors of the people; it would, on the other hand, be a good thing for the community, in some respects, if we could manage to teach a few of them that they are not doctors—that they should give less medicine and with greater caution." We give Dr. Davis credit for the *rectitude* of his design; but the difficulty in the accomplishment of this object lies in the fact, that if they know ever so little, it is possible to convince them that they are not invested with the panoply of the doctorate. The self-sufficiency of man is proverbial. Let him acquire the most superficial knowledge of any subject, and, with the vanity and self-conceit natural to him, he, at once, believes it to be profound; nay, it is hard to prove to him, in many instances, that he does not rank, in omniscience, with the Deity.

There already exists too great a proclivity in the people to prescribe

for diseases. Medicine is the only department of science of which every one professes to know something ; the whole human family being engaged in practising it to a greater or lesser extent. And, if to this proneness of people, universally, but particularly of that of the United States, where no restrictions are imposed upon the practice of medicine, the most unbridled license prevailing, be added a knowledge of it, (howsoever incomplete and superficial, as we have already said, it may be,) they will become emboldened, especially if success crown their first essay in the treatment of a certain disease, to proceed farther, and will feel confident in their ability to combat other diseases of serious forms and complications ; the worst features of the case being, that they will never know when they have got beyond their depth, and the sacrifice of the life of the patient, in too many instances, the result. It is needless for us to mention examples of this kind, inasmuch as every physician's experience must furnish many.

They, too, frequently arrogate to themselves a greater skill in treating diseases than the physician possesses. In cases where the physician is heard to express, or is suspected of entertaining a doubt as to the nature of the disease, and, in consequence, is pursuing a tentative course, (preferring to act upon the principle of doing no harm if he can do no good,) some kind friend, who always has a specific on hand, advises the discontinuance of the former's medicine, and the substitution of her's for it ; and the advice is usually adopted. Thus it is not uncommon to see

"Fools rush in where Angels fear to tread."

But lest our remarks might be considered as too general, we shall descend to particulars, and show wherein Dr. Davis's design is liable to serious objections.

Dr. Davis's plan does not embrace instruction in anatomy, physiology and pathology. Diagnosis being founded directly upon, and presupposing a knowledge of these departments of the science, must of necessity be, with them, in the vast majority of cases, erroneous. One who is acquainted with the principles of medicine is fully aware that all the higher faculties of the mind are called into requisition in distinguishing diseases from each other. The people, in general, are neither by education nor habits fitted for the exercise of these high mental powers. They demand for their proper cultivation unremitting labour on the part of the individual. It is only by constant study that they can be made subservient to the purpose of correct diagnosis and appropriate treatment. By consequence of ignorance of anatomy, physiology and pathology, and physical methods of examination, (auscultation, percussion, &c.,) diseases are daily mistaken for others :—Pneumonia for

bronchitis, and *vice versa* ; bronchitis for phthisis, and the latter for the former ; pleurodynia for pleuritis ; one variety of dyspepsia for another, &c. In short, the errors of diagnosis committed by the people are very numerous, and must of necessity result in detriment to the patient. For we have known pneumonia mistaken for bronchitis and treated as such with demulcents, paregoric, &c., until the patient was *in articula mortis*, when the knowledge of the physician is of no avail.

In connection with this subject, we beg to refer the reader to Dr. David's description of the symptoms of congestive or pernicious fever. The portraiture of the disease may be ever so faithfully made, and yet even physicians who are well-read, and experienced in the diagnosis of diseases, frequently do not recognize it when presented to their observation for the first time. How, then, can the people who happen not to be familiar with the features of this disease diagnosticate it, when they do not possess a knowledge of the collateral aids, (physiology, pathology, &c.,) which the physician employs in his effort at diagnosis ? And this leads us to say a word on a topic suggested by Dr. Davis, on page 21, namely : that the physician when called to a case has frequently to order a discontinuance of the treatment which was unequivocally resulting in injury to the patient. Now, there is no point in medicine that requires a nicer discrimination, and tasks so thoroughly the sagacity and judgment of the *physician even*, than to decide, in a great many cases, when a mode of treatment is of no utility and ought to be substituted by another ; or, if doing good, when it ought to be discontinued, and the remaining part of the cure left to the powers of nature. Therapeutics being founded upon diagnosis and pathology, will be rational and successful in direct ratio to the correctness of the diagnosis and a knowledge of pathology. Having shown the people to be deficient in the latter, the former must, of necessity, in many cases, be practiced at random. One who ventures to prescribe for a disease, of whose nature he is ignorant, is to be placed in the same category with him who uses quack medicines (nostrums) ; because in neither case is the adaptation (or relation) of the medicine to the disease either under-

slightly tender and you spit freely. After which stop the calomel and take a pill composed of five grains of the white oxide of bismuth three times a day, before eating, for two weeks. Then commence and take ten or fifteen drops, as the stomach may bear, of the muriated tincture of iron in three table-spoonsful of water, for one month. Drop it into a glass tumbler, add the water, and drink it through a quill. If you do not take calomel till the gums are touched, you need not commence the treatment, for we have found from experience that this is an absolute prerequisite to a cure."

We might, as a physician, call in question the propriety of inducing pytalism, in the immense majority of cases of this disease; but, waiving this matter, we find him advising the mercurial treatment without reference to the seats and varieties of the disease, and the age, sex, status, &c., of the individual. Is the mercurial impression so innocuous that it can be made with impunity in all cases, irrespective of the above conditions?

Were Dr. Davis to limit himself to teaching, as he professes to do, the treatment, proper to be instituted at the commencement of diseases, his plan would be divested in some measure of its mischievous tendency. Instead of this, he describes the treatment, in many instances, appropriate in his opinion, to the different stages or phases of the disease.

In what are called chronic, or every third day chills, the very best treatment is to give a dose or two of blue pills—say about three at a time, every four or five hours, till the bowels are briskly moved, and then give from three to six drops, according to the age, of *Fowler's* arsenical solution four times a day, for five or six days in succession, if necessary. The quinine will stop this kind of chills, but they will return again on discontinuing its use. The reason of this is, that the chills here depend, in a great measure, upon a depraved state of the system, which is easily corrected by the blue pills and the solution referred to. You should be particular, however, and not give more of the drops at a time than we have stated, and not longer than six or eight days, before suspending their use entirely, for the same space of time, when, if necessary, you may again resume them in the same way, and give them six or eight days more, &c.

Can Dr. Davis seriously recommend the employment of this drug by those who are wholly unacquainted with the mode in which its toxicological properties are manifested! Is it proper to entrust to unprofessional hands, the administration of a substance so potent for evil as well as for good?

people should send for the Doctor for every disease with which they may be attacked; nor is it to be expected that they are to lie and suffer without making some effort, on their own part, for their relief." There are *very few* diseases, in our opinion, for which the people ought to undertake to prescribe, and then *only at their inception*. Convulsions, spasmodic laryngitis, or false croup, and perhaps one or two other diseases, the explosion of which is sudden and the danger imminent, demand prompt action on the part of the people, for their relief. But even in these diseases, how frequently are mistakes made in the application of means adapted to the removal of the cause! Their action in these cases, if they do not know the cause, ought to be limited to the application of external remedies. We maintain that, in the vast majority of diseases, both the welfare of the patient and pecuniary consideration, demand that the physician should be sent for at the onset of the disease. With persons of independent circumstances, the latter consideration has no weight. But let us take as an example, the case of a white man, upon whose daily labour depends the well-being, if not the lives of his family and himself. We suppose that in order to save expense, the treatment of his disease to be undertaken by Mrs. Smith or Mrs. Brown. So far from improving, his disease grows worse. Complications, &c. ensue. At length a physician is sent for. The disease having assumed a serious character, with numerous complications, requires a long treatment; and thus not only are his wages cut off for a long period, but, in the event of his being ultimately able to remunerate him, the physician's bill is larger, and he has undergone more suffering than there was any necessity for. In the case of a slave, it is obviously to the interest, both in a humane and pecuniary point of view, to early employ a physician, independently of the circumstance just mentioned; that the longer the disease has lasted, the longer the doctor's account, &c. The loss of a single adult negro, whose life might have been saved through the instrumentality of the physician, would counterbalance the bill of the latter, for at least two years attendance on a plantation, say of one hundred and fifty slaves. Nay, we will go further

In commenting upon the inferior position which the medical profession hold in public estimation, and assigning reasons, therefore, Dr. Davis makes the following statement, which we consider as the weightiest argument against the judiciousness of his course:—"Whilst medicine, and especially the practice of it, is a continued turmoil, subject to a thousand varying circumstances and affecting causes; other professions and sciences are generally better defined, less changing, and are founded with few exceptions, upon animate (inanimate?) matter and consequently their principles and results are more uniform, and susceptible of an easier exposition."

On page 20 Dr. Davis says:—"It is a great mistake to suppose that all who have been sick and taken medicine, and have recovered, have been cured." We object to this admission, although true. With intelligent men, who are fully aware that the issues of life and death are not in our hands, and that the actions of medicines are uncertain, &c., it is needless to attempt to enforce the conviction that medicine does, in the majority of cases, cure them. But so long as ignorance prevails to the deplorable extent that it does among the masses, they must be kept in the affirmative belief. If they should be induced to entertain a contrary belief, their confidence in articles of the *Materia Medica*, as well as the skill of the physician would be impaired, if not utterly lost; and every one knows the value of *faith* and confidence in the treatment of disease.

But why should we discuss this subject further? The whole matter lies in a nut shell. It resolves itself into the following question:—Is each one to become his own shoemaker, hatter, tailor, blacksmith, machinist, &c.? If so, why then we give our assent to the wisdom of Dr. Davis's design. For our own part, even should we know how a hat, or a machine of any kind is made, by description in the books, yet would we have neither time nor inclination to engage in its manufacture. Besides, such a proceeding, if practicable, would destroy all the advantages accruing to mankind from a division of labour.

We trust, in conclusion, that Dr. Davis will receive these remarks in the spirit by which they were directed. We entertain a high respect for his character, and an equally high estimate of his professional attainments, and regret that we have been forced to express our dissent from the expediency and policy of his undertaking. We cannot but

by which disease may be prevented. As a sample of the nature of the subjects, on which information ought to be furnished the people, we would commend to his attention the excellent article in his first issue on the "Evils resulting from the Intermarriage of Blood-Relations," which exhibits in strong light the physical and mental deterioration thereby entailed upon the off-spring. However inapplicable the opinion advanced by Pope, in respect of the other departments of human knowledge, it is, we believe, strictly true of medicine. The less people know of it, the better. They should be taught that, in practical medicine,

"A little learning is a dangerous thing"

and that it is necessary to

"Drink deep, or taste not the Picrian spring."

In justification of, or apology for, his course, Dr. Davis says that people "will give medicine, more or less, on their own responsibility," &c. If the people have the hardihood to practice that art or science, of which they know nothing, and thereby do injury to themselves, after being warned of the danger attending it, it is no concernment of ours. But, at all events, let us not have cause to reproach ourselves with being the instruments by which such injury is inflicted.

Vivisections, by Dr. M. Hall.

On the invitation of the members of the Medical Profession of Charleston, the distinguished physiologist whose name appears in the caption, conducted, in the evening of the 13th of December, at the Medical College, in the presence of a large auditory, composed of medical men, students, and other scientific and literary gentlemen, vivisections, with the object of demonstrating the truth of the theory of automatic or excito-motor actions, as distinguished from sensation and volition, after the division of the spinal cord.

The subject was a young alligator. The cord was divided at its junction with the brain, and a piece of sponge inserted between the ends of the cord. The animal lay motionless, and, to all appearance, dead. He was then placed in a very uncomfortable position, which, had he been alive, he would not have maintained. In this condition he had the *power* to move, but *volition* was lost. The division of the spinal marrow produces the same effect as that of a nervous shock, from which the animal may recover. A taper was then applied to the

tail which caused the animal to writhe and fall off the instrument on which he was suspended. Another taper was applied to his tail, which he apparently attempted to remove with his feet, one after the other. But volition is not concerned in this act; it is wholly an automatic or excito-motor act. The skin above the left inferior extremity was pinched when the leg of that side was apparently directed towards the instrument, which was finally displaced by it.

At this stage of the experiment, the animal being seen to breathe, Dr. Hall suggested that the heat of the room might have excited respiration, and desired that the temperature of the room be reduced by the admission of fresh air, which was done. After a while the respiratory movement ceased.

A portion of skin was removed from one leg. The denuded part being irritated, the animal evinced no sensation, nor was there motion. Irritation of the skin of the sound leg was attended by motion. The reason of this phenomenon is, that the skin is the chief part of the arc of reflex-action; and when it is removed, that portion of the arc is destroyed.

The crural nerve of the right leg was laid bare and irritated; motion of the member resulted. Section of the nerve was then made, and an irritant applied; there was no motion, because the continuity of the incoming nerve, which constitutes a part of the reflex arc, is destroyed.

The animal was then eviscerated with the view to prevent hemorrhage on decapitation, which was performed. After decapitation, on pinching the animal with the forceps, there was no evidence of motion to remove the imitating instrument.

The spinal cord was then irritated with the point of the scalpel, and a general convulsive movement ensued. These experiments are illustrative of the phenomena of epilepsy.

The spinal marrow in the dorsal region was laid bare and irritated; whereby, phenomena similar to tetanus were excited. In this case an upward and a downward movement are manifested; the latter being a retrograde act.

The spinal marrow was then cut across, below the anus, when, on irritation of the cut surface of the cord attached to the trunk, the hind legs moved, which is likewise a retrograde action. The cord of the

Complimentary Dinner to Dr. Marshall Hall.

During the sojourn of this world-renowned physiologist in Charleston, he was entertained by a number of his medical friends, at a sumptuous dinner provided by Mr. Mixer, at the Charleston Hotel. Prof. Dickson presided, assisted by Prof. Geddings. The cloth having been removed, Prof. Dickson, in a few very felicitous remarks, acknowledged the indebtedness of the medical world to Dr. Hall for the high value of his contributions to physiological science, and of their practical application to pathology and therapeutics; expressed the great gratification which the profession of the city experienced in his presence among them, and in having made his acquaintance, &c., and concluded by offering as a sentiment—"The health of our highly distinguished and respected guest, Dr. Marshall Hall." The rapturous applause elicited by this sentiment having subsided, Dr. Hall responded in a happy strain, in the course of which, he alluded to the satisfaction which he has enjoyed in meeting his professional brethren of the United States. We regret that we are not able to give an outline of Dr. Hall's remarks, the distance from him, at which we sat, and the low pitch of his voice having prevented us from hearing distinctly.

Transactions of the American Medical Association.

The sixth volume of this work, which should occupy a conspicuous place in the library of every physician, has been lately issued. On account of the crowded state of our columns, we have not been thus far able to notice it. It is furnished to subscribers for \$5; to associations, taking twenty-five or more copies, for \$3. Single individuals sending \$3, the minimum price, will obtain a copy. Dr. H. W. De Saussure of this city, will supply those in want of it, at the above prices.

South-Carolina Medical Association.

We take this opportunity to remind those of our readers who are members of the South-Carolina Medical Association, and others who may be desirous of becoming such—that its sixth Annual Meeting will take place in this City on the 30th of January, when a large attendance is expected.

American Medical Association.

sensation in the association, are requested to forward to the undersigned, correct lists of their respective delegations, *as soon as they may be appointed*; and it is *earnestly* desired by the committee of arrangements, that the appointments be made at as early a period as possible.

The following are extracts from Art. 2nd of the constitution :

"Each local society, shall have the privilege of sending to the Association one delegate for every ten of regular resident members, and one for every additional fraction of more than half of this number. The faculty of every regularly organized medical college, or chartered school of medicine, shall have the privilege of sending two delegates. The professional staff of every chartered or municipal hospital, containing a hundred inmates, or more, shall have the privilege of sending two delegates,—and every other permanently organized medical institution of good standing, shall have the privilege of sending one delegate."

"Delegates representing the medical staffs of the United States Army and Navy, shall be appointed by the chiefs of the army and navy medical bureaux. The number of delegates so appointed, shall be four from the army medical officers, and an equal number from the navy medical officers."

The latter clause, in relation to delegates from the army and navy, was adopted as an amendment to the constitution at the last meeting of of the association held in New York, in May, 1853.

E. S. LEMOINE. *One of the Secretaries, St. Louis.*

The Medical Press of the United States, is respectfully requested to copy the foregoing.

Editorial Correspondence of the Charleston Medical Journal and Review.

FLORENCE, Oct. 6th, and }
PARIS, Dec. 28th, 1853. }

Medical Observations, etc., made in Italy and Switzerland.

D. J. CAIN, M.D.

Dear Sir :—During my absence for near three months from Paris, which has been occupied in travelling for recreation, pleasure and general improvement, through the south of France, Italy and Switzerland, including the Neapolitan States, Lombardy and Tuscany, I have avoided having my attention much diverted by medical subjects, hence the meagreness of the following general outlines. Notwithstanding, I visited several hospitals and medical institutions, and observed with much interest the hygienic and general condition of provinces and cities, with that of their inhabitants, through which I passed. Some of these institutions are the very oldest in Europe, and are well known for the great names which distinguished them in the earlier periods of medical science. The Hospitals of Paris have been made the standards for comparison, my favourite, *La Charité*, serving specially for this purpose.

In reaching Italy by way of Lyons Nismes, Marseilles, Toulon and

Cannes, I remained a few hours at Chalons, and had an opportunity of passing through its Hospital, which contains several hundred beds, but which, in general appearance, occupies just such a position as you would suppose natural, when we compare a provincial establishment with one of the models cited above in the great capital; and yet upon how much more extensive a scale was it than those found in the thousand towns of America, much larger than Chalons.

It may be true, that the multiplication and number of public charities of this character indicate a larger proportion of the people dependent upon others, the public, and upon the government, and not taken care of by their friends and in their own homes. But when in a large City or State *one or two are necessary*, why not have them well organized and supported? If, for example, in the State of South-Carolina, for a hundred years, there has existed but two, why permit one of these (the Alms House, of which my attendance there for several months enabled me to know the internal policy and condition,) to remain, so far as it is concerned, a disgrace to any civilized community of the nineteenth century—to an ancient and honoured State, and to the city in which it exists? The crowded rooms, filthy and badly kept beds and utensils, ill-organized system of attendance, and the general and prevailing squalidness and misery there, none can gainsay. Here I often reflect upon it with a species of horror, whilst I ask myself the question—does it still exist? Is all this still there? Can the same, or a similar picture be presented, as has sometimes to myself, when visiting it at 10 o'clock at night, during the prevalence of an epidemic of typhoid fever, when to nurses incompetent to the preparation of prescriptions were added such habits, that the physician could be never sure that his stimulants were not drunk by the attendant instead of his patient, and of such depravity that it was often impossible to entrust to their care certain descriptions of the sick? These remarks are made with no unkind feeling towards any one—for we have nothing to *gain* by these expressions—but simply to institute and induce comparison and reflection. The Commissioners are gentlemen of education and standing, who give their time and attention for no pecuniary reward, and do all that they can with the material placed at their disposal. It is the city upon which falls the odium. However, I have since been informed that another, and more suitable building, has at length been purchased, and the donation of Col. Roper, with the tardy assistance of the Legislature and City Council, in the erection of the Roper Hospital, will also contribute to mitigate for the future the just condemnation by the medical critic and hygienist. The Marine Hospital, whis is so well

conducted, is, of course, another affair, being under the patronage and at the cost of the federal government. Though this Institution is small, and incommensurate with the wants of the student, yet its condition reflects credit upon those connected either with its medical or civil administration.

The Hospital at Chalons was attached to a church, as many medical establishments in Europe are; some of the beds were, in fact, in the chapel, which appeared to be incorporated into the building forming the Hospital. Thus indicating its former alliance with, and dependence upon, the religious establishments and their ministers, which though much more close at an earlier period than now, has still left visible and striking traces. Hotel Dieu, we know, was founded by a Bishop, and La Charité was entirely under ecclesiastical control, of which the daily ministrations of the sisters of charity are but a remnant. Prayers and mass are repeated daily within sight and hearing, and in the same salles with the sick, the altars' figures and images stand close by, surrounded and dressed with garlands, wreaths and flowers. A conductor led us through, but we were not there at an hour when the physicians were in attendance. Every thing appeared tolerably cleanly and well kept; my attention was attracted to the curious effect produced by the rows of red curtains which surround the beds, instead of the white which prevail elsewhere.

At Lyons, however, on the next day, I was really gratified upon entering the noble structure reared and endowed with a liberality which few cities in the world can boast of; the building itself indeed surpasses any I have yet seen. It is a doric, and extends a great distance along one of the quays of the beautiful river, which extends through the city. The central tower or horloge, forms a magnificent chamber, resembling more those in a palace by its magnitude, height, and the richness of its decorations. Within are brazen plates upon the walls, which contain the names of citizens, with the sums which they have at various periods subscribed to the completion of the building. We were conducted by one of the sisters of charity in attendance upon the sick, and entered the amphitheatre just as a patient under the influence of chloroform had been operated upon for strangulated hernia.

I am induced to make a suggestion here. that the facilities for medi-

men, to the detriment of their progress in the acquisition of French, would be forced to acquire it much sooner in Lyons or Geneva, and at the same time be enabled to pursue their professional studies. I may be permitted to observe here, with respect to myself, that not many months elapsed before I entered a private family, where no one speaks English, and in which I have since been living. These, however, strange to say, are very difficult to find of a suitable description in Paris, the natives seeming particularly adverse to admitting strangers, without special recommendations, to become members of their families, and to participate in the daily economy of their domestic establishments.

I did not enter or examine the hospitals at Nice, Genoa, Naples or Rome, my attention having been engrossed by other objects of more immediate interest to the traveller. In passing through Naples though, and under the walls of one of its hospitals, I was struck with its magnitude and extent; it would have been curious, perhaps, to have seen the *sick* lazzaroni; some of the *well* looked shocking and repulsive enough, but always light-hearted, idle and careless.

There are several academies and learned institutions at Naples. The Medico-Chirurgical Society was instituted by Boccanera, Professor of Surgery in the University. The ordinary members cannot be greater in number than sixty, and are divided into five classes, to wit: anatomy and physiology, pathology and medical nosology, therapeutics and natural medical history, pathology and surgical nosology, and legal medicine, hygiene, and public and medical police. There are honorary and corresponding members. The University of Naples dates back much further, to 1224; to it belong museums of natural philosophy, *materia medica*, anatomy, zoology, &c. Botany has always been studied in this city, but the present botanical garden, constructed for the benefit of the students, is on the most liberal scale, covering a large extent of surface; it is taught by two professors. A catalogue of 12,000 species was published in 1845.

The charitable and medical establishments at Naples are numerous and extensive; one of these, the *Maison de Charité*, was built by Charles III. as an asylum for the poor and indigent, and to prevent mendicancy and begging. It contains accommodation for five thousand persons; its actual rents amount to 250,000 ducats and dependent upon it are four or five others for poor blind persons, for young girls, for those affected with cutaneous diseases, and for the aged.

But the principal hospital of Naples is the *Hotel Dieu*, a vast pile to which I have already alluded. It was founded, in 1521, by an illustrious Neapolitan lady. (See Naples, *ses monuments, et ses curiosités*, p. 533.)

It is said to be one of the largest in Europe, and receives indistinctly all classes of the sick, of either sex, and of every condition of life, country, or faith. There are beds for 1400, a number which can, in case of necessity, be raised to 2000. The service of the hospital is confided to 33 physicians and 80 surgeons. Lunatics, who were formerly received here, are now treated elsewhere. at Aversa.

Besides these there are three other hospitals.

Among the exceedingly rich collection of objects discovered at Herculaneum and Pompeii, some of which I saw, there also preserved at the 'Musée reale Borbonico,' I was shown the following surgical instruments used by the ancient Romans. The numerals affixed indicate the number of each, which are in different degrees of preservation. Many of these resemble very closely those now in use, and are interesting in tracing the progress made in 1800 years in the improvement of surgical implements, and towards the perfectionment of the practical tools employed at each of these epochs, so far removed from one another. These were buried about 67 A. C., and I remember seeing it stated in Middleton's life of Cicero, that medical science was at a very low ebb in Rome 200 years before this period, in the time of Cato for example. There was actually no physician in Rome, and the first, coming from Greece, soon received the title of *carnifex*, or the executioner. But they did increase and multiply exceedingly, for I read elsewhere, that some three hundred years later (A. C. 170) it was written over the tomb of the Emperor Hadrian, *who perished of a dropsy*, that "verily the emperor hath died from the multitude of his physicians." So these relics of the chirurgical art of that day, which I see at Naples, exhumed at Pompei, had advanced rapidly, both in number and perfection. I saw also portions of medicines, drugs, &c., generally mineral, some of which kept in jars, were in very good state of preservation, though not so perfect as were the specimens of articles of food, rice, figs, oil, wine, water, &c. Here is the list above alluded to:

Speculum uteri 1, Speculum ani 1, Trocar 2, Lever, (*Levier*,) 1, Lancets 70, Boxes for instruments 33, Scissors 1, Catheters 2, Syringe for paracentesis 1, Pincers 1, Tenaculums 2, Spatulas 16, Marble boards for preparing medicines 14, Trusses 15, Scarifying instruments 13, Alabaster vases for preserving medicines 15, *Fibula*, (*pour l'infibulation des garçons*,) 1, other instruments of which the use is not known 121.

This *infibulation*, as it is called, consists in passing a kind of ring through the prepuce of males, and was intended to enforce and ensure chastity, its object being analogous with those now shown at the Hotel de Cluny at Paris, and which subserved the same purpose in the person of the noble dames of the chivalric period of the crusades I am

informed that these physical impediments to incontinency are resorted to, at present in certain agricultural districts of France, with respect to the lower animals.

In the neighbourhood of Naples and Veauvius are numerous warm springs of volcanic origin, which are still, as in the times of the Romans, used in rheumatism and cutaneous disease. I visited one of these called the "Baths of Nero," near to his villa, during an excursion made to Ischia, Baiæ, the cave of the Cumæan sybil, &c. Near the *Grotto del Cone*, also, I entered into, artificially, the internal terrestrial fires of the region which many find of great service; they are fuming hot, and the walls are encrusted with saltpetre and alum.—(Nitrate of potash and sulphate of alumina.) I could not remain in longer than a few minutes. It may be stated that on my way to Rome, I crossed the Pontine marshes after nightfall, and though having a seat in the banquettes on the top of the diligence, where I slept most of the time, from the usual drowsy feeling inspired by the locality, I suffered no inconvenience. It should be added, however, that though late in June, the weather was cool. The torch lights, in the hands of those stationed at the few straggling houses on the roadside, to assist in changing horses, made their occupants look even yellower and sicker than they really were.

In order to visit the falls of Vellino or Terni, and the ancient Pelasgic and Etruscan cities of Cortona, Arezzo, and Perrugio, I took *ravetu* from Rome to Florence. By this means I passed more at leisure through the central districts of Italy, and the richest portions of Tuscany, and could thus better observe the people, and face of the country.

The extensive valley of the Chiana, through which our route led, struck me as the most highly cultivated and most beautiful I had met in Europe. There were there some of the finest roads, most costly embankments, thickly settled villas, and less poverty, than anywhere else in Italy, at least. What was my surprise to learn that this smiling tract which the eye sees stretching out before it for thirty miles, was once a pestilent plain, almost uninhabited, but which modern science has so entirely changed.

This begun in 1829 has been accomplished by Count Fossombroni, the celebrated Tuscan engineer, who followed the plan recommended by Torricelli, and which was employed in draining the Maremma during the last century. This is called the system of *Colmates*, and consists in turning the course of rivers or streams, flowing from clay hills over these spots, so that they deposit their sand and mud with which they are charged, and thus raises the general level, and at the same time causing a fall of the stagnant water, converts it into a rich and fertile

tract. The portion reclaimed embraces an extensive area of country, constituting an agricultural district, which we have only seen equalled by the Isle of Wight.

Dante, in the *Inferno*, refers to the malignant fevers of the Chiana over which hydraulic engineering has now signally triumphed. For an account of the structure of the locks of canals, and the method of draining employed we were referred in the hand books to "*Memorie Fisico-Storiche sopra la Val di Chiana*, recently reprinted in an 8vo. volume at Montepulcano." We in South Carolina might derive reliable hints from this work. It would not perhaps be out of place to mention here, the phenomena in physical geography, presented by the river Clanis, which flows diametrically in an opposite direction to what it did in former times—from natural causes, as well as those indicated above its waters, with those of the upper Arno, now enter into the Arno, whilst they formerly emptied into the Tiber. Farm houses on a very large and extensive scale, and under improved systems of management, are shown in the neighborhood. (See also Murray's Guide in Central Italy.)

There are two hospitals in Florence; the one, Ospedale di Santa Maria Nuova—founded in 1287 by the father of Dante's Beatrice, I visited. It contains about a thousand beds, and the internal arrangements are in very good keeping. The ceilings are rather low, but there was apparently, a general attention to cleanliness. I was conducted by a young gentleman, attached to the chemical department, through the laboratory, in which is prepared most of the medicines, extracts, etc., employed in the Hospital. The Medical school is attached to it, and here the students study clinical medicine. This institution from its antiquity has been identified with the medical history of the city for centuries, and here most of the distinguished Florentines have practiced and acquired their reputation. As usual, in the courts and halls of the building were statuary and paintings. The second is the Hospital of Boniface, built in 1377. This is partly a military and partly a general hospital; in it are special wards for cutaneous diseases, and also for lunatics. It is said to be richly decorated, and was endowed and constructed by the liberality of the Marquiss of Soragina, a great Captain in the pay of the Republic—of whom an account may be read in Sismondi's Italian Republics.

But one of the most curious and interesting objects in Florence, both to the physician and to the general traveller, but which is well known, is the Museum of Natural History, (*Museo de Storia Naturale*.) also established in 1137. It contains long halls and chambers of great extent filled with wax preparations, representing human anatomy in all its details

These have been made at great cost, by the best artists, and is said to be one of the richest and most beautiful in the world. In morbid and minute anatomy, including injections, etc.; the "Musée Dupuytren," and the "Musée Orfila" in Paris, as well as that belonging to the College of Physicians and Surgeons of London may rival or surpass it, but in the department indicated, alone, it has no superior any where. It may be as well to add that the collection commenced with the family of the Medici's some of whom encouraged experimental science. Leopoldo and Targione added to it: and recently the Grand Duke of Tuscany has also made important contributions. The collection of fossil bones and of zoology is quite extensive—to these are added a fine botanical garden, and preparations in wax, illustrating vegetable physiology, seeds, organs, etc. The guide book tells us that the corpses in wax, which are intended to be represented in various stages of decomposition were by Zummo, a Sicilian, who executed them for Cosmo III. There are lectures delivered on Mineralogy, Zoology, Geology, Botany and Natural Philosophy. Attached to the museum is also the celebrated Tribune with the monument to Gallileo.

At Bologna, the university attracted my attention on account of its antiquity and reputation. At one time, as in the thirteenth century, twelve thousand students, from all parts of Italy, were said to have assembled there, among which were many medical. Now the department of Medicine and Surgery receives greater attention than the others. Prof. Thommassini, whose name is familiar to the readers of modern Italian medicine, practised here. This university was once conspicuous for its female professors, and learned females of foreign countries were admitted as members. One of these, Madonna Mangolini, was both surgeon and anatomist, and held a chair in the latter branch. Even quite recently, the Greek chair was filled by a woman, Matilda Tambroni, who preceded the last incumbent. The medical department acquired celebrity, also, in the twelfth century, as being the first school in which were practised dissections of the human body. Galvanism, we know, was discovered here. I went through the library, where were a very large collection of ancient works. The botanical garden of Bologna was established since the beginning of the present century, and contains some five thousand species, with every arrangement for their successful preservation. The guide books mention that an agricultural school is under operation. There are three hospitals and institutions for the sick

past." Dr. Fraser states that five hundred students now attend the hospital and school; in which there is a museum of wax preparations, but inferior to that at Florence. He was informed that a very large number of cases of stabbing were admitted annually into the hospital. Mr. Murray says the ordinary fee for physicians and surgeons, in Bologna, is five pauls (forty-five cents) and for simple consultations, two scudi (two dollars.) The sulphureous *baths* of Porretta are not far from Bologna, and as in former times, here and elsewhere in Italy, many persons resort for diseases of the skin and for glandular affections.

Attached to the hospital St. Anna, at Ferrara, where the traveller goes to visit the prison of Tasso, which lay under it, I saw and entered into wards and yards for lunatics, as well as those for the sick generally. Tasso, it is well-known, was confined there under the false imputation of lunacy.

Among other objects of much interest, as, for example, illuminated MSS., etc., etc., preserved at the *Studio-Publico*, in this city, which I saw, is a work cited by the books as "*Musculorum humani corporis picturata dissectio*." It is considered a great curiosity, and was written by Canani somewhere in the sixteenth century.

In leaving Bologna, I passed through Padua in order to reach Venice. Here I spent several hours, in order to visit objects of interest, including the medical school, etc. This was one of the most celebrated and widely known of Italy, and of Europe, and to it belongs the names we are most familiar with; as, for example, *Vesalius* (1540,) *Fallopianus* (1581,) *Fabricius ab aqua pendente* (1565), and *Spigelius* (1618.) *Sanctorius* also professed here, and *Morgagni*; with the names of all of whom the student in anatomy, particularly, is acquainted. I saw the portraits of these worthies of the sixteenth and seventeenth centuries in one chamber of the studio, as it is called. The amphitheatre in which they lectured is preserved. In the dissecting room and anatomical theatre, built by *Fabricius* (1594) and the oldest in Europe, the students were at work. In the museum, contained in another portion of the university buildings, were a very handsome collection of preparations, particularly of fossil fish. One of the vertebræ of the spine of *Gallileo* is shown, "stolen by Dr. Cocchi when his remains were removed, in 1757, to the church of Santa Croce, at Florence—a theft displaying an equal absence of good taste and of good feeling!"

The botanical garden is also the earliest in Europe, and contains some of the oldest planted trees existing. We are informed that it was instituted by the Venetian Senate in 1543, upon the application of the celebrated *Prosper Alpinus*.

This university, which was distinguished as early as 1221, also gave rise to eleven of the softer sex, of remarkable mental acquirements; one of whom was created Doctor, and spoke five languages, almost as many as Cardinal Mezzofanti, of Bologna, and wrote dissertations upon mathematics and the sciences. She died *unmarried*, the author of "Mr. Murray's Hand Book" tells us, at the age of forty-eight.

I did not go to Pavia, but the university and medical school there enjoyed a very high celebrity—I understand that even now, 600 attend every year.

There are three or four hospitals and public charities of Milan, but I visited none of them. One of these, the *Ospedale Maggiore* is a "splendid establishment," founded by Francesco Sporga and his consort in 1456. It is richly endowed, and continues to receive constant accessions to its funds from the citizens, who are extremely attached to it. On more than one occasion, individuals have left their entire property of great value to it.

Here I commenced to observe cases of *goitre*, and in the course of two hours in riding through the city, I counted as many as six affected with it. In passing over the *pass* of the *Simplon*, and into Switzerland, they began to multiply, and I counted a much larger number in the same space of time, in a village of a few thousand inhabitants, at the foot of the Alps. In Switzerland I observed more of this still, with cretinism also. My guide at Chamounix, who was quite conversant with the region surrounding Mount Blanc, and who gave me much information respecting the people, their habits and manners, (in consideration, I suppose, of my having prescribed for his niece and daughter,) informed me that cretinism, was confined almost exclusively to one side of the mountain chain. He seemed not to question the fact, and declared that the limits were quite distinct. He said that there they were not reconciled to the deformity, as some have asserted. Whilst in Paris, I became personally acquainted with Dr. Guggenbuhl, who established the institution for cretins in Switzerland, but, I did not visit it. It is in successful operation, and arrangements are being made to increase the accommodation. My stay in Geneva, Berne, Fribourgh, Basle and Strasbourg did not permit me to collect any thing of importance in a

Editorial Correspondence, Charleston Medical Journal and Review.

PARIS, Sept. 15, 1883.

Visit to the Veterinary School and Grounds of Alfort, etc.

DR. D. J. CAIN—

Dear Sir:—An Institution so well conducted, and upon such an extensive scale as that at Alfort, merits the attention of the physician, and I will, therefore, in addition to what I have besides sent you, make it the subject of a letter. Although its objects are for the most part practical and utilitarian; they are also scientific in the strictest acceptance of the term; and the daily contributions to anatomical and physiological knowledge made by an enlightened corps of Professors, continue to render it famous among the very few others of like character. The amplest opportunities for experimenting upon, and for studying the structure, maladies and peculiarities of the most useful of the lower animals, are there furnished under the patronage of a government, which has never forgotten that though science, experience, and mental cultivation are hard earned, and costly, and must be paid for handsomely, yet, that when judiciously brought to the assistance of establishments of a useful character, they increase their activity and value ten-fold, and prove of inculcable service in a national point of view. Besides, when the government has done this, it continues to show itself no charlatan—it opens wide every gate, and unites scrutiny, examination and criticism—it does not seek to confine its inventions, discoveries and ideas in France, but it is proved to be able to instruct, it loves to disseminate and proselytise, and this wins for itself both fame and wealth. It follows also as a consequence, in the present instance, that the humblest farmer or labourer, as well as the owners of the hundreds of thousands of horses, cows, sheep and dogs of Paris, find near them and obtain for nothing, all that the highest ability can furnish, when their property of this description is maimed, diseased or unfitted in any way for service.

Knowing this, I did not hesitate to apply at the gate, and readily gained admittance into the establishment, which is situated on the Seine, near its confluence with the Marne, a few miles ride to the north of Paris.

Upon entering the court which was surrounded by several extensive stone buildings, we met the Professor of *Clinical Medicine and Surgery*—M. Boullay, holding his external clinique; which consisted in the examination of a number of animals, principally horses, brought there that morning. These he proceeded to prescribe for, and operation on, or condemn, upon the most approved principles of veterinary practice.

He pronounced incurable, several horses, which are then conducted to the dead house to be killed; post-mortems and dissections are practised by the students; the skins are returned to the owners, and the remainder is either boiled for the manufacture of soap, or employed as are the portions of animals in the abattoirs which are not consumed, for manure. During the time that I was there, among others, a young woman presented a little cur, which, in relating the history of the case, and urging the diagnostic and purely objective indications of a grave malady, including internal lesions of much moment, she said, it *cried* almost the whole night!! After careful scrutiny, however, her apprehensions were declared groundless, the animal was pronounced not dangerously ill on this occasion, suffering merely from a cutaneous affection; which was duly administered to, and the happy mistress was sent on her way rejoicing. Many hundreds of these creatures, large and small, are thus annually relieved, and much serious anxiety and mental suffering saved the inhabitants—the female portion of which, you know, are proverbially fond of this description of pet. We next followed the Professor into a large covered amphitheatre, surrounded by seats for students, in the middle of which, upon a bed of straw, lay several horses well secured, and undergoing surgical operation of more or less importance. To one was applied a series of hot irons in parallel lines from some ten inches above the fetlock to the hoof—as counter irritants in chronic inflammation of the tendons. Another, suffering from what was thought to be a malignant disease of the internal substance of the hoof, was having it pared away and probed, during which it suffered a great deal, but was finally condemned. On my return to Paris, M. C. H. Robin, from whom I receive private instruction on the microscopic structure of medicinal plants, drugs, morbid specimens, etc., examined a portion of the growth, but declared it not malignant; it had involved a large portion of the neighboring tissues, and I have no doubt, would have proved fatal. M. Boullay then continued his visits into the infirmary of the stables, (*écurie*), where were a number of stalls for horses, which required to be kept in hospital, and to have special treatment for acute or chronic diseases. Upon introducing myself to him, with the request that myself and friends might have permission to visit the Institution, he enquired from what country we were, and immediately requested one of the students, (*élèves*), dressed, as all those in service, in blouse, to conduct us to all the objects of attraction. He led us accordingly to the museum, which is divided into four or five compartments, and which is quite rich in the interest and variety of its contents. The first hall contained specimens, wet and dry, of comparative anatomy—bones of

horses, cows, camels—internal organs, stuffed and injected—preparations, plates, etc. The prepared skeletons of man, the horse and camel, with the stomach and ruminating organs of the latter, and that of the ox allowed of very instructive comparisons.

In a second were morbid growths, involving bones, muscles, tendons, etc., illustrating most of the diseases to which this class of animals is liable.

Our youthful guide showed considerable acquaintance with the details, and gave us quite an interesting commentary upon what was passed in review. He next led us into an apartment containing in cases and in cabinets along the walls, calculi of the bladder, extracted from the horse by surgical means, as well as those found in the cadavre upon dissection. Some of these were from four to five inches in diameter and weighed several pounds. By far the most curious, however, were the calcareous bodis discovered in the large intestines, generally in the coecum, several of which resembled large stone cannon balls; two or three were from eight to ten inches in diameter, and one weighed ten kilogrammes. These of course are the cause of the severe colic to which these animals are subject. Besides these, were what are called *Aegragrophyls*, (?) or compact balls of hair, which form in the stomach of the ox, from their habit of licking the skins of their companions; these also varied from one to four inches in diameter, there were many much smaller, which are taken from sheep. The calculi formed in Wharton's duct and the duct of Steno, were of extraordinary size, almost equal in magnitude with those first mentioned.

In a third compartment was an extensive assortment of horse shoes—some attached to the diseased hoof; others of curious shapes and dimensions—some brought from foreign countries, including a few Arabian. This chamber also held the complete armamentary of the veterinary surgeon—being almost as extensive as the one corresponding to it, in the School of Medecine of Paris. Here were obstetrical forceps for the cow or horse, with the blades forty inches long, resembling very closely Caseaux's for females of our species. A much smaller one for dogs; anal forceps; trocars for puncturing in ascites; large instruments with double blades for opening, in cases of colic, into the left flank, and into the secondary stomach of the ox, by which means they can be mechanically relieved of indigestible matter. The offending substances are then sometimes extracted by a curette or spoon, of which I saw a specimen. There were, in addition, various apparatus for trepanning in spina ventosa, of which an admirable example is shown, and for minor veterinary surgery, phlebotomy, cauterizing, opening abscesses, etc., etc. A very striking figure in wax, represents a man with *morve*,^h who died in the service of M. Andral, at La Charité. I saw one case under the care of M. Nelaton, at the Clinique of the School of Medicine some few months since; It was contracted from a horse in this establishment, and terminated fatally. Before leaving, we saw the primary disease in a horse in one of the stables, in whom it existed in the different stages on different parts of the body.

Another young gentlemen, who was attached to the department of

Pharmacy, next offered his services to us, and led us through the laboratory, and into the dissecting rooms of those for particular anatomy and physiology. These were divided into three compartments, in the first of which the students of the first year studied the skeleton and bones—during the second, the muscular and arterial systems, and lastly the nervous and the higher branches, including experimental physiology. In one of these I saw animals for vivisections and for various experiments. There are also other rooms classified according to the periods of times, during which the incumbents have remained at Alfort, and in which the grade of studies pursued, correspond. I learned that there were nine professors, and about three hundred students, who pay 360 francs yearly—forty are sometimes permitted to enter gratuitously. In one of the halls, in which the former lecture, devoted partly to jurisprudence, and the study of sanitary police and hygiene, was a bust in marble, of the founder of the Institution, Claude Bourgelat, (1766.) The number of animals yearly treated within the institution, amount to ten or fifteen thousand, and about the same number externally, that is, those merely brought there for advice, operations, etc. I saw six large forges in one room for the iron work to be done.

Passing through an enclosure, we were conducted by shaded walks and by an extensive botanical garden, to stables somewhat removed from those just described, where experiments have been, for many years (25), pursued in the endeavour, if not to improve, to create a new race of sheep. Five varieties, one or two of them foreign, by successive crosses, have thus far given rise to offspring which bear a valuable wool, and it is called the "Race of Alfort."

In another enclosure were a large number of dogs confined by chains to the walls, and suffering from a variety of diseases; and in four or five compartments were others completely mad, upon which experiments upon hydrophobia were in course of operation. In one or two the disease had been produced artificially by inoculating them with virus from those that had become so in the course of nature. Two were raging and barked ferociously, and without ceasing an instant during the whole time we remained. Their eyes had a peculiarly wild and singular lustre, which appeared to me almost sufficient to characterize the disease. I could not obtain the data respecting the relative time of the inoculation, and the appearance of the malady, with sufficient accuracy to warrant me in giving them here. Sheep, I was informed, became mad in fifteen days after inoculation. With true French politeness, the young gentleman who conducted us seemed, and I believe was *afflicted* that we could not remain longer, in order to visit one or two buildings pertaining to the domestic economy of the school at Alfort, and with many thanks for his kindness and attentions, we bade him adieu.

My travels in the South of France, Italy and Switzerland, including Lombardy, the Neapolitan States, and Tuscany, having occupied two months and a half, I returned to Paris about six weeks since. Although too much occupied with other objects of interest, to pay much attention to those relating to medicine, yet necessarily I saw what may be of sufficient importance to give you some account of, perhaps, in another communication.

I may mention that I saw M. Guersent, among other surgical operations performed this morning at the "hospital for sick children," execute one for opening the trachea in a child with croup. As you are aware, he does it frequently, but this morning selected the method of Chassaignac, which succeeded admirably. It consisted in a simple incision, keeping the mouths of the wound open with his forceps until the false membrane and blood ceased to fill the orifice, and finally inserting the double canula and keeping the external air warm, by means of flannel wrapped around, the neck. I have, however, alluded to Guersent and Trousseau's operation for tracheotomy before.

F. P. P.

TO READERS, CORRESPONDENTS AND PUBLISHERS.

WE must beg the indulgence of a few correspondents for the non-appearance of their papers, at this time. They shall appear in our next.

Bibliographical Notices of several new publications are, by reason of the great amount of original matter, excluded from this issue.

The following books and pamphlets have been received:

Meigs on the Diseases of Children; *Ricord and Hunter on Venereal Diseases*; *Dr. Browne's Treatise on Hair and Wool*; *Piggot's Dental Chemistry and Metallurgy*; *Cazet's Surgical Pathology*; *Physician's Visiting List*; *Transactions of the American Medical Association*, Vol. VI.; *Transactions of the New York Academy of Medicine*, Vol. I., Part II.; *Sims on Vesica-Vaginal Fistula*; *Buckler on Fibro Bronchitis and Rheumatic Pneumonia*; *Mussey on Anaesthesia*.

ERRATA.

IN our last issue several typographical errors and omissions occurred, in Dr. BOWNE's paper, on the "Mechanism and Management of Parturition in the Shoulder Presentation." Our readers will please make the corrections—

Page 762, last line, let the sentence read—"Delivery effected after evisceration; the child, which was large, being dead. The mother recovered."

Page 776, ninth line from the bottom, insert after the word "synchondrosis," "in the second the head rests at the right foramen ovale and the breech at the left sacro-iliac synchondrosis."

Page 779, eighth line from the top, insert the word "by" between "to" and "them." Sixth line from the bottom, let the sentence "and that its back is to the back of the mother," read "and that the back is to the back of the mother."

Page 780, eleventh line from the top, after the words "the abdomen is in front," insert, "as in the second of the right."

Page 781, last line, insert "backward," in place of "forward."

Page 782, twelfth line from the top, for "proposition," read "proportion." Seventeenth line from the top, for "forward," read "backward."

Page 785, twelfth line from the bottom, for "case," read "cases." Sixth line from the bottom, between "from" and "accident" insert "some."

Page 786, second line from the bottom, for "process," read "powers."

Page 789, fourth line from the top, after "iliac," insert "bone."

Page 794, seventh line from the bottom, for "ascends," read "descends."

Page 796, thirteenth line from the bottom, for "pubes," read "shoulder."

CHARLESTON MEDICAL JOURNAL AND REVIEW.

Vol. IX.]

CHARLESTON, S. C., MARCH, 1854.

[No. 2.

ARTICLE I.—*Report to the City Council of Charleston, on a Resolution of Inquiry relative to "the Source and Origin of Yellow Fever, as it has occasionally prevailed in Charleston, and the means of prevention or exclusion, as may seem worthy of adoption, in order to obviate its future occurrence."* By WILLIAM HUME, M.D., Alderman, and Prof. of Experimental Science, State Military Academy.

Its first appearance in Charleston was in the year 1699, and it made fifteen subsequent visitations, to the year 1799, at long and short irregular periods of time intervening, apparently following no law of weather, viz: 1699, 1703, 1728, 1732, 1739, 1745, 1748, 1753, 1755, 1792, 1794, 1795, 1796, 1797, 1799. From 1799 to the present period, it has made 18 visitations, with similar irregularity, viz: 1800, 1802, 1804, 1807, 1812, 1817, 1819, 1824, 1827, 1828, 1830, 1834, 1835, 1838, 1839, 1843, 1849, 1852, thus presenting to our view the same picture that our ancestors contemplated; thus going through the same phases of phenomena, with the same pertinacity, and equally defying the skill and care of preceding and succeeding generations. Shall modern energy quail before this monster, or shall we resolve to rise above the contingencies that give it birth? Shall we receive and worship it as a monarch, or shall we expel it as a tyrant? Our safety demands the latter, and may success attend our laudable endeavours to accomplish our sacred purpose.

The practice of men is always derived from their precepts, and however erroneous the practice may appear, if it is in accordance with long-established precept, it is deemed just and proper. The truth of a precept is rarely questioned; popular prejudices, errors and ignorances, prevail in every department of knowledge, and truth is frequently suffocated at its birth by the incumbent mass of error and superstition. It has been maintained by the medical faculty of this city, since 1800, that

the yellow fever takes its origin in Charleston from causes existing within the same, under the influence of peculiar conditions of the atmosphere; that it is capable of self-production, whenever these conditions exist, similar to bilious fever in the country, whenever vegetation, putrefaction and temperature acquire a certain development. This may be an analogy for explanation, but it is by no means a proof of identity of cause, for the results differ in the remarkable condition of time and regularity. The natural causes of country fever exist every season, and produce their effect every year; not so with yellow fever. We have every variety of irregularity, indicating no permanent cause dependent on the regularity of the seasons. Its time of origin has varied from May to October, and its interregnum from 33, 10, 5, 3 and 2 years, to each successive year, no corresponding variation having ever taken place in our climate, adequate to explain these anomalies.

If we compare the meteorological details of individual years with each other, we will not find that all the healthy years are alike, nor are the unhealthy; nor are the healthy always unlike the unhealthy; 1845 and 1846 differ materially, yet they were both healthy; 1845 and 1852 differ but little, yet one was healthy and the other unhealthy; 1849 and 1853 are similar, (except, meteorologically, 1853 should have been more productive of disease than 1849, yet 1849 was unhealthy, while 1853 was healthy. Such comparisons tend much to weaken our faith "in the influence of the peculiar (because unknown) condition of the atmosphere." It is evident that there must be some disturbing cause, occasionally introduced to produce the observed irregularity; and from the irregularity of that irregularity, the disturbing cause cannot be directed by a natural agent. When we compare 1849 with 1853, we see a strong resemblance; 1849 is alleged to have produced the fever we presume, by its peculiar meteorological condition; if so, 1853 should have been equally, if not more, prolific in producing the same evil; but as it was not, the meteorological condition of 1849 must have been equally innocent, or the same conditions are capable of producing opposite results—a position not generally admitted by philosophers.

In the similarity between the origin of country and yellow fever, we must not omit the emanations from the soil, which are known to issue both in town and country, and no doubt play an important part in the development of disease. These emanations should materially differ; the purely vegetable productions of the country soil offer a strong con-

sumption of nearly equal quantities of animal and vegetable matter, each successive year, by the inhabitants, should deposit beneath the surface, equal quantities of relics for future decomposition. Yet these equal quantities do not produce the same effects upon every year, showing satisfactorily that the regular emanations from our soil cannot produce the irregularities of our disease.

We have no disposition to depreciate the efficacy of medical police to accomplish subordinate purposes, but to rely upon it exclusively to eradicate our disease, would be vain. Equally vain would it be to believe that the great care taken by those who have the charge of the medical police of the city, during the last summer, was the cause of the fevers' absence. To no one single cause can we justly attribute an effect which observation proves to belong to many causes. Care and caution should be extended to all the suspected causes, and until the principle is fully acted on, we may despair of success. A rigid police, to secure cleanliness and perfect drainage, is indispensable. Free ventilation of houses, and an avoidance of excessive population in small premises, is also necessary, with the other precautions which experience has shown to be essential to the preservation of health in the most favoured cities.

The exemption of Charleston from yellow fever, during the last summer, is a subject of astonishment to all observers. That the season was unpropitious to health, the ravages of the fever on the south-western waters and other regions, fully exhibit. That we shared the general diffusion of unpropitious atmosphere must be admitted, and our meteorological instruments testify to the same; yet the devouring angel spared our city. Was it that he came and found not a congenial atmosphere and soil? Or did he not visit our city to test our weakness? We apprehend the latter, for we can find no traces of his footsteps within our limits. It is true that two attempts were made by the fever to invade our city, but it was rudely and unceremoniously repelled and expelled on both occasions. As action was prompt, the history is short. Capt. P——, of the *Ellen Goldsborough*, arrived from Baltimore, on the 30th August, sickened on the 4th of September, was sent to the Marine Hospital on the 6th; was declared to have yellow fever, and on 7th was sent to the Lazaretto, where he died on the 10th. On the 23d Sept. Capt. P——, of the *Berkelew*, was reported under similar circumstances, and was immediately sent to the Lazaretto, where he also died. Thus ended both attempts at invasion, and we may here remark that it

the city is to be attributed. It is impossible now to say what might have been the result, if these vigorous measures had not been adopted, but we all know what has been the result when they have been neglected. Foreign importation is probably the perturbing cause of all the irregularities that have characterized the history of yellow fever in our city, and to this cause we propose to direct attention, and to found a course of action commensurate with its importance. We are fully aware that on this point we now have the full sanction of the Medical Society of South-Carolina, which has been hitherto withheld, and they abjure the dangerous doctrine "that the rigid enforcement of the quarantine laws was by no means necessary on account of yellow fever." Dr. David Ramsey, in the year 1800, writes to Dr. Miller, of New-York, that "the disputes about the origin of yellow fever, which have agitated the Northern States, have never existed in Charleston. There is but one opinion among the physicians and inhabitants, and that is, that the disease was neither imported nor contagious." Now the fruits of those disputes in the Northern States have been, the proof that the fever was imported, and was contingently contagious, and on these creeds they have succeeded in excluding it from their cities for the last 30 years; while we, who believe otherwise, have been nearly destroyed by its ravages in the same period. Do not the results show which is the true creed, and can there be any difficulty in explaining the revolution which has taken place in the medical faculty of our city? They have collected facts and arrayed them against doctrines. They have read on the same day, Dr. Jewell's report of the introduction of yellow fever into a part of Philadelphia, from the barque Mandarin, from Cienfuegos, Cuba, and its subsequent extension by persons who visited the vessel or places where the disease prevailed, and the flimsy and imaginative arguments composed by Dr. Rush, in the year 1802, when he recanted certain errors, into which, he says, he was led by Dr. John Lining, of our city. Now, Dr. Lining was a true observer, had no doctrines to maintain, and no opposition to combat; he knew nothing of the unity of disease, and described the yellow fever as he saw it in 1748. As the work is scarce, not easily accessible, and apropos to our present purpose, we make no

these 25 years, it has only been four times epidemical in this town, namely, in the autumns of the years 1732, 1739, 1745 and 1748, though none of these, (excepting that of 1739, whose summer and autumn were remarkably rainy,) were either warmer or more rainy (and some of them less so) than the summers and autumns were in several other years, in which we had not one instance of any one being seized with this fever, which is contrary to what would probably have happened, if particular constitutions of the weather were productive of it, without infectious *miasmata*. But that this is really an infectious disease, seems plain, not only from this, that almost all the nurses caught it, and died of it, but likewise as soon as it appeared in town, it soon invaded new comers, those who never had the disease before, and country people, when they came to town, while those who remained in the country escaped it, as likewise did those who had formerly felt its dire effects though they walked about town, visited the sick, in the different stadia of the disease, and attended the funeral of those who died of it. And lastly, whenever the disease appeared here, it was easily traced to some persons who had lately arrived from some of the West Indian islands, where it was epidemical. Although the infection was spread with great celerity through the town, yet if any from the country received it in the town and sickened on their return home, the infection spread no further, not even so much as to one in the same house."

The truthfulness of this picture we can all attest, except perhaps we have neglected to trace it easily to some persons who had lately arrived from some of the West India islands. What was easy for Dr. Lining to do, in the village of Charlestown, is not now so easy to accomplish, in the city of Charleston; yet, nevertheless, we shall see, in the sequel, that this search has been made, and the assertion of Dr. Lining fully corroborated. Dr. Lining was born in 1708, and was sufficiently mature in 1753, to think, observe and judge for himself. Dr. John Moultrie of this city, a contemporary of Dr. Lining, published his "*Dissertatio Medica inauguralis de Febre maligna biliosa Americae*," in the year 1749, when he graduated. It is alleged that he advocated a doctrine opposed to that of Dr. Lining. but on reading the article. we do not see

ports to be literal and expressive of the author's views. There are but two paragraphs that bear on our subject, and are to be found on the 9th page of the original edition of Edinburgh, 1749 :

"Almost every one is persuaded that this disease is contagious. All, doubtless, are liable to this disease, where it prevails, since they are surrounded by, and breathe the same air. From the bodies of the sick, also, acrid and semi-putrid exhalations are continually escaping, which soon infect the air of the place in which the sick man lies, and which is no less dangerous for the well than for the sick to inhale. I have seen many, however, who have sat, day after day, by the bedside of those suffering from this disease, who, by using precautions against the infected atmosphere, and avoiding much exercise, have escaped unharmed."

Dr. Moultrie continues :

"I cannot believe, with others, that this disease is propagated from one place to another, by contagion, unless the atmosphere is in a fit state to produce it. In the year 1745, this disease prevailed to a very great extent in South-Carolina, when it was very evident that contagion from some other place had no influence in producing it; for the first person with the disease, that year, was a sailor who, having been drunk for two or three days in succession, afterwards fell ill of this fever, and the heat continuing excessive during the month, the fever spread widely throughout the city of Charleston; for when the weather is very hot, there is danger lest an ordinary ardent fever should change into this disease of a more violent character; and I very little doubt that this is also occasioned by improper remedies and bad treatment. Nay, if I am not mistaken, I have myself seen some such cases."

It appears that Dr. Moultrie did not inquire from whence the sailor came, and it is possible that he was one of those persons easily traced, by Dr. Lining, as coming from some of the West India islands.

From the above extracts it is apparent that the opinion of the medical faculty, in 1800, was not derived from Drs. Lining nor Moultrie. We have no record that they ever attempted to trace the first cases to West India vessels. They seemed to assume that Charleston was within the

impure atmosphere, by the addition of another unknown element. The doctrine of a contingent contagion, or more properly, a contingent infection, will embrace all of the phenomena observed. It implies that the atmosphere of crowded, filthy cities, is necessarily impure, but contains no element capable of producing yellow fever; but if a vessel arrives from a yellow fever port, with or without yellow fever cases on board, another element is added to the atmosphere of the city, which renders it capable of producing the disease. It is as a leaven, which operates a new kind of fermentation, and results in a new production. Its effects are limited to the atmosphere of the city; hence the phenomena observed. Had the atmosphere been originally pure, the disease could not have extended, as is observed in the pure atmosphere of the country.

This doctrine plainly indicates the objects which a wise government should endeavour to effect, namely, to keep the atmosphere of the city as pure as possible, by removing filth, draining, ventilating and covering the surface of the earth. Secondly, excluding vessels and passengers from infected ports, by a rigid and effectual quarantine, sufficiently comprehensive to embrace all the means of possible entry. Should the restrictions on the commerce of the West Indies, by quarantine, amount to a suspension of the whole trade for six months each year, the evils to the city will be less than it now suffers from the annual apprehension of the fever, and "the pecuniary loss of a hundred years by the quarantine establishment cannot equal the ruin and desolation of a single season of the pestilence." Fortunately, the most effectual quarantine need not do more than interrupt the landing of cargoes for a few days, and as all vessels will be subjected to the same delay, the equality of competition remains undisturbed. If the imposed restrictions should add to the value of the imported article, it is evident that the consumer suffers this extra expense, and we doubt not but that our citizens are willing to pay the difference, and consume the article in health and happiness. Who can calculate the price of health? And is the health of the city to be placed in competition with a few cargoes of sugar and molasses, introduced without care or caution, so as to afford a luxury to our people, at the least possible expense of money, and at the greatest cost of human life. In modern times, the growth of cities depends on the increase of inhabitants. The value of city property depends upon the same, and the quantity of merchandize imported depends upon the number of consumers: hence the first act of a good government should

must defend them against the importation of diseases endemic to other climes, and exotic to our own. That yellow fever is one of this class, we have every reason to believe. Its capability of introduction into other countries is admitted by all who have carefully examined its history with unbiassed minds, but still, commercial interests induce many to believe that the matter is not fully established. The yellow fever, as an epidemic, has never been introduced into England; quarantines there are useless; hence the quarantines of other countries are injurious to British commerce, and the British government denies the necessity of them, and strives to prove their inutility. The last and most memorable case of an attempt to suppress the truth, was the introduction of the yellow fever into the island of Boa Vista, one of the Cape de Verde, by the British steamer *Eclair*. The government sent Dr. McWilliam to investigate the matter; on his return, his report was unfavourable to the government views of the subject. Dr. King was then sent, who returned with opposite views. Although he could not disprove the facts of Dr. McW., he gave them a different construction, and thus the government was enabled to maintain the integrity of its original creed. (See *Brit. and For. Med. Chir. Review*.)

The facts of the case were, that the *Eclair* contracted the yellow fever in Africa, visited this island, landed the crew, and remained three weeks, The labourers employed in cleaning the ship, and the guard who watched over the sick, were the first to take the disease, which subsequently extended over the island. The island had been healthy for fifty years, and the disease was the same as that brought by the ship's crew. Whether it was contagion or infection, matters little; the mortality was great, and the yellow fever was thus introduced.

The recent introduction of the yellow fever into a part of Philadelphia, by the barque *Mandarin*, from Cienfuegos, is a fact too well authenticated to be disproved. The extension of the disease from New Orleans along the course of the Mississippi, tells a tale for itself; and if the commission in New Orleans should succeed in proving its domestic origin in that city, it will not be able to deny its extension to other neighbouring parts, by transmission or other transportation.

To assert the domestic origin of yellow fever, in Charleston, requires as much positive information, as to assert its foreign origin; yet we have never been favoured with the exposition of the facts. We have had denials on the other side, that "the transmission of the fever is neither

from contagion, derivable from cases imported, but from local and general causes." From what observations, or from what facts, these results were drawn, we are yet to learn; and we rejoice to believe that we never will, for the Medical Society has abandoned the conclusions.

The absence of the fever from our city last summer is still a matter of general astonishment to all observers. Why should New-Orleans be depopulated, and Charleston preserve its health? It is a point which has not escaped the examination of your Committee. All local and general causes remain the same, but the most minute inquiry cannot discover the introduction of a case from abroad. No drunken sailor brought the germ of disease to infect our atmosphere. No unclean vessel lay at our wharves to be visited by a foreign sugar merchant, to inhale the disease and return to his shop to sicken and to die. No infected ship sent her crew to the Marine Hospital—in fact, excepting the cases already referred to as sent to the Lazaretto, the fever did not enter our city; and to this cause, probably, we are indebted for the preservation of the health of the city. These negative facts become more prominent and valuable, when contrasted with others of a more positive nature. The investigation of cases introduced into our city in previous years, is not so easy as some may imagine. The impression of domestic origin was formerly so prevalent, that no one attempted to trace the origin of any first case; hence we have only fragments of knowledge, collected with some labour, and reminiscences of what, "Now you speak of it, I remember." It is a lamentable fact that, notwithstanding the pertinacity with which the older medical men adhered to the belief of its domestic origin in each year, they have left no proof that such was its origin, nor have they given any proofs that it was not imported. This remains yet to be done.

Charleston was exempt from epidemic yellow fever from 1807 to 1817. During the interval of these years, the embargo, non intercourse and war occurred, and West Indian commerce was suspended. In 1816 it was renewed, and the fever prevailed in 1817. This may be esteemed a casual concurrence, and the return of fever in 1817, with the revival of the trade, may be also a casual concurrence. All we can now gather is, that the first city case was in Lynch's Lane. How it got there, no one can say. On the 3d of August, three deaths are reported in the city, and on the 28th of July, we find the record of the first case in the Marine Hospital. This predilection of the fever for sailors is at least curious, if

Capt. Bythwood sailed into their harbour from Charleston. Two days after, one of his passengers complained of sickness, and was placed under the charge of a coloured nurse. The fever was soon characterized; the patient died. The nurse was the next taken. She died; then some of her family; and from this point, the fever extended over Beaufort, and destroyed one-sixth of its inhabitants. These facts are on the authority of the clergyman of Beaufort, who officiated on the melancholy occasions, which this unfortunate arrival produced. Had the belief of a contingent contagion existed in Beaufort, this man would have been remanded to the vessel, the vessel would have been removed to a distance, and the town might have escaped the calamity which subsequently befel it.

In 1838, the Lord Glenelg, British barque, arrived from Demarara on the 4th July, with yellow fever cases on board, and lay at Boyce & Co.'s wharf. The Medora, from Liverpool, lay at Magwood's wharf, about 120 yards north. The first case in the city this year, was the wife of the steward of the Medora, who frequently visited her husband in the ship, and the first case in the Marine Hospital was a seaman from the Medora. What intercommunication took place between the two ships is not known. The steward's wife (Mrs Martin) resided on East Bay, opposite to Hard Alley. (Pritchard & Strobel.) It is important to notice that the cases of the Medora were one month after the arrival of the Lord Glenelg, being on the 1st and 4th August, a circumstance observed in many other instances, and too frequent to be considered a proof that the fever was derived from some local and general cause, and not from the infected vessel. It is needless to say more of the fever of 1838, than that there were 353 deaths, and such was its origin.

Of the origin of yellow fever of 1839, we have more detailed accounts, for which we are indebted to the industry and zeal of the late Dr. B. B. Strobel, and is remarkable as the first instance of a rebellion against the established creed of our physicians, and a determination to investigate and publish the truth as it was seen. This season was rich in West Indian commerce; not fewer than 36 vessels arrived in the months of May, June and July, each of which might have been the means of importing the fever.

On the 7th June, the Burmah arrived from Havana, and immediately sent three yellow fever cases to the Marine Hospital, of which Dr. Strobel was physician. She lost her cook in Havana, and one seaman on the voyage: hence the source of the disease is unquestionable.

could not escape attention. These fevers could not have been brought from Boston and New-York, nor could they have been produced by local and general causes in Charleston, for there was no fever in the city, and all the vessels lay in the stream. On examination, it was found that these four vessels, in company with two others from the West Indies, viz, the *Medora* and *Copia*, lay together in the stream for several days. What intercommunications took place is not now known, or whether any did; suffice it to say, that the same disease prevailed in all, and it is reasonable to conclude, for want of a better explanation, that the disease was transferred from the infected vessels to the sound. On the admission of other cases into the hospital from presumed sound ships, that is, ships from northern ports, the Doctor invariably observed that they lay in proximity with West Indian vessels, either in the stream or at the wharves. The cases were so multiplied, that accident could no longer account for the coincidence, and the infected vessels were so numerous that there was scarcely a wharf uninfested by them. A few examples will suffice to demonstrate this fact: *Grafton*, *Boyce's wharf*; *Catherine*, infected; *Thos. Bennett*, *Commercial wharves*; *Alliot*, infected; *John Anderson*, *Catherine*, infected; *Charles Carrol*, *Medora*, infected; *Silas Richards*, *Bridgeton*, *Lancet* and *Al Mendares*, infected; *Denmark*, *Alliot* and *Burmah*, infected; *Allen*, *J. C. Calhoun* and *Dela-ware*, infected. (For particulars we refer to the essay of Dr. Strobel.)

"On the 26th June Samuel Humphreys came in the Hospital from a boarding house in Market-street, kept by Jacobs. He arrived on the 19th June from Havana, in the *Caspian*, and was landed in the city before taking sick. The *Caspian* proceeded to New-York. This was the first case in the city," and was probably the origin of the cases that subsequently occurred in Market-street, for "on the 7th July, *Beitzung* took sick at the house of *Baptiste*, four to six doors west of Jacobs, and on the same side of the street," and Dr. Mackey reported many cases received in the *Pour House* as coming from this neighbourhood. After this the fever became general. The questions for consideration, in view of the cases narrated by Dr. S., are: Did the West Indian vessels infect the northern vessels, or did the city infect them? Did the West Indian

may have been requisite to complete the incorporation of the West India infection with the Charleston atmosphere impurities. The same fact was observed in the Boa Vista infection; one month elapsed before the disease assumed an epidemic form, and two months before it became general. Many similar cases may be reported where even more than one month expired before the epidemic became general. The work of Dr. Strobel contains a great many facts illustrating the introduction of the fever of 1839 into Charleston, and it is folly to suppose that it is a work purely of the imagination, unfounded on observed facts.

The fever of 1843 will be remembered as invading a portion of Charleston Neck, in the vicinity of Wolfe and Reid-streets, and proving fatal, among others, to a distinguished citizen who had lately removed to the city, and whose path to his residence passed through the infected district. We allude to Albert Rhett, Esq. Dr. Wragg reports "the city and suburbs remarkably healthy until the latter part of September, when a violent epidemic of yellow fever began on the Neck." The first case admitted to be yellow fever, occurred on the 14th of September, (Dr. Toomer,) on the southwest corner of Wolfe and Meeting-streets. The cause, of course, was referred to general and local conditions of the climate and soil. Heavy showers of rain had fallen at the end of August, and a hot sun had succeeded. There were present, perhaps, all the elements to constitute a bad atmosphere, such as has previously been engendered in this neighbourhood without producing epidemic yellow fever, but the specific modifying agent had probably been previously absent, but was present on this occasion, and produced the peculiar modification of fever which characterized the season. The following extracts from a letter from Mr. E. E. Hewes, may throw some light on the origin of the disease.

"On the 27th of July, 1843, I arrived in Charleston in the brig Hayne, Capt. Trescott, from Havana, suffering under fever. Dr. Simons came on board, and was told by Dr. Wurdeman (also a passenger) that no yellow fever was on board, he thinking that I had common bilious fever. The day before I left Havana, an Englishman, who soon after died, was laying for a few ours with a high fever on my mattress. I took him off with much difficulty, as he was delirious, rolled up my mattress, and sent it on board the Hayne. I think I put it in my berth, and slept on it during the passage. The mattress went with my baggage, and was given to a negro in the house. I was taken to the house northwest corner of Wolfe and Meeting-streets, as soon as possible after the vessel touched the wharf, where I remained until I recovered, 15 days in bed, and then went to Aiken. The first deaths I heard of in

my neighbourhood were two Dutchmen in a small house on the south side of Wolfe-street, nearly opposite my room.

"Yours,

E. E. HEWES."

Mr. H. was attended by Dr. Mitchell, seen by Dr. Dickson, and the nature of his disease was fully established. It may be urged, against the probability, that Mr. Hewes introduced the fever into this region—that the length of time between the arrival of Mr. Hewes and the development of the epidemic was too long. But according to Dr. Wragg, "there were no fevers prevalent until the 10th to 13th of September." There were literally no cases of bilious remittent and intermittent fevers previous to that time, but after the rains and hot sun, these fevers did prevail; and it is to be presumed that the yellow fever element, until then latent, gave its peculiar type to the fever. This is in strict accordance with the facts elsewhere observed, and consonant with the hypothesis derived from them. The union of the domestic and imported malaria is deemed necessary, to produce yellow fever, while the single domestic on the Neck produces bilious, &c. It is rather a curious coincidence that the only time that yellow fever has prevailed as an epidemic on the Neck and was not epidemic in the City, that a case should be proved to be carried there, and the disease should commence in the opposite house. Such coincidences much resemble cause and effect, and are difficult to refer to general and local causes alone.

The fever of 1849 will next be considered, with reference to the facts of its origin. On the 18th July, 1849, the barque Numa arrived from Havana, and on the 6th August one of her crew (Sauraboni) was sent to the Marine Hospital, where he died that night of the black vomit. Shelay at the Union wharves, foot of Pinckney-street. The Isabel and Merchant, both from Havana, arrived on the 25th July and 25th August, and lay also at Union wharves. The engineer of the Isabel was attacked on the 7th August, proving infection on board of these two ships at least. On the 20th August the British barque Queen Victoria hauled into Patton's wharf, next above Union, from Hull, England, sound and pure, and the Huron, from a British port, lay in the dock below: thus the pure and the impure vessels were all in the vicinity of each other. On the 20th or the 21st of August, the Captain of the Huron was attacked with fever, but recovered. On the 28th, Capt. Laycock, of the Queen Victoria, was attacked with yellow fever, and died of black vomit on the 1st September; five of his crew were also attacked, but recovered. Thus we have another example of what occurred in the stream in 1839, and must be attributed to the extension of the infection from one vessel to another. If we proceed up the streets leading to these wharves, we find

a case, on the 3d of September, at the corner at East Bay and Hasel-street, (Betts;) another on the 7th, east side of East Bay, one door above Hasel-street, (Carrol;) and on the 10th one at Bennett's Mill, foot of Wentworth-street, two wharves above Patton's (Drew;) one corner of Anson and Market-street, on 9th, (Behlin;) and one in Linguard-street, on 10th, (Collins.) Supposing these persons never to have visited the infected vessels, they must have been within their influence, for they were attacked in the same way, by the same disease, and at the same time as those who were adjacent to the ships. It is impossible to ascertain now what promenades these several persons may have made, in order to determine whether they went to the disease, or the disease came to them. But we have a case where the person went to the disease, brought it home, and transferred it to his neighbours, viz: Sahlman, residing at the corner of East Bay and Elliot-street, who was in the habit of going on board of vessels from the West Indies, and had been into one of the vessels at Union wharves, to purchase segars on the day before his attack. He was taken sick on the 26th Aug., and died on 2d Sept. Wm. Martin lived in Stoll's Alley and kept a Bakery in Church-street, between Broad and Elliott; was taken on 2d Sept. and died on 7th. From this period, our records enable us to trace the progress of the disease to Church and Elliotts-streets, to Anson and Pinckney, to Market and Linguard, to Hayne and Meeting, and so all over the city. It is not proper that we should omit to mention another origin of the disease, viz: from Savannah. On the 24th Aug. Holman arrived from Savannah in the steamer, on the 25th he entered the Poor House with yellow fever, and died on the 29th. We could learn nothing of the antecedents of this man. He may have come from the West Indies through Savannah, with the disease undeveloped until his arrival here. It is certain that he did not take it here, and we have no information that the fever was in Savannah.

We trust that enough has been advanced to demonstrate the extreme probability that the germ of epidemic yellow fever is imported more frequently than has been generally suspected. We acknowledge the difficulty of tracing the origin of early cases, and we also regret, that until lately, none of our physicians have sufficiently doubted the accuracy of our forefathers as to make further investigations necessary. It is no honour to Charleston, to be the parent of such a pestilence; it is no credit to her sons to endeavour to prove her to be. There is no positive proof that she is; and there is much positive proof that she is not. There is both damage and danger in the charge, and she has the right to be esteemed innocent until proved to be guilty; and it becomes her

sons of the present day, by industrious research, to disprove those assertions which the indolence and negligence of her former sons induced them to make. That Charleston is beyond the northern limit of self-generating epidemic yellow fever, is exceedingly probable, for it is the property of epidemics never to die in the places of their nativity. Yellow fever does die in our climate, for it is not renewed each successive summer; while at the Havana it flourishes in perpetuity. If the fever is not a native of the West Indies, it is at least naturalized; but it cannot be said to be naturalized in Charleston. Our winters extinguish it effectually. Charleston does occasionally produce sporadic cases of fever of domestic origin—but they do not extend, nor infect localities. In 1847, a drummer at the Citadel Academy, went on a drunken frolic to Sullivan's Island, returned to Elliott-street in the city, and when he became a little sober, reported at his quarters at the Citadel. In a few days he was taken with all the symptoms of yellow fever, even unto black vomit; he neither died, nor did he extend his disease to the numerous subjects in his immediate vicinity. Had he obtained his disease from an infected West Indian vessel, the result and consequences would have been very different. We apprehend that it is from such cases as this, that the elder physicians founded their belief of the domestic origin of yellow fever, and inferred if it was seen occasionally, to be of domestic origin, it must necessarily always be so, without distinguishing the remarkable difference which exists between the sporadic and epidemic. Another source of error was the observation, that in the pure country air the epidemic fever was not extended; such is still the observation, but the present conclusion is different. The impure air of the city is necessary for this extension, and so long as the city air is pure, the fever will not even extend in the city. We never hear of yellow fever in winter, and we always notice its decline and destruction as cool weather advances. It is possible that the reduction of temperature impedes the putrefactive fermentation, diminishes the amount of eliminated gases and vapours, and thus destroys the medium of intercommunication, and consequently the foreign element is extinguished by defect of combination and influence of a low temperature. It was a standing order to the French ships cruising in the West Indies, to make sail for the Polar seas as soon as the fever was discovered on board, and the success was generally satisfactory. It is now observed that ice constitutes the best disinfectant that can be used at quarantine grounds, and we all know

and animal matters distributed within and upon the surface of our soil, giving rise to secondary productions of every variety, is not at all extravagant nor incredible.

Having assumed an hypothesis adequate to explain all the observed phenomena, connected with the origin and propagation of the yellow fever in Charleston, we are prepared to apply it to the prevention of the evil. We have assumed, on sufficient observation and evidence, that two conditions of the air are necessary to produce epidemicity. First. A condition of impurity of domestic origin, arising from the fermentations of the earth and its contents, derived from the exuviae of a dense population. Secondly. A foreign imported element, which readily combines with our domestic impure air, and imparts a property to it, identical with what it enjoys in its native clime. Without professing to ascertain what the elements may be, we only identify them by their effects upon the human system, and their only property, at present known, is that of producing disease and death in the human family, wherever the combination is allowed to be consummated. This single observation contains all the principles of human action. Prevent the combination, and security from its ravages must be the result. Exclude the foreign element from our port, or extinguish it in the harbour, but never let it reach our wharves to effect the deadly combination. Can this be done in a commercial city? Can our wells be poisoned for the sake of commerce? Where is the difference between poisoning our air, or poisoning our water? Both are essential to the vitality of our citizens, and merchants have no more right to poison the one, than our enemies have to poison the other. Poisoning is the foulest kind of murder, equally abhorrent to the savage as the saint, and it is only necessary to demonstrate, beyond a doubt, that the poison of yellow fever is introduced among us by commerce, and we will soon find that there is among our merchants a moral principle paramount to the love of gain. However valuable the West India trade may be to the city, it cannot be equivalent to the injury produced in other branches of commerce, by the dread which the disease produces in the minds of country purchasers, and the compulsion it induces to flee to other markets, where the disease is prohibited entrance. Of death and desolation we need not argue, for habit has rendered us callous to such considerations; and the exemption which the native enjoys, may make him careless of the sufferings of others. Far be it from us to attribute a bad motive to the non-believers in the introduction of the disease, but the facts collected appear to be sufficient to produce a reconsideration of the matter, and to induce the performance of an experiment which will settle the matter beyond a doubt. The

experiment does not involve the destruction of the West India trade, although it will encumber it with additional expenses, and thus raise the price of commodities beyond their former value; but this increase of price will fall equally upon all the West India merchants, thus producing no change in their relative profits, while the community at large are made to pay the extra charges. It is not observed that the rigour and extra expenses incident to the New-York Quarantine increases the price of sugar in that market; and with all the impediments of quarantine, the West India trade is prosperous in that city. We desire to accomplish no more for Charleston than what the Legislature of the State of New-York has done for the city of New-York. We desire to imitate her example, to profit by her experience, and be rewarded by the same happy results. For, "let it be remembered," says Vaché, (State Commissioner of Health to the Marine Hospital, Staten Island,) "that the malady has not appeared in this city for nearly a quarter of a century, and not since the present health laws have been rigidly enforced, and never will, as long as the statute remains as it is, and its provisions are observed to the letter." Here we have the fruits of difference of opinion between Drs. Rush and Hosack. So long as Rush prevailed, and domestic origin was advocated, the fever continued its annual ravages. When Hosack prevailed, and foreign importation was admitted, ways and means were found to stay the pestilence, and nature seemed to yield to a correct opinion. Let public opinion change in Charleston, and ways and means will also be found to stay our pestilence. We have been taught how it may be done; we have only to put those principles into practice, and the scourge of Charleston will be as a thing that was.

From the preceding facts and observations, your Committee venture to draw the following conclusions, viz:

1. The origin of yellow fever is not dependent upon particular constitutions of the weather alone—for it has appeared, occasionally, in seasons materially differing from each other in all the details of meteorological phenomena. Yet, as a general rule, it prevails oftener in rainy seasons, with cool nights and hot days, and least in dry seasons with hot nights and hot days, all other perturbing causes being absent.

2. The development and extension of the fever is promoted by the accumulation of filth, pools of standing water, excavations in the earth, crowded population in small ill-ventilated apartments, exposure to the

necessary for the preservation of health in all cities, and which, fortunately, are within the power of a good government to effect. It is at least prudent and wise to attribute the origin of some of our fevers to the neglect of the above considerations—particularly those whose origin we cannot or have not traced to a foreign source; while it should be strictly enforced on the public mind, that the neglect of the above precautions create those impurities of the atmosphere of the city, which form the recipient, the domicile, as it were, of the foreign element through which the poison is carried to every part of the city and to every chamber and sitting room.

3. Your committee are constrained to aver that the introduction of the yellow fever by vessels from foreign ports, is by far the most frequent cause and the best established origin of the epidemic in our city; and it affords your committee great satisfaction to have collected so many facts, in support of this opinion, as it places within the power of the city authorities the means of successfully contending against its introduction. So long as the enemy entered secretly to our camp, we were unprepared for resistance: but now, as we have learned his mode and manner of attack, he is disarmed of half his terrors. Our city is our castle; and we will close our doors, both front and back, external and internal, and bid defiance to our former enemy. Our strategy is self-defence, our base of operations the Charleston bar, and our prison the quarantine ground. On this frontier we will wage the war, and leave our citizens to pursue their vocations in security, unalarmed by any other save the apprehension of neglect of duty in our officers and soldiers. Fortunately the State has given full powers to the city to execute the quarantine laws, and they are sufficiently comprehensive to embrace all the necessary details when directed to be rigidly enforced. That direction for rigid enforcement, we hope, will now be given; and the unanimous sentiment of the Medical Society, will no longer give their opinion to the government, that the rigid enforcement of the quarantine laws is by no means necessary on account of yellow fever. It is proved, on trial, to be necessary elsewhere; and we have every reason to believe that it will be equally salutary here, as the neglect of it has been accompanied with so many disasters for so many years, that its rigid enforcement promises relief, rather than an aggravation of evil. The certainties of commerce must yield to the uncertainties of medical opinions; and if we are to bear the expenses of a quarantine, we are at least entitled to perform an experiment with them, and know positively whether or not they are worth the annual support. If the execution of the laws has not hitherto excluded the disease, the error and fault was ours; we

have never believed it necessary to attempt the execution ; we now recommend the reverse instructions. Exclude it by every means, consistent with the vitality of the West Indian trade, and the vitality and health of the city. If the prosperity of the city depends upon its health, we have now the means to preserve it ; and no subordinate or secondary considerations should be allowed to interfere with and interrupt a rigid and effectual enforcement of those laws, which were enacted by the wisdom of the State, to preserve the health and advance the prosperity of the city.

In framing regulations for the successful performance of quarantine, we are not to rely upon the exact original import of the word, implying that a vessel is to be detained 40 days before coming up to the city—for in that period, it was presumed, that all persons on board, who were liable to the disease, would have taken it, and died or recovered. This was the early idea on the subject, when the plague, which was considered truly contagious, that is, diffused by personal contact or presence, was the disease to be guarded against ; but it was afterwards discovered that the disease could be extended by the vessel and cargoes ; hence the introduction of purifications, fumigations and other means directed against the vessel, in order to destroy the infection which it contained.

These two principles are still the foundation of all judicious quarantine regulations, and the details are modified in such a manner as to secure the primary object of security to the health of a city, and to offer the least impediment to its commerce with reputed infected ports. Experience has taught that it is not every article of a cargo that retains the infection. Some are purified by simple exposure to the air, others by washing, and the timbers of the ship appear to be more difficult of purification than most articles of a cargo, and so long as the cargo remains on board this purification is impossible. It is for this reason that cargoes from infected ports should be discharged at the quarantine station, purified in the manner found successful by practice, and rehipped to the city. The vessel itself is now in a fit condition for purification. It is futile to undertake the purification of the cabin and forecabin, when the hold retains its infection ; for, on the vessel's arrival at the wharf, and the hatches removed, the infection is diffused. In the recent case of the *Mandarin*, in Philadelphia, the diffusion of the infection took

of notice, while we read with avidity the history of an identical case in a northern city, protected by an efficient quarantine, and sympathizing with the people in their denunciation against the Health Officer, who was so negligent of his duty as to allow an unclean ship to unload at the wharf, and infect a portion of the city. Transshipment of the cargo, in order to ventilate and purify the hold of the vessel, is indispensable to an efficient quarantine; whether this be done by lighters, or into public stores, to be erected at a convenient station, as at New-York, is a question for future consideration. The concurring advantage of this process, is, the early delivery of the cargo to the merchant, encumbered, we must admit, with the expenses of the transshipment. After the thorough cleansing of the ship, she is allowed to come up and be ready for her export cargo. To guard against the failure of complete disinfection, it would be prudent to assign an especial and separate wharf for these vessels and their preceding cargoes, removed to the greatest possible distance from the mass of clean vessels, and out of the way of the usual congregations of seamen and other foreigners. While the removal of the cargoes and the purification of the presumed infected ship is progressing, the period of incubation of the fever is advancing, and it is probable that these delays will equal the time necessary to decide whether the disease will or will not be developed in the crew. The acclimation of our citizens to the infection of yellow fever, presents a great advantage in the process of transshipment, for they can pass from these vessels to the city, without danger of taking the disease, and transferring it to the city, while a foreigner would receive it in the vessel, bring it up, in apparent health, and in a few days be the means of counteracting all previous efforts, by its development and subsequent extension. Great caution should be exercised in the selection of the transferring crews and labourers; negroes are decidedly to be preferred, and the unacclimated stranger should be positively prohibited from joining the party.

In conclusion, your committee would offer the following resolution :

Resolved, That a committee of — be appointed to examine our laws and compare them with those of New-York and elsewhere, and report upon such modifications of the regulations as may be more efficacious in accomplishing the desired object, while it shall be less onerous to commerce.

ART. II.—*Observations on the use of the Tinct. Ferri. Mur. in Scarlatina.* By H. L. BYRD, M. D., Professor of Mat. Med. and The-
, Savannah Medical College.

DURING the present year, both erysipelas and scarlatina have prevailed to some extent in this city, and the latter disease has been attended with considerable mortality among young children. After having used the tincture chloride of iron, in erysipelas, with considerable success, as recommended by Dr. Bell, (page 126 of your Journal for last year,) and remembering the general sequela of scarlatina—often after the most favourable convalescence—I determined to give it a trial in the latter disease. My first impressions, as to its usefulness, were strengthened by the recollection, that chlorine had been recommended in scarlatina, and a knowledge of the value of mur. tinct. ferri, in the anasarcaous swelling, which so often result after an attack of that disease. My most sanguine hopes have been more than realized, experimentally, in more than twenty cases. So much am I convinced of its value, that I would not willingly exchange it for all the other remedies which I have heretofore used, or seen recommended, in scarlet fever. I am not aware of the article having been noticed before. Should such have been the case, however, I feel it due to the profession, and to suffering humanity, that the fact should be as widely disseminated as practicable. I will leave others to determine its *modus operandi* and proceed at once, to the narration of some of the most prominent cases, and the peculiar circumstances connected with them. The first case in which I used the mur. tinct. iron, occurred on the 10th July, 1853. The patient, a bright mulatto boy, æt. eight years, exhibited the eruption very distinctly, over the entire surface of the body and extremities; the throat was very much injected, and the ulceration of the tonsils had already taken place. I ordered the bowels opened with salts, senna and manna, and the tincture of iron given afterwards, in eight drop doses, diluted with gum water, every four hours. Gargle—1 drachm of the tincture to 4 ℥ water, every three or four hours. This case went on well, and in two or three days the patient was up about the chamber. The second case, was the son of H. C., Esq., aged two years. This child is rather delicate, with fair hair and full blue eyes. On my first visit, (20th July,) I found the skin covered with the eruption peculiar to scarlatina. The entire surface seemed to be involved in the eruption; the tonsils very red and injected; the parotids enlarged and painful to the touch, the left particularly so; the pulse 150, compressible; tongue furred, except at

the edges, which were red; considerable thirst; no action on the bowels since the previous day. I directed a dose composed of ol. ricin. 3 iii. sup. carb. soda, grs. ii. tinct. opii. camph. gutt. iv. camphor liniment to be rubbed over the enlarged glands of the neck, and a flannel saturated with the same, applied afterwards; throat penciled three times a day, with a solution of 6 grs. of argent nitrat, to one ounce of rose water. After the action of the oil mixture, the following was directed to be given in pape spoonful doses, every two hours, until visited again, viz: R. Salt tart. ʒ i, mucil. G. acac, ʒ iv. tart. ant. et Pot. gr. ʒ, tinct. op. camph. ʒ i, M; iced gum water allowed as a common drink. On my visit in the afternoon, I learned that the bowels had been acted upon once or twice, and the pulse was reduced by the last mixture, to 140, and the skin was soft, though not actually moist.

The child being delicate, and of somewhat strumous habit; and having been pleased with the success of the iron in the preceding case, and believing the indications even stronger in this one, I resolved to venture on its use at once. I directed mur. tinct. ferri. 50 drops, mucil g. arabic, ʒ ii, M. a teaspoonful to be given every four hours. Suspend previous mixture and continue iced gum water.

On my visit the following morning, I found the skin less red; pulse 130; no action of the bowels since yesterday; urine somewhat increased in quantity; tongue clean and red; throat the same; glands the same; continue treatment to throat and neck; also, the iron as before. Evening visit—found the patient in much the same condition as at last visit: pulse 128. The next morning the skin less red; appetite increased; tongue clean and red; some thirst, which, however, is rapidly allayed by the iced gum water; neck and throat the same. Ordered the castor oil mixture mentioned above; directed the iron treatment to be resumed after its action. Did not call again until the following morning, at which visit I found the bowels had acted well the previous day; condition generally improved: pulse 118. The iron was continued, and the lunar caustic twice per day to the throat. The next morning the skin was much paler, the throat much improved, the pulse 110. The bowels

entirely subsided from the first illness, became swollen and very painful, and despite of every effort, suppuration took place on the left side. I opened the abscess, kept it poulticed with flaxseed, and put my patient on the syrup of the iodide of iron in six drop doses, four times per day, with one-half a grain sulphate quinine, three times per day, and in a few days he was well. I have been thus minute in the details of this case, as it was the only one out of twenty in which suppuration occurred; and, further to convey a correct general idea of the course usually pursued. In no case did anasarca supervene, and the cases were usually cured in from seven to ten days. It is proper that I should remark, that occasionally other remedies, as blue pill, rhubarb, etc., were found to be necessary; and in all cases, with one or two exceptions, I made an effort to lessen the frequency of the pulse, and to determine to the surface before commencing with the iron. Minute doses of tart. ant. or ipecac, in combination with other diaphoretics, were used for this purpose; and in all cases in which the tongue grew dry under the use of the iron, it was suspended until that difficulty was removed. I found but few cases, however, in which the dryness of the tongue was traceable to the iron. I used demulcents and cool drinks, as asked for, throughout the disease. In one case treated recently, the infant child of the Hon. E. J. H., I used no other remedy than the mur. tinct. of iron. It was given in doses of from $\frac{1}{4}$ to $\frac{3}{4}$ of a drop suspended in gum arabic water, three times per day. Notwithstanding the throat was considerably inflamed, and the glands enlarged, this patient convalesced rapidly, and was well in four or five days. The child was an infant at the breast, six or seven weeks old. Two other cases had previously occurred in the same family, in one of which, I scarified the tonsils, and blistered the throat; finding both these expedients necessary to relieve the unusual inflammation and tumefaction of the tonsils which existed. It yielded readily to the iron, after the reduction of the high arterial excitement that ushered in the attack. In this case, complete desquamation of the epidermis occurred. The last case worthy of notice, is that of Ella, aged eight years, the eldest child of Dr. R. A., now under treatment (Dec. 2d). I saw her first, five days ago. The eruption was very distinct, and the throat unusually ulcerated; the tongue was coated with a brown fur, except the edges, which were red; the bowels constipated; pulse 145. I prescribed three grains each, of mass. hydrarg., pulv. rhei., and creta preparat., to be followed by castor oil in four hours. After the action of the oil on the bowels, the pulse remaining the same, and the skin dry, I used a mixture composed of sal. tartar, 3 ss, mucil. g. acac. 3 vi., tart. ant. et pot. gr. $\frac{1}{8}$, and paregoris 3 ss, in spoonful doses, every two hours, for ten or twelve hours;

after which, the mur. tinct. iron was begun with and continued in doses of ten drops, three times per day, until this morning, when it was deemed no longer necessary. This case is alluded to, simply on account of the obstinate character of the ulceration and inflammation of the throat. I have used the mixture of silver in different proportions to the ounce of water, without much benefit until yesterday, when a solution of twenty grains to the ounce was used. Gargles of tannin and pyroligneous acid, were also resorted to as auxiliaries during the intervals that the caustic was suspended. A circumstance worthy of note was, that little or no swelling or enlargement of the glands of the neck occurred during the entire case. After the solution was used, as just stated, two or three times, without any perceptible improvement in the ulceration, I applied the solid nitrate of silver with complete success.

ART. III.—*Epidemic Dysentery, as it occurred in Hancock county, Ga., during the years 1851-2-3.* By E. M. PENDLETON, M. D.

FOR several years past, this portion of Georgia has been visited by an epidemic, which has left behind it many a sad memento of its intractability and malignancy. The dysentery of the tropics seems, for a time, to have been transplanted to our more temperate latitudes. A few cases occurring in the spring and summer of 1851, within the range of our practice, was sufficient at once to foreshadow its approach, and to show what it could do, under circumstances more favourable for its development. As but few facts have been, as yet, elicited through the medical press, in reference to its etiology, pathology and treatment, we propose to do something in this way, hoping that much may yet be presented, before the period for its annual visit shall come round.

We have been frequently asked by our patrons, what we supposed was the cause of this peculiar form of disease. We have consulted with our fellow labourers, in this harvest of sorrow, as to what might be ascertained of its etiology, but all to no purpose. I know of none who have any definite opinion on the subject: and, as vet. there is too

of the epidemic, another, not less worthy or less lamented, fell a victim to its fearful invasion.

As we have already intimated, its first appearance in this county was in the spring of 1851. A few neighbourhoods, only, suffered from its attack during this season, and these were distant from the village. The neighbouring county of Taliefferro, from report, suffered severely, and we heard of its prevalence in some other sections of the state. It confined its depredations, also, to a limited period of time, when compared with the two succeeding years—being restricted, almost exclusively, to the months of April and May.

The following table will show, at a glance, its relations as to time, fatality, &c. We have included under the general term of dysentery, all the forms of bowel affection, falling under our observation, during the time designated. Some were simple diarrhoea; others cholera morbus; others summer complaint, cholera infantum, &c.; but the great bulk were dysentery proper, or mixed, showing the tenesmus, tormina, bloody mucous discharges, &c., peculiar to that disease.

Table showing the whole number of bowel affections occurring in my practice, during the three past years, together with the number of other diseases treated for each month :

	1851.		1852.		1853.		Total.	
	Bowel Diseases.	All others.	Bowel Diseases.	All others.	Bowel Diseases.	All others.	Bowel Diseases.	All others.
Jan.	3	48	5	63	2	42	10	148
Feb.	3	40	7	68	1	45	11	153
March,	0	66	4	63	0	69	4	198
April,	13	64	22	51	10	87	45	202
May,	22	68	29	81	28	70	79	219
June,	5	49	27	53	38	82	70	184
July,	4	46	14	57	34	79	52	182
August,	12	57	24	71	21	87	57	215
Sept.	3	58	15	107	10	74	28	239
Oct.	7	74	9	83	15	89	31	246
Nov.	4	113	8	50	9	62	21	225
Dec.	8	101	2	45	4	47	14	193
Aggregate.	84	779	166	792	172	833	422	2394

ence. The same months in the succeeding year, including October, we find the same influences existing in the production of disease, beginning in May, reaching the maximum in June, and gradually subsiding till it reaches even November. By a combination of the three years, we find the same results reached, all tending to demonstrate that the six warm months, beginning with April, and ending with September, are the ones mainly infested with the disease, if we except October, which seems to enter for a share, and even exceeds its predecessor. The one half of the year, numbers three hundred and thirty-one against ninety-one for the other, and if we include October and November, which seems to share to some extent, in the epidemic influence, we have three hundred and eighty-three cases for eight months, against thirty-nine for the remaining four.

It is true, then, that heat must be an exciting cause of this disease, or a generator of the peculiar poison which produces it. This fact must be received, however, with some modification ; for while the warm months, as the table shows, presents, *en masse*, the greatest number of cases, the very warmest months are more exempt from it than some others, particularly those of spring ; and this does not only hold good in the aggregate, but for each several year. Thus we have for the most prevalent month in 1851, May ; the same in 1852 ; and in 1853, June. In the aggregate, May and June far exceeds all others—showing that, while heat may be a principle in the production or evolution of the poison, it cannot be the only or main one. This is farther shown in the fact, that there is an abrupt invasion of the epidemic in the month of April for each year, which cannot be considered a warm month. Thus, the aggregate shows for March, only four cases, while April has forty-five. If heat be an essential cause, certainly July and August would exceed April and May ; yet we find for the latter, one hundred and twenty-four cases against one hundred and nine for the former, while May and June number one hundred and forty-nine. It is evident that the epidemic influence reaches its climax in May, and gradually subsides during the succeeding warm months.

In investigating the etiological features of this disease, some reference must be necessarily had to its seat, pathology, &c. The mucous membrane of the intestinal tube, in its lower portion, is evidently the seat of the disease. Upon this, the poison acts by producing simple phlogosis, irritation, or the different grades of inflammation, according to the circumstances by which the patient is surrounded. Thus, if he be a dyspeptic, an unfavourable prognosis may be expected ; what otherwise might be a simple irritation, amounts to an actual inflammation,

even though the exciting cause might be comparatively weak. Some have always diagnosed inflammation in the bowels, and used the most active antiphlogistic means as an invariable treatment. This, I apprehend, has been productive of harm. It is true, that all, or most of the post mortem examinations, exhibited relaxation of the bowels; yet I am certain, that I have treated many cases, for days together, in which neither the pulse, nor tongue, indicated any thing more formidable than a phlogistic or irritable state of the mucous membrane of the lower bowels.

What is it, then, that produces this peculiar state of things, and brings about so intractable a disease? Is it a subtle poison floating in the atmosphere, which we inhale at every breath, and thus carry into the system, deteriorating the blood, and throwing out upon the mucous membrane an irritating substance, which excoriates and inflames it? Or is it derivable through the solid or liquid ingesta taken into the system, which, by some abortive operation of nature, has been rendered poisonous, as we know is sometimes the case in certain seasons in the growth of fruit and vegetation? Or, could we attribute it to a certain combination of atmospheric causes—thermal, hygrometrical, barometrical and electrical?

The theory of contagion at first adopted by some, seems to have been generally abandoned, although some facts go strongly towards establishing a specific contagion. Thus, the first family attacked in the county, was through a servant who had been living in a family at Warrenton, then labouring under the disease. The servant was first taken down, and then, successively, nearly every member of the family, white and black. I think eight died out of thirteen cases, among whom, was Mr. A. himself, the head of the establishment. Strange to say, Dr. Pearson, the attending physician, took it while attending these cases, and died himself, after being removed to his residence. I saw both of these latter cases in consultation. My impression is, that although no specific contagion exists, there is a strong infection attending the worst form of this disease, as it generally is confined to localities, and goes through whole families without affecting others in the same neighbourhood.

We have already shown, that while heat might be instrumental in the production of the disease, we would have to look further for the

disease? We know of nothing, unless it be as already hinted, something taken as food into the stomach, which has not been properly perfected by some deficiency in the season itself. This seems to be a strange proposition, but its probability may be inferred from a number of collateral facts and statements.

That fruit and vegetables, and even grain, may sometimes be unhealthy, and produce certain morbid effects on the human system, is a fact abundantly attested, by a number of instances recorded in medical works. Thus, abortions among women, have been epidemical, from the production of ergot of rye in particular seasons. We have known the most violent cases of cholera morbus produced by eating certain articles of food, which were unripe, or unhealthy in themselves. One case, recorded a few years since, proved fatal in the city of Augusta, from eating an over-supply of cucumbers. This vegetable, as well as others, may become very unhealthy from the effect of the season upon it. It is known to contain a poisonous principle, which possibly might be diluted in a wet spring, and become more concentrated and poisonous in a dry one. This is, possibly, the case with all succulent vegetables; and as the two last springs were both very dry, we know not how much this very principle may have been effective in producing, or, at least, aggravating the epidemic in question. We have thrown out these few hints upon the etiology of this disease, more to elicit philosophical enquiry, than to make any pretension to having solved, what has, up to this period, been considered a great mystery.

We now turn our attention to a more melancholy part of our work, but one which honour, truth and justice demands. I mean the mortuary statistics of the disease. And, in order to give a proper view of its general fatality, and, at the same time, prevent invidious distinctions, I have embraced in the table, not simply the cases that occurred in my own practice, but all that I saw in consultation with other physicians. It is but sheer injustice to remark, that of the twenty-six cases chronicled for the three years, eleven were under the treatment of other physicians, till a late period, and five others were but partially treated by myself. I take the responsibility, however of the whole, in order that I

Class.	Whole No.	Per cent.	Deaths.	Per cent.
Infant,	138	32.8	15	10.8
Adult,	217	51.6	8	3.6
Youth,	65	15.4	3	4.6
White,	260	61.9	17	6.5
Black,	160	38.1	9	5.7
Male,	236	56.2	12	5.0
Female,	146	34.8	10	6.8
Unknown,	38	7.0	4	10.5

This table embraces every case of cholera infantum, and summer complaint, as well as other bowel affections occurring among children, which will partly account for the great mortality among that class. Of course, many of them originated in a morbid state of the mucous membrane of the alimentary canal, superinduced by teething or improper diet, and cannot be properly classed with epidemic dysentery. But as many of them were so blended with the prevailing type that it was difficult to make proper discrimination between them, we concluded to introduce all, and let the reader judge as best he might, the probable extent of each several type.

The African race seems to have suffered much less than the Caucasian, which will appear not only from the less number of patients as exhibited by the table, but also in the fact, that in the population of Hancock county, they are largely in the ascendent. Of the number seen, the mortality also was decidedly less among them. Why this marked difference? Possibly, because the disease is the offspring of a warm season and a warm climate, which being better adapted to the negro constitution, affords him a greater immunity from epidemics originating in his native thermal regions. Who has not read of the malignant dysentery of the Torrid Zone? And what tyro in medicine has not learned the necessity of acclimation in all diseases originating in peculiarities of location and climate? Putting the two incidents together, we

sexes as exhibited by the statistics--236 males to 146 females, where the number of each is the same in the community, presents a difference far too extensive to be considered as merely coincidental. There must be some fixed law of nature which protects the female from the invasion of epidemics of this character, possibly, as a set-off to the loss annually sustained among that class from diseases peculiar to them; or else, the greater exposure of the male to out-door employments, his recklessness, aptitude for drinking and other vicious habits, may, in part, account for his being the greatest sufferer. All of the cases marked unknown, were infants, whose sex could not be ascertained without special inquiry, which was frequently forgotten in the hurry of our arduous practice.

One more table yet remains to be offered, which will show, at a glance, the relative extent and fatality of the disease, for the several years in which it might be supposed to be epidemical. The per cent. of the aggregate of each year is taken relatively to the whole number of cases, while the per cent. of the fatality is taken relatively to the whole number of each year separately.

Year.	Whole number.	per cent.	No. of Deaths.	per cent.
1851	84	20.0	4	4.7
1852	166	39.3	12	7.2
1853	172	40.7	10	5.8
Total.	422	100.0	26	6.1

It was supposed by many that the mortality was greater during the past year, than either of the preceding ones. This, I believe, would be found true, could all the cases in the county be presented. In fact, there can be no doubt of this, as the general mortality of the county was greater than ever known before. The sexton's report for the burial-ground at Sparta, nearly doubled any of the preceding years in the memory of man. It will be perceived, however, that, as far as the table

of the pathology and treatment of the disease, as I am fully apprised that the physicians in this county have learned better to treat it than formerly.

We will now present, by way of contrast, an exhibit of the general fatality of acute diseases which most commonly prevail in this county. It extends over a period of seven years, and, perhaps, would not vary far from an estimate taken for any number of years, under similar treatment, in this section of country :

Intermittent Fever,	0 per cent.	Bronchitis,	4.0 per cent.
Bilious Rem. Fever,	0 " "	Bowel Affections	4.2 " "
Measles,	0 " "	Gastro Enteritis	5.3 " "
Influenza,	1.1 " "	Croup,	1.1 " "
Worm Fever,	2.3 " "	*Typhoid Fever,	8.8 " "
Woman Diseases,	3.0 " "	Hepatitis,	9.5 " "
Catarrhal Fever,	3.3 " "	Pneumonia.	13.6 " "

The per cent. of mortality of bowel affections, for the last three years being 6.1, shows a more aggravated form of disease, than during the seven years previous, which was only 4.2 per cent. And, when contrasted with other acute affections, we find none more formidable, if we except pneumonia, typhoid fever and croup, all of which, it is well known, have sent

" Many a trembling shade below."

We now come to the treatment—a part of the subject of vital interest to the practitioner, as well as the general reader. And in this department it is probably more easy to tell what has been done, than what is best to do ; for, as yet, no specific has been found—no physician who has come from this Aeldama with untarnished laurels. It is true, many at a distance from its first points of invasion, were disposed to speak confidently of their own skill in the treatment of this class of diseases, and some even went down, like Saul, among the prophets, to try their hand in what others had failed to do ; but if, unlike Saul, they remained not to prophesy, they at least fell into the same malediction as their brethren.

It would be a curious and somewhat startling history if all the vari-

pointed at their success, launched forth into the sea of experimentalism, and if their luck turned, as we say, theirs was the crack remedy for the remainder of the time. It was rare to find two who agreed as to the general treatment of the disease. In fact, one remedy seemed to be about as efficacious as another, or rather, some died under every varied form of treatment; and so, all felt at liberty to try their inventive powers, as many did at the expense of reason, pathology and science, if not of life. This remark is not intended to be applied specially to physicians, but equally to the medical world, who were quite as busily engaged in finding and recommending remedies, as the opposite class. Hence, strange things were affirmed, and spread far and wide by the busy tongue of rumour. One man cured forty cases on one plantation, with No. Six. The forty-first, possibly, died in the hands of a physician, because they always send for one when they have tried long enough to find out that the patient cannot survive much longer, without a change. Mutton suet was another remedy which had quite a run among a certain class, and, as is sure to be the case, was adopted and advocated by some physicians. One or two practiced hydropathy with rare success, for a while, and not a few paid their 50 cents a visit to have nothing done but wrapping the denuded patient in sheets, wrung out in cold water. Quinine had its advocates, and its day. Stewed brandy, with pitch, performed wonders in some hands; castor oil and laudanum, was a cure-all with others; while others adopted the singular combination of blue pill and laudanum, as a substitute to the old and more heroic treatment, of calomel and opium. This latter was tried in every possible form and combination, separately and together, from doses the most infinitesimal, to doses the most Herculean. The French practice, [expectant], of gum water and counter irritants, was studiously adhered to by a few, in some very bad cases; while the whole routine of astringents have been; again and again, administered by others, from tannic acid down to persimmon hash and pine-top tea. One physician announced chloroform as a specific, after trying it in several cases; and another never failed to bleed, if he could get a pulse; while a third never failed to produce the hanniast results, if sent for in time, with salts and magnesia, in broken

craved. Old ham was chosen, and the result was glorious. Hereafter, the poor starved dysenteric should send his prescription to the provision store, instead of the apothecaries' shop, if he wants certain relief from this terrible malady.

Passing from what might be termed the empirical department of this subject, we now propose to introduce the opinion of the more scientific of the profession, and the treatment severally predicated thereon. Physicians, who first encountered the disease, were inclined to adopt the alterative mode of treatment, as generally laid down in the books, and advocated by many of the ablest writers of the profession. Hence calomel, to correct the state of the liver, change the secretions, and purge off the vitiated matters of the alimentary canal, combined with opium, to allay its extreme irritability, was adopted as the favourite remedy. Another school, however, soon sprung up, after some had died, who denounced calomel as a deadly poison in the treatment of the disease, and instituted, in its stead, that class of remedies which might answer best to check the discharges from the bowels, and relieve their irritability. The various preparations of opium, with tonics, stimulants and astringents, of every grade, were given *pro re rata*. A third class adopted the opinion of decided enteritis in every case, and, hence, treated it on the antiphlogistic and counter-irritant plan.

Each of these theories, doubtless, embraced much that was good, but, taken separately, in many cases, amounted to positive harm. The first opinion is predicated upon the assumption that the liver is the radical seat of the disease. The portal system becoming itself disordered, the bile is vitiated, and acts as a corrosive poison upon the mucous membrane of the primæ viæ, when thrown out upon it, through its natural channels; or the liver, being locked up, refuses to aid in the healthful process of digestion—hence, all the secretions of the alimentary canal become themselves vitiated from the lack of this essential ingredient, and by consequence, sources of irritation to the delicate mucous membrane. This is inferred, from the almost universal absence of any—the least particle of—healthy bile in the egesta, thrown off during the progress of the disease.

The second class—equally obstinate and pertinacious—contend that the foundation of the disease is in the exhalent system of vessels connected with the lower bowels especially. That these vessels are in a relaxed and weakened condition—their mouths opened, and deprived of all their elastic and contractile powers. That, consequently, the only true, philosophical treatment is, to apply, as directly as possible, such tonics, stimulants and astringents, as might be supposed calculated to

relieve this unnatural condition. This, of all other theories, seems to have been the most universally adopted, and carried out in practice, not only among the scientific, but the unscientific. If they did not express it in so many words, they manifested it by the remedies they gave. For, upon what other principle can it be supposed that such irritants as nit. argenti, alum, acet. plumbi, tannic acid, kreosote, *et id genus omnes*, are employed? The patient, nurse, and not unfrequently the physician, all seem to dread the frequent discharge, as the great evil to be prevented. Hence, you will frequently be accosted with such expressions, "cant you check it, doctor? Do give something to stop his purging so; it will kill him." So the doctor, true to the instincts of his pathological views, if he has any, begins to inject, *per ani*, all the hosts of astringents, and administer, by the mouth, tannin, kreosote, and opium, to complete narcotism—thinking, if he can brace up the exhalent system of the bowels, and stop the flow, the patient will soon get well. But, alas! it generally continues until death, or a cessation of hostilities, by a change of treatment, relieves the patient. The truth is, the amount of egesta is generally very small, though the operations frequent—and it seems never to have occurred to many that the condition of the bowels, in this disease, is very different from cholera or a simple diarrhoea—perhaps the very reverse—and, consequently, demanding a very different treatment.

A third class of practitioners, have instituted still another mode of treatment, founded upon quite dissimilar pathological views from either of the others. The lack of the general success of any mode of treatment heretofore practised, led a few discriminating minds to examine more particularly into the etiology and pathology of the disease. Hence autopsic observations were instituted, and, in almost every case, it was found that death was preceded by ulceration of the bowels. Hence, it was inferred, that the disease was primarily an inflammation of the mucous membrane of the alimentary canal, in its lower departments. The treatment, consequently, adopted, in nearly all cases, were venesection, epispastics, gum water, with opium and calomel in minute portions. The result was, a better success than formerly, but, by no means, a uniform success. Particularly did the results of the last season teach the disciples of the lancet, that it could not be used without great discrimination and much caution. And, instead of adopting, as our motto, the exultant sign of *ureka*, we had to substitute, once more, and almost in despair, the more ominous and disheartening word—*tekal*.

the absence of proper statistical records, it is difficult, for the uninitiated at least, to determine what best is to be done in the rational treatment of this disease. We have said but little, yet, as to symptoms. They are simple, and easily defined. After presenting these, in a condensed form, we will briefly give our views of the pathology and treatment, as far as they have been formed. One of the most prominent and frequent symptoms is the tenesmus; next, pain in the bowels, as in sporadic dysentery. There is, generally, a well-marked type of fever, which is frequently of a low form, connected with a cool skin. Sometimes the fever is higher, and more inflammatory, with hot skin, thirst, etc.; and, at other times, I have seen all the other symptoms present, save the fever.

The character of the discharges deserve particular attention, as they are pretty much the same in every well-marked case; and, to a considerable extent, determine the severity of the disease. They seem to consist, almost exclusively, of mucus, with or without blood, but, generally, more or less bloody. I have seen cases in which, for days together, nothing passed from the bowels, save a gelatinous-looking mucus, with about enough blood to give it the appearance of hog brains, just taken from the skull; this, too, in such quantities as to excite wonder as to whence it could have come. In other cases, blood, fresh or coagulated, pass in large quantities. Generally speaking, when the discharges are of this character, and tolerably free, the fever, pain and inflammation, seems inclined to subside. But when the passages are scanty, and resemble shreds of cotton, cut up in small pieces, with but little blood, you may augur a bad case. The least appearance of healthy bile is always a favourable indication; although many cases recover, with the mucous discharges passing for some time after convalescence. The alvine evacuations seem to be almost exclusively composed of blood and mucus. If fresh, both as to odour and appearance, it generally marks an early, and not very dangerous, stage. But when the blood is dark and grumous, or the mucus becomes vitiated—resembling the washings of flesh, etc.—it indicates a later and more dangerous stage of the disease, and one from which recovery is rare. If a muco-purulent discharge sets in, without blood, having the odour of the dissecting room, you may feel assured that ulceration has taken place, and consider that death is almost inevitable; although I have seen one case recover, even after well-marked ulceration, and the passage of actual pus from the bowels, which yielded this result to all the usual tests.

With reference to the views which I entertain of the pathology and treatment of this disease, I am free to confess, that they have been modified more than once, and, even now, are not so clear as they might be.

Experience, however, has taught me some things, which I consider well established, and which I would not withhold from others, who have had less experience. At first, my practice was strongly tinctured with the hepatic theory, which gained strength from two notable considerations. First, the almost universal absence of healthy bile in the egesta; and, secondly; the marked tendency of the calomel and opium treatment to improve that condition, and, in doing it, always to improve the condition of the patient. The light thrown upon the pathology of the disease by autopsic examinations, introduced a new class of remedies. The lancet, which before was very sparingly used, was considered the sheet anchor, and a strictly antiphlogistic and counter-irritant plan, conjoined, was productive of eminent success. During the last season, however, the disease assumed less of an inflammatory, and more of a typhoid character, and my views and treatment received a corresponding modification.

My opinion as to the pathology is, that, in the first stage, there is a phlogistic and irritable state of the mucous membrane of the lower bowels, which causes it to throw out large quantities of mucus, mingled with blood when the vascular system is so overcharged as to be ruptured. This state of things may exist for days together, without inflammation, as I have frequently seen. But, most generally, it passes into the second stage, which is denoted by great pain, tenderness upon pressure, and severe tympanites, with a rapid pulse if the inflammation is acute—if sub-acute not so quick; and, sometimes, it assumes a chronic character, with a slow, febrile pulse. The tongue exhibits the usual redness around the edges, with white fur in the centre present during such inflammations. The third and last stage is that of ulceration, which lasts an indefinite period of time—being dependent entirely on its extent, the tissues involved, and the particular part of the intestinal tube implicated. I have seen patients die very suddenly, after all the worst symptoms had subsided—no doubt owing to perforation—or linger along for weeks, in pain and an emaciation, only to be equalled by that of tubercular phthisis.

Not unfrequently, if sent for in time, the disease is arrested in the first stage. My prescriptions are varied, according to the constitution, the state of the pulse, and character of the evacuations. If the skin be hot, pulse full, with scanty stools, attended with tenesmus—and particularly if the constitution be good, and the patient plethoric—I do not hesitate to bleed; which I would, under such circumstances, follow up with this prescription:

℞. Sulph. magnes. 3 ss.
 Magnes. calc. 3 ii.
 Tinc. opii. camph. 3 ss.
 Aqua font. 3 vi.

M. S. Shake the vial—and give a table-spoonful every two hours. This combination I regard excellent; and the results have answered all my expectations. I was led to prescribe it from having read, some years since, of the successful treatment of malignant dysentery by a distinguished physician in the East Indies, by simply using the Glauber salts. This succeeded wonderfully, after everything else had failed. I substituted the Epsom salts—because the other was more nauseous—and added the magnesia to correct any acid secretions of the stomach and bowels, and the paregoric in just sufficient quantities to allay the irritation. The salts, themselves, would quietly purge off the superabundant mucus, and relieve the phlogistic condition of the membrane. I have frequently seen, under this treatment, all the symptoms pass off in twenty-four hours, the healthy bile begin to flow, and the patient rapidly recover. But is it, hence, adapted to all cases? By no means. I abominate hobbies, which some are so disposed to mount and ride, in medicine. It tells badly for a man's mental resources, and indicates, to a philosophical mind, a great lack of that proper analytical acumen which is so essential to the medical mind, and without which no physician ever succeeds, except by empiricism. “*Qui dit docteur, ne dit pas toujours un homme docté, mais un homme qui devrait être docté.*”

Under an appropriate set of circumstances, when the depleting effects of the lancet and the saline mixture are both contra-indicated, I use something like the following:—

℞. pulv. opii. grs. xii.
 Merc. dulc. “ vi.
 P. ipecac. “ iii.
 Creta, ppt. “ vi.
 M. ft. chart. No. 12.

Take one every three hours. I prefer the powders to pills, because, in this disease especially, they are dissolved quicker, and I have known some cases when I had reason to believe that the pill was a long time being reduced to the soluble state. If a milder preparation seem de-

does. As an adjuvant to this treatment, where there is much pain and tenderness, I invariably administer the starch and laudanum injections, particularly at bed time, and during the night. In giving the saline mixture, I always add a Dover's powder at night, or even through the day if the bowels have been sufficiently acted on by it. But not unfrequently do they cease of themselves, after the scyballæ and other irritating substances have been removed.

It is impracticable, and perhaps useless, for me to go into the minutia of treatment, after so much has already been written. The mere empiric would receive but little addition to his stock of knowledge, that he might consider valuable; and the man of science, becoming acquainted with the true pathology of the disease, can adapt his remedies as well without my teachings, as with them. I will, however, present some additions to what has already been given, which I have learned by experience, and which, possibly, may be of advantage to others. Regarding them as I do, in the light of aphorisms, I will thus present them, not merely for the sake of brevity, but that they may, if possible, have the greater weight. They are honest conclusions, made up from facts hard to be controverted, and sustained by the opinions of others besides myself.

1. Death is the result, always, of inflammation in the mucous membrane of the bowels, and not debility, as some suppose, brought on by an over catharsis of the bowels, as in diarrhœa or cholera.

2. Any treatment which seeks to constrict the exhalent system of vessels, connected with the prima viæ, and check the discharges from the bowels, without first unloading them, is likely to prove injurious, superinduce inflammation, and destroy the patient. Hence, the purely astringent treatment is reprehensible, being discarded by the teachings of good philosophy, as well as experience.

3. Although death is always the result of inflammation, this morbid state does not exist primarily in many cases, but is generally the result of unwholesome diet, improper remedies, a depraved constitution, or some other untoward circumstance.

4. Although antiphlogistic remedies are generally indicated, they should be cautiously used in many cases. as nature has commenced the

disease, but frequently results in positive harm, by so lulling the system as to stop the healthful flow of the secreting surfaces.

6. No remedy can be relied on as a specific, in this disease ; but it demands combinations, and variations of treatment, according to the type and stage of the disease, and the habits and constitution of the patient. This is eminently true of this, more than most other diseases. Thus, we use cathartics, anodynes, alteratives, counter-irritants, sedatives, and even astringents and stimulants in certain cases, with good success.

7. In the sthenic form of the disease, bloody discharges generally mitigate the symptoms, and are, doubtless, not so much a radical of the disease, as an effort of nature to relieve itself of the threatening inflammation. And even the tenesmus may teach us a lesson, as to the necessity of employing laxatives—a class of remedies much dreaded by some.

8. The only diet admissible, is toast-water, parched rice-water, corn and rice gruel, milk, tea, &c., during the inflammatory stage ; subsequently, a more generous diet may be gradually instituted.

ART. IV.—*Case of Rupture of the Womb.* Reported by JAMES S. LAWTON, M. D., of Lawtonville, S. C.

IN June, 1850, I was requested, by Dr. Joseph W. Duncan, to visit, with himself, a woman, the property of Mr. B. When we arrived at Mr. B.'s plantation, we received the following statement from him :

“The woman, Mary Ann, was regarded pregnant, about the last of March or first of April, 1849, at which time her catamenia ceased, and she did not complain more than usual, (she was the mother of two living children, her age being about twenty-three,) up to the last of December of the same year, when she stated that she had labour-pains. An old black midwife, in the neighbourhood, was called in, and about an hour after she arrived, I went to the door, and enquired of her how the case was progressing. She stated to me, that it would soon be over, as the head was nearly born. It being quite late, I went to bed, and the next morning repaired to Mary Ann's house, to see after her. Upon my arrival there, I found the old midwife about to leave. I asked her about the case, and she then told me that ‘her (Mary Ann's) time had not come.’ Since that time, her health has been very bad, and she was

sure—pulse small, and ranging from one hundred and twenty, to one hundred and thirty. Asked the following questions: "When the midwife saw you in December last, were your pains as they were at the birth of your other children?" "They were, until I felt something tear inside of me, and heard something like the noise of gurgling water; after which, my pains ceased entirely." "Had you any flow of blood afterwards?" "I had a slight discharge for two or three days." "Did you have a subsequent discharge of blood?" "About the last of January, I had my regular courses, and have had no more up to this time."

On examining the mouth of the womb, we found it entirely closed. Attempted to introduce a silver catheter, but did not succeed; nor with the smallest bougie, for the os uteri was completely obliterated. We made the usual examinations for ascertaining if there was a foetus within the womb, and if it was still living. But after we had used every means in our possession, we came to the conclusion, *that there was no living foetus there*. We left, and I heard no more of the case until the 20th of August following, when I was requested to visit the case again—Dr. Duncan having left on a tour for his health shortly after we saw the case.

On my second visit, I found the woman a perfect skeleton; pulse one hundred and forty, thready and intermittent, but her spirits good. As I approached her, she said to me, "Mas. Lawton, I want you to cut me open, and take out this little devil in my belly; and you need not fear that it will kill me, for I am determined not to die, if you will rid me of this child."* On examination, I found that there was a small opening about two inches above the umbilicus, on a line with the linea alba, sufficiently large for me to introduce a small silver probe, and through which was exuding the most offensive purulent matter. I felt, distinctly, the bones of the cranium, and in withdrawing my probe, discovered, at the point, some hair. Having stated the hazard of an operation to the woman and her owner, and the certainty of death without it, Dr. Johnson, (who was with me) and myself determined to call in as many of the profession as we could procure in our sparsely settled country; and to do this, the 23d was fixed upon by us for the operation. Accordingly, on that day, with the assistance of Drs. Ellis, Ogil-

quantity of purulent matter burst forth, so offensive that all the bystanders were compelled to leave the room. The anterior portion of the sack, which contained the fetus, adhering closely to the abdominal parietes, over the line of the linea alba, formed a coat, through which I had to cut, of more than an inch in thickness. Having introduced my finger, I cut carefully down upon it, until I had made an opening sufficiently large to introduce, together with the index finger, the middle finger of the same hand. Turning the palmar surface of my hand to the internal anterior wall of the sack and abdomen, (my position being upon the right of the woman, as she lay upon the table,) I separated my fingers, and continued the incision through both walls, allowing the point of the bistoury to pass between the fingers, until I had made an incision of seven inches in length, following the linea alba, taking care to dash around the umbilicus. This being done, Dr. E. on one side, and Dr. O. on the other, parting the lips of the wound, a well developed female fetus appeared, which I seized by the breech and head, and withdrew from the sack. The membranes were all dissolved; a portion of the umbilical cord only remaining. I again introduced my hand, concaving the palmar surface, and dipped up all the dissolved membranes; then sponged out the whole sack perfectly clean, brought the lips of the wound together, and secured them with sutures and adhesive strips, and used the simple cold water dressing.

I am sorry that I cannot give the exact statement of the case as it occurred afterwards. Suffice it to say, that in a few weeks she was doing well, and walked to my house, a distance of three miles, in order to be examined by me. The wound had not then entirely healed at the lowest point, as I had taken the precaution to keep it open by the insertion of a tent, that all the vitiated matter might readily escape.

ART. V.—*New Remedies, and Old Remedies in New Forms.* By
W. M. CORNELL, M. D., Boston, Massachusetts.

Messrs. Editors:—Having read your excellent Journal, from its commencement, and formerly written somewhat for its pages, I will now endeavour to redeem my pledge, that I would write again, when an oppor-

ing Dr. F.'s remarks upon the remedial use of this root, I sent to Charleston for a quantity of it, (as it was not then kept by our apothecaries,) and I have continued to prescribe it from that time to the present—a period of over seven years—consequently, I feel somewhat prepared to speak of its medicinal properties. I have used it in constitutional syphilis, scrofula, bronchitis, in hepatic derangements, and in a great variety of cutaneous diseases; and, I may add, I know of no one remedy, of more general benefit.

Dr. F. speaks of its *emetic* properties. I have no doubt it possesses such properties, but I have never employed it as an *emetic*. I have used it as an *alterative*, and *tonic*; and for the various forms of *indigestion*. In all of these cases, it has generally done good service.

I might speak of several cases in which, in my judgment, the alterative effect, and the removal of the disease, have resulted chiefly from the *Stillingia*, but I will name only one.

In March, 1848, a respectable house-carpenter called upon me for medical advice. To use his own language, "Doctor, I am in a bad fix." He said, he "had been to three physicians, and still, he only grew worse." One year previous, he had contracted syphilis. The primary symptoms soon disappeared; but, in a few weeks, he was attacked with sore throat, inflamed tonsils, ulcerated uvula, enlarged glands of the neck, with numerous scaly eruptions about the neck and head, &c. Said, "he had taken a great amount of medicine, but it had seemed only to keep the disease at bay, and not to cure it."

He *was*, indeed, "in a bad fix." He had all the symptoms just named, with the following additional ones: the turbinated bones on both sides, were sloughing, and there were several nodes on the scalp, and ulnar bones.

He appeared to have been fully *mercurialised*, and said, he had taken "a great deal of mercury." I prescribed for him, a *saturated tincture* of the *recent stillingia*, one fluid drachm, three times a day, in a tea, made of *gum-wood*, *sassafras root* and *sarsaparilla root*. The *tincture* to be added to the tea, when taken. The patient improved under this treatment; but, after employing it for eight weeks, it was exchanged for the following:

R. Recent *Stillingia* root, - - - 3 viii.

Water

℞ ::

a week. The patient was then discharged *well*. He has remained well for the last four years.

I have found the root equally useful in various forms of *cutaneous* diseases, which had resisted many other remedies. I have had patients to apply to me for this root, from a distance of many miles, and who could not even tell its name; but it was "*the southern root*," which such a patient took.

Thus, Messrs. Editors, as I first received the hint of using this root, it is but just, after a very thorough trial with it, that I should return the mite of my experience to the Journal whence it came.

I scarcely need add, I am satisfied, it is an exceedingly valuable article as an *alterative and tonic*; and in all cases where the system needs to be *rejuvenated*.

For several weeks past, I have making trial of the *stillingin*, prepared at the Chemical Institute of Messrs. Heith & Hendrickson, 582 Houston-street, New York. These gentlemen have been preparing various *concentrated medicines* from American and foreign vegetables, in a manner similar to that by which *quinine* has been made from Peruvian bark, and morphine from the poppy. The *stillingin* which I have used, has been incorporated with some mucilaginous substance or sugar of milk, in the form of *pills*. They contain the medicinal properties of the root, with its pungent and acrid qualities, but can be conveniently taken in this form. I have not used the medicine in this form a sufficient time to be able to say whether it is equally, more, or less beneficial than the root itself. It certainly is a very *convenient* form of administering the remedy.

As I have also been indebted to Dr. Norwood, through your Journal, for the use of the *Veratrum Viride*, or American hellebore, with your leave, I will call the attention of the profession to one or two articles which I have employed, for some time past, with considerable advantage.

I have used the *Podophylina*, or the active principle of the *Podophyllum Peltatum*, commonly called the *may-apple* or *mandrake*, in many cases of constitutional impurity and torpidity of the abdominal viscera. It possesses alterative and cathartic properties. It is a little acrid, and inclined to irritate the bowels when employed *alone*: but when

employed. It can certainly be employed with more safety than mercury, so far as injury to the constitution may result. Its influence in rousing the liver from torpidity, is manifested quite early, and continues as long as the effect produced by the mineral. In diseases of the skin, it must be employed for some time; but, in my hands, it has not failed of ultimately producing its effects, with as much certainty as any other one remedy.

Combined with the *Leptandrine*, a preparation made in the same manner, from the *Leptandria Virginica*, commonly called *black root*, in doses of half a grain, $\bar{a} \bar{a}$, it is both alterative and corroborative, and a highly useful medicine in relaxed, debilitated habits.

In cases of dyspepsia, combined with the *Leontodine*, the active principle of the *Taraxicum*, or dandelion, it has been useful, in doses of a quarter of a grain of each, three times a day.

In a word, I have used, for some time, these concentrated preparations, made as above stated, and with good success, and feel confidence in recommending a trial of them to the profession. There may be cases in which the plants themselves would do better than these extracts; but they are not more numerous than those in which the bark is more efficacious than *quinine*, and *opium* than *morphine*.

ART. VI.—*Cicatrization of the Ovaries after the fall of the Ova. Formation of the yellow bodies.* From M. COSTE's "Histoire generale et particuliere du développement des corps organisés. Paris: 1847." Translated by F. M. ROBERTSON, M. D., Lecturer on Obstetrics, in the Charleston Summer Medical Institute.

Where the ovarian capsules are ruptured, to allow the escape of the ova which they contain, their walls, at first, widely expanded or gaping, soon undergo modifications which tend to fill up their cavities, or cause an absorption of their tissues. The organ, of which they form a part, altered, for a time, by the solution of continuity which the emission of the ova has caused, resumes its primitive form. But this return to the normal condition, does not take place, always, in accordance with a fixed mechanism, nor in the same lapse of time.

There are animals in which the phenomenon is simple and rapid; some, in which it is slow and more complex; and others, again, whose ovaries are never the seat of this organic restoration, because the maturation of the ova always leads to the death of the animal that produces

them, and the destruction of the maternal body, is the necessary condition upon which the female product of generation is set at liberty. This is precisely what is observed in the polypi. In them, the ova are formed upon a small ovary, enclosed within a sort of abdominal cavity, without an opening. They are, ordinarily, furnished with a solid envelop, which, in acting against the tube, which each animal represents, ruptures it, as a foreign body would do. The *crystatella nuxedo* affords a remarkable example of this destruction of the maternal body, by the ova which are developed in them; these ova, which are upon a filiform ovary, are seen sinking the long spines, with which they are surrounded, into the walls of the tubes—which are not sufficiently large to contain them—gradually encroaching upon them, and effecting a breach, through which they escape, or in which they remain entangled, until the dissolution of the parent sets them at liberty. There is no occasion, then, in this instance, to seek by what mechanism the ovary is cicatrized, as this phenomenon can only take place in those animals which survive the fall of the ova.

Animals in which the maturation and fall of the ova do not cause death, offer wide differences in the manner in which the cicatrization of the ovaries takes place, in its rapidity or slowness. This difference is explained, or may be accounted for, when that which exists between the forms of the organs, or the density of the tissues, which becomes the seat of this periodical restoration, is taken into consideration. Thus, for example, in all inferior beings, from the most simple to the bony fishes, and the batrachians, the substance of the ovaries is, ordinarily, so soft and delicate, that, with some exceptions, the walls of the capsules—attenuated by the distention caused by the ova—disappear gradually, or has nearly entirely vanished, when the moment of dehiscence arrives. So that the productive organs of the ova, in becoming despoiled of the superficial generations, expose the subjacent—which become apparent in their turn—without leaving durable traces of the rupture which has taken place beneath them. It is true, there is seen, on their surface, slight inequalities, which are the vestiges of the recent solution of continuity; but these irregularities are so promptly lost in the mass of the organ, that it is scarcely possible to establish their existence. To be convinced of the rapidity with which they are effaced, it is only necessary to examine what passes in the bony fishes and frogs, immediately after the fall of the ova, or at the moment they are about being detached.

rugose surface of the organ, an aspect a little different from that which it has at the moment of its complete integrity.

The cicatrization of the ovaries, then, in this instance, offers no peculiarity, requiring a special description. It is realized in the most simple and rapid manner, and consists in a sort of leveling of the tissue, which is produced as much by the prominence of the ova, which are no longer grasped by the superficial layer, as by the immediate retreat of the folds, which follows the dehiscence.

In the scaly reptiles, and birds, the tissue of the ovaries being at the same time more dense and extensible, than in those of the animals of which I have just spoken, the ova—in consequence of the considerable volume which they acquire—elevate their capsules so much, that they are only attached to the organs by a pedicle, through which the vascular trunks, which ramify upon their walls, pass. When, then, the moment of dehiscence arrives, and the capsules are opened, to allow the escape of the ova, they remain gaping upon a more or less flexible pedicle, which is their only medium of continuity with the organ of which they form a part. No plastic lymph fills their open cavities; they are effaced, become a little thickened, but not obliterated. Their internal walls, not contracting adhesions between themselves, always remain smooth, and lined with a layer of granular-like cellules—grey in birds and the chelonians, yellow in serpents and lizards—which is quite thick, and appears to be analogous to that which lines the Graafian follicles in the mammalia.

By degrees the species of withered and empty calices, which these ruptured capsules represent, incessantly submitted to the active influence of continued absorption, are visibly lessened, but always preserving their primitive form, and finish by vanishing from the surface of the organ, and even the points at which the development of the ova gave rise to them. You may observe, upon the single ovary of a laying hen, all the degrees of this descending series, because the ova are detached, one after the other, and at sufficient intervals to find the capsules in all stages of atrophy.

Reptiles are much less favourable for this study, on account of the fall of the ova being nearly simultaneous; all the capsules, also, disappearing at the same time, and not showing, on a single individual, the stages of degradation to which they are submitted; but they disappear by the same mechanism. There is, however, in serpents and lizards, a peculiarity which it will not be amiss to point out here. In these animals, the layer of cellular granules, which lines the internal face of the capsules that are about to emit their ova, becomes thicker

than in the tortoise and birds. It is, also, of so marked a yellow colour that, were we to judge from appearances, we should affirm that there was an analogous formation to that which is termed *corpus luteum* in the mammalia. This error is the more easily committed, since, in serpents and lizards, the capsules, in the process of absorption, are slightly folded, and resembles, to a certain extent, the Graafian follicles in the process of obliteration; but they always remain patulous, while the obliteration, of which I am about to speak, is that which constitutes the distinctive characteristic of the *true yellow body*.

In the human female, and the mammalia, the tissue of the ovaries is more dense, and less extensible, than that in birds and reptiles. It is limited, externally, by a resisting fibrous tunic, which does not permit the Graafian follicle to elevate itself into a pediculated and floating capsule. In consequence of the obstacle which this resisting membrane opposes to the follicle, it is compelled to sink, or burrow below it, into the substance of the organ in which it is developed. When the fluid which fills the follicles overcomes the resistance, and is discharged through the opening in the fibrous membrane—carrying with it the ovule—it leaves an empty cavity in the tissue of the ovary, the walls of which, in consequence of the density of the organ, are not allowed to approach each other so as to effect its obliteration. It is necessary, then, that there should be a peculiar mode of obliteration; and this end is accomplished by the combined action of the two principal coats of which the follicle is composed, and the exudation of plastic lymph into the cavity of the follicle. It is accomplished in the following manner:

No sooner are the Graafian follicles ruptured and evacuated, than their cavities are invaded by a sort of plastic secretion, frequently coloured red, and, sometimes, reddish brown, from the blood which flows from some of the vessels which have been ruptured. But this sanguineous extravasation does not ordinarily take place. It is an accident which frequently occurs in the sow—and scarcely ever in rabbits, dogs, and the human species; at least, in those cases in which the rupture of the follicles and discharge of the ova are not followed by impregnation. In almost all cases, the exhaled material is exclusively transparent, gelatiniform, adherent, filamentous, at first like melted glass, assuming afterwards, by degrees, a more marked consistence. I have frequently had the opportunity of observing this upon females who had committed suicide during gestation.

When physiologists pretend that the ovarian capsules of the superior vertebrates are filled with blood immediately after their rupture, they

express an incorrect opinion—mistaking the appearance for the reality—taking the exception for the rule. The production of plastic lymph is the only phenomenon to be noticed. It appears, however, that this phenomenon is not so indispensable that it may not, in certain cases, be produced in a very slight degree, or be entirely deficient. I have frequently been able to prove its absence in the ruptured follicles of the sheep and cow; from which it may be inferred that its intervention is not the express and indispensable condition of the cicatrization of the ovaries. It is, without doubt, a useful element; but the principal cause of this cicatrization consists, above all, in the part performed by the two coats, or layers, of which the ruptured follicle is composed, and particularly in the modification of which the internal becomes the seat—which Haller* seemed to have had a glimpse of, when he said that the yellow body was only the modification—a granulation—of the internal walls of an ovarian vesicle.

In effect, at the very moment the Graafian vesicle ceases to be distended, in consequence of its rupture, the walls retract naturally upon themselves; but the external coat of these walls, in consequence of its fibrous structure—being more plastic than the internal, the composition of which is exclusively cellulo-vascular—retracts more than the other. The internal coat, which is intimately connected with it, is compelled to follow the movements of the former; and, as it is completely passive in this act, it necessarily follows, that it is folded or becomes wrinkled in the cavity of which it forms the internal wall. This folding is produced with such promptitude, that it is always seen to be formed, however early after the rupture of the follicle the dissection may be made; and, even at this period, the folds, which result from the rapid retraction, are so numerous, salient and crowded together, that—the proportions being considered—they have, in form and disposition, the greatest resemblance to the circumvolutions of the brain.

The sudden development of these circumvolutions has caused many physiologists to suppose that the internal travail, which gave rise to them, extended back to a period anterior to that of the dehiscence of the follicle. In a word, they have admitted that the follicular walls are thickened or tumified some time before their rupture; and that, in becoming tumified, particularly at the base and sides, they press the fluid which they enclose towards the culminating point, and contribute to provoke the rupture.† I have never observed anything resembling

* *Elementa Physiol.* Concept. Berne, 1766. T. 8—p. 33.

† Baer, *Lettre sur la formation de l'œuf*, trad. Franc., p. 22—Wagner, *physiologie*, p. 129—Bernhardt, *symbolæ ad ovi mamm. hist.*, p. 38,

this. The metamorphosis of the follicular walls, is always consecutive. It can only happen, sometimes, that when the capsule is ruptured, the peritoneal tunic, which covers the ovary, may oppose, for some moments, the escape of the fluid, and the emission of the ovule, and, then, the retraction and tumefaction commences before the dehiscence; but, even in this case, it is not difficult to find material proof, that the capsule is already open, under the serous membrane, which is tardy in giving way. All that passes, here, is still conformable to the opinion which I maintain.

The causes which produce so remarkable a folding of the Graafian follicle, after the expulsion of the ova, does not consist, solely, in the influence which the external coat exercises over the internal: if this mechanical influence was the only one that produced it, the cause would be inadequate to the effect. There is evidently something more; and it is in the coincidence of this retraction, with a profound modification which takes place in the folded coat, that we must seek the complementary reasons—if I may be allowed the expression—for the amplitude of the circumvolutions that are produced. In effect, while the external coat is retracting, the internal, which is provided with an abundant vascular network, becomes phlogosed and reddened, and is profoundly modified and decidedly thickened, under the influence of which it is the seat. Now, as the first phase of this increasing hypertrophy coincides precisely with the moment at which the folding commences, it follows that the capsular circumvolutions should be, at the same time, numerous and prominent, because there are two causes tending to produce this result.

Thus, to resume, the first modifications of which the Graafian follicle becomes the seat, after the rupture of the walls, consists in the folding of the hypertrophied internal coat, and the secretion of plastic material, which fills the cavity, binding the circumvolution together as they touch each other, and become adherent. The rent, through which the ovum has escaped, is seen at the summit of the excavation, which the follicle represents, sometimes, as a straight outlet, as in rabbits, and at other times, as in the human female, as an irregular opening, the extent of which ordinarily varies from two to six millimetres and sometimes

a certain time, they touch each other, and, as they converge, the plastic material, which principally fills the follicle, being gradually absorbed, ceases to be an obstacle to their union. When this material has entirely disappeared, the circumvolutions, touching each other along their salient borders, become consolidated with the distended follicle, and form, on the surface of the ovary, an enormous prominence, which, in unipara, in general, and the human female, in particular, acquires a volume frequently larger than the organ itself, while, in multipara, the protuberance is much less prominent. It could not be otherwise in the latter, because, as there are numerous tumors formed at a time, on each ovary, there would not be sufficient space for their lodgement, if they were of as large dimensions as in the unipara.

Not less than an entire month is necessary, in pregnant females, for the folds of the ruptured follicles to completely fill the cavity, or to be on the point of adhering together. I say in pregnant females, because in those who are not so, the phenomena have not the same intensity, nor the same duration, as I shall be careful to demonstrate hereafter. I have under my eye, the ovary of a female, who was poisoned ten days after conception, in which the follicle, which had emitted the ovum—already cicatrized externally—contained a mass of plastic material, which equalled the volume of an ordinary hazel nut. In that of a young girl, drowned towards the termination of the third month of pregnancy, the plastic material had diminished considerably; it still occupied a small space between the folds, which were being more and more developed. Finally, in a third female, who had committed suicide about the thirty-fifth or fortieth day of gestation, these folds commenced to be confounded; but there still remained, in the whole extent of their adherence, a sinuous ramified white line, which represented the vestiges of this absorbed plastic lymph, which could be easily traced upon a section of the ovary.

Some time before the circumvolutions, which tend to fill the cavity of the ruptured follicle, are sufficiently enlarged to touch each other and adhere together, their phlogosed tissue loses the inflammatory redness which it had at first, and passes to a yellow colour, the shade of which varies, considerably, according to the species. It is of a vivid orange in the cow; lemon yellow in the human female; rose and flesh coloured in the sheep, &c.

The thickening of the walls of the internal coat of the ovarian capsule, results, simply, from the large volume acquired by the vesicles or cellules of which the tissue is composed; and its yellow colour is owing to the nature of the molecular granulations which these cellules enclose.

The progress of this internal change, can be observed, under the microscope, in all its different shades. If the tissue of the internal coat of a capsule be examined, by the aid of this instrument, a little before its rupture, it will be found that, independent of the abundant vascular network which is distributed upon it, it is, exclusively, formed by an assemblage of small vesicles or cellules, in each of which there are some uncoloured molecular granules; but, soon after the dehiscence, the vesicles are developed to such a degree that, at the moment the circumvolutions fill the cavity of the capsule, they have all acquired a volume five or six times larger than they primitively had. It necessarily follows then, that the membrane, of which they form the walls, will be proportionately thickened. It, also, becomes much softer and more friable, because the vesicles are no longer so adherent, and their walls become weaker. Consequently, at a certain epoch, the capsular circumvolutions assume an encephaloid appearance, which is, as I have before stated, the result of the modification, both of the constituent vesicles and their contents. There is a period in which the disunion of these vesicles is so easily effected, that simply scraping the capsule is sufficient to detach nearly all of them, and nothing more than the despoiled vascular arcade, which penetrates into each fold, remains in the latter. I have made this preparation in many bodies previously injected, and I have clearly observed the peculiarities which I have described.

In proportion as the constituent vesicles become enlarged, their contents are modified in an appreciable manner. There is formed, in the cavity of each of them, an innumerable quantity of molecular granules, which become more and more opaque, and, under the influence of the slightest pressure, pass through the rupture in the walls which contain them. These granules are not alone remarkable for their number, but also for the yellow tint which slightly colours them. Now, as they are very abundant and compressed together in the interior of the vesicles which contain them, it follows that the yellow tint, which, in each one isolated, is not very great, becomes more evident in the entire mass; it would appear that something occurs, here, analogous to that which takes place during the yellow colouration of the vitellus of birds. In making known the material conditions of this phenomenon, I have stated, in effect, that it was produced by the crowding together of the granules

vividly coloured particles, or little globules, which are mingled with the granules, are really oleaginous as in birds; I have been led to believe, on the contrary, that they are of a different nature, although their appearance is very similar. It is particularly in the tissue of the yellow body of the cow, that these globules of colouring matter are abundant; they sometimes assume such large dimensions, that their volume visibly contrasts with that of the molecular granules with which they are mingled. The yellow colour, which they contribute to produce, is, also, the most vivid and intense that can possibly be imagined. This colouring, then, cannot be attributed to an extravasation or imbibition similar to that which is manifested in echymosis or in the centre of cerebral hemorrhages, as has been done by M. M. Ponchet* and Raciborski.† The phenomenon is owing to a very different cause.

Finally, when the hypertrophy of the internal coat of the ovarian capsule has arrived at its term, when its tumefied folds have contracted intimate adhesions, the assemblage forms a compact and resisting tumour of a variable volume, according to the species; but, as I have already stated, in unipara as large as the ovary itself, so that, where the organ is the seat of this enormous vegetation, it appears to be constituted of two equal bodies—one representing its proper tissue, and the other the follicle converted into a yellow body. In multipara this tumour never acquires such proportions. It ordinarily forms a small mamillary eminence; so that six, eight, and sometimes, even ten may exist on each ovary, without this organ disappearing—thus to speak—under the pressure which they exert upon its tissue; but, in the one case, as in the other, they arise and are developed in the same manner. It is always the vegetation of the internal coat of the ruptured follicle, that obliterates its cavity.

In the human female, this vegetation arrives at its apogee, from the tenth to the fifteenth day of gestation. It remains stationary some time afterwards; then, towards the end of the third month—sometimes sooner, and often later—another period commences; it is that of its decline. At this time, the capsular tumour, having followed an ascending progression, commences to pass through its ulterior modifications, in an inverse manner. The adhesion of the circumvolutions, of which it is

vanish; the vessels which furnish the nourishing juices, are extinguished; the decadence of the total mass progresses actively, and is consummated by degrees. The tumour, progressively absorbed, seems to re-enter into the organ, upon the surface of which it was elevated, and from which it soon vanishes. It finally diminishes to such a degree that, at the end of a certain time, it is nothing more than a small, indurated, yellowish, lardaceous tubercle, the last vestiges of a phenomenon which has come to an end. This tubercle still persists, however, under the external cicatrix, which indicates the point at which the rupture of the obliterated capsule was effected; but it is not long in disappearing, sometimes leaving, in its place, a small body of a fibrous nature. When an ovary has been the seat of a travail, similar to that which has been described, a number of times, the cicatrices which result produce irregularities, which gives to the surface of the organ a cracked, rugose and fissured appearance.

I have followed all the phases of this decadence, in the pregnant female, with the greatest care. It begins to be really appreciable about the end of the third month. In the course of the fourth, the yellow body diminishes nearly one-third, and, towards the end of the fifth, it is ordinarily reduced one-half; from the sixth to the ninth, it has lost at least two-thirds of its volume. It forms still, some days after accouchment, and in a great number of cases, a tubercle not less than seven or eight millimetres in diameter. This tubercle afterwards diminishes very rapidly, but nearly a month is necessary, before it is reduced to the condition of a small indurated kernel, which persists, more or less, as the last vestige of a travail, slow in arriving at its final term. However, there is nothing absolute in the decreasing progress of the phenomenon. I have seen females who had died at the sixth, and even at the seventh month of pregnancy, with yellow bodies as voluminous as others at the fourth. Although, in general, it is only after parturition that the yellow body is effaced, we are not without examples of its disappearance at an earlier period. I had occasion to examine the cadaver of a female who had died in the course of the eighth month of gestation, in whom the absorption was already complete. Facts of this kind

allow the ovum to escape; their ruptured walls are folded, become tumefied in their term, obliterate their cavities, and afterwards disappear, following the same phases as those whose history I have just given.*

Such is the mechanism by which the ovarian capsules of the superior vertebrates are obliterated, and the ovaries cicatrized. This mechanism differs greatly from that which takes place in all the rest of the series. It is here, of a much more complex character. It is not a simple leveling of the tissue, as in the inferior animals, nor the atrophy or retraction of an open calix, as in reptiles and birds, which is absorbed and disappears without an obliteration of the cavity; it is a cavern formed in the substance of the ovary, which is filled by a sort of granulation of its internal wall, and is only cicatrized after having given rise to the peculiar modification designated by the term *yellow body*.

The number of these bodies is ordinarily equal to that of the ova that are found in the uterus, as was perfectly recognised by Graaf. This rule is not so absolute, however, but that exceptions are sometimes met with. These exceptions are owing to two principal causes: a single capsule may enclose more than one ovum, in which case the number developed in the uterus, would be greater than the number of yellow bodies produced on the ovary; second, among the ova detached from the ovary, there may be some of them that will abort while traversing the vector tube, and then the number of ruptured capsules would be

*The following table shows the dimensions of a certain number of yellow bodies of the human female, at various periods of gestation, and after delivery.

		Yellow bodies.		Observations.
		Long diameter.	Short diameter.	
Period of Pregnancy.	25 to 30 days.	18 Mill.	15 Mill.	It is rare to find a yellow body of a spherical form, or with equal diameters, whatever may be the direction in which the section is made. It undergoes, in its development, a sort of compression, similar to the ovary. The measurement of the long and short diameters are only given, taken from a section of the yellow bodies, the
	about 40 days.	24 "	16 "	
	2 months.	24 "	13½ "	
	3 months.	25 "	18 "	
	In the 4th month.	15 "	15 "	
	" " "	13½ "	10 "	
	" " "	13 "	10 "	
	In the 5th month.	15 "	15 "	
	5 months.	13 "	13 "	
	In the 6th month,	12 "	12 "	

greater than that of the ova found in the uterus. But these exceptions do not invalidate the general rule.

There is a marked difference between the yellow bodies that are formed in consequence of conception, and those which arise aside from the conditions developed by impregnation. The duration of the first is longer than the second; their volume, also, is much larger, although, in reality, their nature is identical. I have too often had occasion to observe this fact upon the ovaries, of females who had committed suicide, to entertain the slightest doubt upon this point.

While, in effect, the obliterated capsules form enormous tumours in those in a state of pregnancy—requiring five or six months to pass through the principal modifications which they undergo—not more than twenty-five or thirty days are necessary, in those who have not been fecundated, in order to accomplish the, almost, total obliteration of these capsules. At the commencement, they are, alike, the seat of identical phenomena, equal in intensity; but, when unaccompanied by gestation, they soon lose the conditions necessary to following the ascending progress, suddenly become soft, and are, frequently, absorbed without the convolutions of their internal coat ever touching or contracting adhesions with each other. If it was otherwise, many yellow bodies would be found existing at the same time on the ovaries of females dying at any moment, each one having a volume equal to that of the organ in whose substance it was developed; because, ordinarily, a Graafian follicle is ruptured at each menstrual period, and after three months of existence, a yellow body has lost but little of its volume. Now, during this lapse of time, as three follicles should be ruptured, the result would be, that, in all non-fecundated females, there should be found three or four large capsular tumours incessantly renewed and reproduced. But experience proves that such is not the true state of things. The ovaries of females, who have met with a sudden death, while in the unimpregnated state, have never presented yellow bodies similar to those met with in the second and third months of gestation; they have neither the volume, density, resistance, nor the duration of the latter.

I do not hesitate to affirm then, that there is an appreciable difference between the yellow bodies, whose development commences with gestation, and those which arise at other times. Anatomists who have denied this difference, have not made researches sufficient to decide such a question. They thought that, as the ovarian capsule could be rup-

those in which it was coincident with conception. They did not reflect that there was, in the genital organs of the pregnant female, physiological conditions which were not found in the unimpregnated, and that the acts accomplished in the uterus, would, necessarily, react upon those of which the ovary became the seat. Now this is precisely what takes place. When the fecundated ovum fixes itself in the uterus, it impresses upon this organ an increase in activity, which is continued during the whole term of gestation; it extends its influence even to the ruptured capsule, and imparts to the process of cicatrization, greater intensity. This is the reason why the yellow body, found under such circumstances, assumes an increase growth, remains longer, and is distinguished, in this respect, from those that are not developed under the same conditions. If, on the contrary, the uterus remains passive, the ruptured capsule, not receiving from it this unusual impulse, fails in its development, as also does the pregnancy to which the ovum, which has been emitted, should have given rise.

It is thus, according to my view, that the great difference, between the yellow body developed during pregnancy, and those that are formed in the unimpregnated state, is explained. But even if the value of this explanation is questioned, it is not the less established, by an examination of the preparations which are in my possession, that this difference does exist.

Taking this fact as a point of departure, we are led to ask, if the differential characters of the yellow body are not sufficiently characteristic to enable us, by a simple inspection of the ovaries, to say if the female has or has not been pregnant. I believe, myself, in the possibility of such a determination, and I am convinced it will lead to important applications in legal medicine. But it must not be forgotten that, in so delicate a question, such means of conviction should not be admitted except in cases of undoubted significance, because there are circumstances in which no inference could be drawn from it.

I have said, in substance, that all the ovarian capsules that are ruptured during the act of conception or in the absence of its influence, always undergo, during the first eight or ten days after the escape of the ovum, the same transformation, and that it is impossible to find the slightest difference at this epoch; but this first period passed, the travail

the proper characteristics, which they preserve even to the commencement of the fourth month of gestation. Later—during the period of decadence—it is more difficult to distinguish them from those that have not experienced the influence of pregnancy. Those who are familiar with observations similar to those of which I have spoken, will look upon this point as I do.

In a word, if an experimental physiologist is presented with a yellow body as large as the ovary itself, forming a solid resisting tumour—on a section of which it can be shown that the internal circumvolutions fill the cavity of the ruptured capsule, and are adhering and closely bound together—he will not hesitate to say that this ovary belongs to a pregnant female. If the circumvolutions are but feebly united, and a certain amount of the plastic material, which serves to agglomerate them, is seen between them, he will say that the yellow body corresponds to the commencement of the second month of gestation. If, on the contrary, these circumvolutions are intimately coalesced and already confounded in a compact mass, having always the same volume as in the preceding case, he will judge that the yellow body is from a female at the end of the third month of gestation.

This being admitted, I will now suppose that a female has died, a victim to criminal procedures, by which the expulsion of the fœtus and its appendages have been procured; upon an examination of the cadaver, the uterus is found rather more voluminous than in a state of vacuity, and the true mucous membrane affords traces of a more or less extensive exfoliation. But, are these evidences sufficient to demonstrate that abortion has taken place? Certainly not; because, without existence of pregnancy, the uterus may exhibit the two peculiarities which I have just indicated; the presumption, however, approaches much more to certainty when the ovary contains a yellow body, having the organization of those characteristic of pregnancy. In order to give to the phenomena, which may vary to a certain degree, their true value, it is proper to acknowledge that it is not sufficient to be prepared with a few isolated observations; it is indispensably necessary to have in view a great number of facts to be enabled, properly, to appreciate those upon which we may be called upon to pass judgment.

Since the discovery that menstruation in the human species, and rut or heat in the mammalia, was the result of the spontaneous evolution of ova—

evolution of the matured capsule does not always reach the result to which it tends. It frequently happens that these capsules abort before having run through all the phases of their development, and are absorbed without being ruptured. It may, then, happen that the ovaries of females that are examined, after rut or menstruation, offer no traces of yellow bodies. I have had occasion to verify this fact several times, in rabbits who had ceased to be in heat, as well as in females who had committed suicide some days after menstruation.

The remarkable colour of the ovarian tumours, which I have described, caused Malpighi to give them the name of yellow body, *corpus lutum*,* at an epoch in which the immortal experiments of Graaf had already shown that the development of these transitory productions—discovered by Volcherns-Coiter†—were connected with the rupture of the ovarian capsules and the function of generation. It is true that Graaf, followed by most modern physiologists, incorrectly imagined that these productions, the internal configuration of which, have a great resemblance to conglomerate glands, were produced before the rupture; that they were formed, between the two coats of the capsules, by the deposition of a certain material that enveloped what he believed to be the ovum, which, being augmented by degrees, compelled it to escape through an opening produced by the pressure. This great anatomist believed, also, that the deposition of this species of glandular matter—to use his own expression—only commenced as a consequence of conception and under its influence, because the great and sole object which preoccupied his mind was, to establish—against the prejudices of his time—that the superior vertebrates had ova, and that those ova were nothing more than the contents of the ovarian capsules.‡ Consequently every thing that tended to connect the transformation of these capsules with generation, and to place it under the influence of this act, became an argument in favour of the opinion which he defended. This is why he attached so much importance to a demonstration that it was only in fecundated females that the ovaries exhibited similar productions; and that, in virgin females, no traces of them were ever found. Science was not sufficiently advanced, to show that these two facts were unascertainable of being reconciled. But whatever may have been the ar-

observations, the difficult problem of the functions of the ovaries, he had, not the less clearly, recognised that the yellow body was a modification of the ovarian capsules.

Malpighi added scarcely any thing to what Graaf had said touching the origin and mechanism of the formation of the yellow body. He was completely deceived upon all these questions; for he took the small tubercles, which are the last vestiges of these transient productions, for the first stage of their development. He supposed that—even in the cow—the tubercles, smaller at first than a grain of millet, gradually enlarged; that, when they became the size of a pea, a cavity appeared towards their centre, filled with lymph; that when they acquired the size of a cherry, their substance, formed of irregular lobes, had all the appearance of a gland; and, in their capacity of secreting organs, each one of them had, for its function, to contribute to the formation of the ovum, and its nourishment, even up to the moment of expulsion. Now, all physiologists who, at the present time, know that the Graafian vesicles which are transformed into the yellow body in the cow, has, at first, an enormous volume, cannot doubt that the small yellow tubercles, considered by Malpighi as nascent yellow bodies, are the same yellow bodies, having arrived at the final limit of their atrophy. Nevertheless, if the anatomist of whom I have spoken, has completely mistaken the nature of these bodies, their true origin, and the mechanism of their formation, he has described, with more exactness than Graaf, the anatomical disposition which characterizes them at the epoch of maturity. He has recognised, also, contrary to the opinion of Graaf, that they are not less frequently found in the virgin female than in those who have been fecundated; an observation, the exactness of which has been confirmed by his disciple Valisnieri, and nearly all his successors, Haller excepted.

Buffon,* always pre-occupied with his famous system of organic molecules, only wished to see, in these yellow bodies, species of transitory glands, charged with the elaboration of the pretended seminal fluid of the female of the human species and the mammalia. He consequently occupied himself in attempting to discover, in the midst of the plastic lymph which they enclosed before entire obliteration, the moving mo-

Graaf, and protested, in advance, against all attempts at giving the name of ovaries to those organs, which, according to him, were destined to secrete the semen, and endeavoured to preserve that of female testicles, by which they were primitively designated.

Haller was the first, who, in following the track of Graaf, rectified the inaccuracies of the latter, and, as he admitted himself, recognised the true origin of the yellow bodies, discovered the mechanism, and the precise epoch of their formation. "*Veram originem, tempusque nascendi et ex vesicula deformationem nos addidimus.*"* As Graaf, he then saw that these bodies were nothing more than a simple modification of a ruptured capsule; but, instead of admitting, after the example of his predecessor, that they resulted from the deposition of a species of glandular matter between the two coats, of which the capsule is composed, he demonstrated that they arose from a thickening and vegetation of the internal walls, and that the excrescence, produced by these vegetations advanced towards the centre of the cavity of the empty follicle, which they obliterated after the manner of fleshy granulations.* His researches enabled him to establish also, contrary to the sentiments of Graaf, that this transformation of the internal walls of the follicle, did not commence until after its rupture; and, in this respect, his observations are, perhaps, more exact than a great many physiologists who have succeeded him. There is one point, however, upon which his accuracy was at fault—that is when he pretended to believe that the formation of the yellow body was always an indication of a previous fecundation, and that virgin females offered no traces whatever of them. He entertained so firm an opinion upon this point, that when females, whose ovaries contained yellow bodies, were brought to him, as those who had never conceived, he accused those who offered them for sale, with positive falsehood.† But this error, which he had inherited from Graaf, did not prevent him from properly comprehending the mechanism by which the ruptured capsules were obliterated, nor from indicating the moment at which their walls commenced the modification that attained this end. Aside from this mistake, from which he was unable to guard himself in consequence of the preconceived opinion, which he entertained, that the influence of the male could alone provoke the rup-

capsule; 3d, that this transformation only commences after the rupture, and not before, as nearly all modern physiologists have supposed.

When Haller had demonstrated that the conversion of the ovarian capsule into the yellow body, arose from a simple vegetation of its internal wall, it only remained—in order to have a complete knowledge of the intimate nature of this phenomenon, already perfectly appreciated by him as a whole, and in its different phases—to show that the vegetations or granulations, of which this body was formed, resulted from the folding of the internal coat of the follicle, and that their augmentation of volume was owing to a modification of the cellules which constituted its tissue.

It was M. Baer who first comprehended the mechanism by which these folds or circumvolutions were produced;* it was M. Pouchet who explained how they were thickened; I claim to have established that their colour is due, exclusively, to the nature of the molecular granules, or globules with which the cellules, which form their tissue, are filled, and not, as M. M. Raciborski and Pouchet suppose, to an extravasation of the colouring matter of the blood. I have seen, with pleasure, in a brochure, which Professor Meigs† has recently done me the honour to transmit to me, that, on this last point, this observer has arrived at the same conclusions.

Finally—and not to pass in silence, any opinions which have been emitted on the origin of the yellow body—I shall terminate this historic sketch by stating, that some authors have laboured under a complete error, when they supposed the ovarian tumours in question, to be the result of an evolution of the cells of the granulous membrane which lines the interior of the Graafian follicle. The portion of this membrane, which is not carried away with the ovule at the moment of the dehiscence, does not perform the function which they have assigned to it. It is exfoliated, dissolved, and disappears without taking any part in the formation of these bodies, which are developed independent of the elements which compose it.

[In a prize essay, published in the 4th volume of the transactions of the American Medical Association. for 1851. Dr. J. C. Dalton. ir.. claims

REVIEW.

- ART. VII.—1. *A Treatise on Operative Ophthalmic Surgery.* By H. HAYNES WALTON, Fellow of the Royal College of Surgeons in England—Surgeon to the Central London Ophthalmic Hospital, and Assistant Surgeon to St. Mary's Hospital—First American from the first London Edition—illustrated by one hundred and sixty-nine engravings, on wood—Edited by S. LITTLE, M. D., author of a Manual of the Diseases of the Eye; Surgeon to Will's Hospital for the Eye and Limb; Fellow of the College of Physicians of Philadelphia, &c., &c. Philadelphia: Lindsay & Blakiston. 1853.
2. *A Treatise on the Diseases of the Eye.* By W. LAWRENCE, F. R. S.—Surgeon Extraordinary to the Queen; Surgeon to St. Bartholomew's Hospital, and Lecturer on Surgery at that Hospital; Surgeon to Bethlem and Bridewell's Hospitals; and late Surgeon to the London Ophthalmic Infirmary—a new edition—edited, with numerous additions, and two hundred and forty-three illustrations, by ISAAC HAYS, M. D., Surgeon to Will's Hospital; Fellow of the Philadelphia College of Physicians; member of the American Medical Association—of the American Philosophical Society—of the Academy of Natural Sciences, of Philadelphia, &c., &c. Philadelphia: Blanchard & Lea. 1854.

THE scope of the two works, whose titles we have given, it will be seen, is widely different. The first is confined to the surgical *treatment*, alone, of the diseases brought under view, and, we may add, to the surgical *diseases* exclusively: everything being omitted which does not call for an operation. And in treating his subjects, there is usually no more said of the general character, the history, physiology, pathology, &c., of the diseases, than just so much as is necessary to introduce the operative process by which he proposes to effect the cure. In endeavouring to carry out this plan by which, where it can be adopted in works of this kind, the greatest simplicity and clearness of description is ensured, we are not certain that our author has not fallen into the other extreme—that of too much exclusivism—since we look in vain for many diseases which can hardly be omitted in a work intended for general use, without depriving it of that completeness which is so desirable in books of this kind.

Our author has, however, fully succeeded in laying before us his views with a degree of clearness not often attained in works so entirely practical. To a lucid and flowing style, he joins a temperance which gives an

passing condemnation on the book for not being what we might imagine it could have been, we will content ourselves with recording our judgement of it as it is.

In doing this, we will not attempt the prolix task of going over the author's ground, field by field, and examining how he has done his work in all its details, but will simply take up such portions as arrested our attention in a rather careful perusal, and without much regard to order, give our impressions.

In one or two places, we detect him straying from his usual course, and going into more detail on the etiology and physiology of the disease he is treating, than usual. Thus, on the subject of entropium, (page 127,) he enters, at considerable length, into an examination of the causes of the disease. After alluding to the various opinions on this question, he decides in favour of muscular action as the cause; and in support of his views, gives the result of his own very careful dissections of the muscular structures of the eye. These dissections were made with the especial object of determining this very question. He shows that the common opinion of anatomists of the present day, which describes that part of the orbicularis occupying the edge of the lid as the thinnest and weakest, is not correct, and that the older anatomists were right in describing this as the thickest and strongest portion of the muscle. Albinus describes this part of the orbicularis under the name of *musculus ciliaris*. Mr. Dalrymple, from his description, seems to have been ignorant of the actual arrangement of these parts.

Mr. Walton's dissections, also, convinced him that the tensor tarsi muscle has an extensive connexion with this portion of the orbicularis, and may justly be considered as a part of it—that part which has a bony attachment.

An extract from Duverney's work, written about 1761, proves that he was well acquainted with the structure of these parts. The same extract shows that our countryman, the late lamented Professor Horner, had no just claim to the credit he assumed, of first describing the tensor tarsi (page 132): while to Dr. Physick alone, among modern anatomists, is awarded the credit of having taken a more extended and juster view of the range of action of this little muscle.

With these views of the cause of the disease, Mr. Walton summarily dismisses all the operations, including those of Ware, Crampton, Guthrie, &c., heretofore employed, and substitutes one of his own.

Here it is: "Let us suppose," he says, "that the right eye is to be rectified. An assistant stands behind the patient, and having made the lid tense, by drawing it outward, and raising the brow, as is shown in the operation for Trichiasis, the surgeon should make two incisions through the skin and muscle" (as shown in a diagram, one being along the upper edge of the tarsal cartilage, the other so planned as to form an ellipse with about two lines of separation at its middle part). "The flap thus isolated, should be forcibly drawn forward, and slowly dissected by

pressure with the finger. I have never found a ligature necessary. The exposed surface must be inspected, and if any muscular fibers have escaped, the forceps and knife must be re-applied. The assistant should continue the proper retraction of the skin, till the knife has been laid aside, as essential to steady and effectual dissection. Three or four sutures should be used, and if a patient desires some local treatment, water dressing may be employed. I have operated in about fifty cases, and in not a single instance have any bad symptoms supervened. In all there has been union by the first intention."

"The cilia might appear to be in danger of being dissected off, but, in reality, they are not."

After reviewing the operations usually recommended, he sums up his argument in the following language:

"The discrepancy between these statements and those of Mr. Wilde, may, I think, be in a great measure, if not entirely, reconciled. With the removal of the cilia after his method, on the upper lid especially, he must frequently, in milder cases, remedy the curvature of the tarsus; since the excision of them cannot be effected without also removing a large portion of the musculus ciliaris and skin—doubtless enough, in many instances, to influence the entropium, and remove the cilia from the globe when uncomplicated with trichiasis. Again, a portion of the edge of the lid is also removed; for, as I have observed, it is quite impracticable to excise the cilia without doing that much. Hence it is apparent how, in the early state of entropium of the upper lid, with yet but slight alteration in the cartilage, that the operation in question may be effectual, and how it may be remedial, even when the edge of the lid is unhealthy and thickened. And as regards the under lid, the effect of entropium is generally so slight, compared with that of the upper, and so much of its severity depends on the contact of the cilia on the globe, that after their removal the case is exceptional that produces more than slight inconvenience. Thus, then, it would appear that this is the best of all the old operations, and such it certainly is; yet it possesses very serious drawbacks in the loss of the cilia, the somewhat shortening of the lid, and the destruction of a greater or less extent of the meibomian glands. There is yet an explanation needed regarding the supposed frequent success of various kinds of operations for entropium. I hope to be pardoned, if I offend, in giving what appears to be the solution. A large portion of the so-called entropia are, in reality, cases of trichiasis. In the upper eye-lid of the aged, the skin is apt to overhang the cilia at their bases and simulate entropium, and if with that there be trichiasis, an incorrect diagnosis is very probable. And a somewhat similar deception is likely to occur in the young, when, with the inversion of the hairs only, the skin of the lid is swollen from the inflammation that the trichiasis excites.

"Cutting off the edge of the lid along with the cilia, partakes of the imperfections of ordinary amputations; and in its kind, even surpasses them in evil, from physiological relation of the lids, and the adaptation of its edge to the other parts of the visual apparatus; as a general principle, it would be even worse than removing a thigh to cure a popliteal aneurism, or a leg, to remedy a distorted foot. Although its gen-

eral application is to be reprobated, the peculiarity of a case, and the limitation of our resources, may render its especial adoption expedient, and even to be preferred to the last operation. If any case calls for such an extreme measure, I imagine it to be that of severe curvature of the tarsal cartilage of the upper lid, with considerable and extensive changes in the physical character of the edge of the lid, from long-continued inflammation, by which it has been rendered very thick and very hard, and unlikely to yield to other means of remedy. I cannot conceive that excision of any portion of the tarsus can ever be required in the lower lid."

As connected with this subject in a measure, we may lay before our readers Mr. Walton's opinion on a debated point, viz: the possibility of additional hairs forming on the cartilage in cases of trichiasis. He arrives at a negative conclusion, on the ground that the hair bulbs are original formations, and cannot be produced by disease. When surfaces which have been repeatedly blistered, present the appearance of an increased quantity of hair, this is owing to the development from irritation of bulbs already existing, since the human body at birth, is as fully covered with hair bulbs as that of any animal. (p. 112.)

This argument, we may remark, does not disprove the position taken by others, since he admits that their may be dormant hair bulbs in the lids.

In a work intended to be of an eminently practical character, we cannot but think that serious remarks on the manner of transplanting cilia are, at least, out of place. That surgical dilettanti might pride themselves on saying everything that a subject would possibly admit of, we can conceive; but that an every-day man, writing for every-day people, should think himself called upon to occupy the time of his readers with directions for laying out and planting, pruning and cultivating, a plantation of eye lashes, savours less of the real than the visionary. It may be well, as a physiological experiment, to try these "*tours de force*," but to recommend, or even appear to recommend them, to students, or young practical surgeons, is, in our opinion, out of place.

In the chapter on Nevus, (page 208,) as well as in that on malignant affections of the eye, (page 492,) our author has largely deviated from his course, and gone extensively into the physiology of the diseases. The first of these chapters is particularly full, and will amply repay careful perusal. The various forms of disease, more or less approaching this in character, are fully brought under review, and the different operative plans candidly and carefully laid before the reader. But there is nothing new offered on the subject, and one may, therefore, pass to the chapter on malignant growths.

We cannot better give an idea of this interesting chapter, than by making an extract, which we will take from the description of "encephaloid cancer of the eye ball." Dividing it into three stages, he describes the first stage thus:

pearance of a cat's eye, when seen in the dark. At the same time the iris will be found to have changed in colour, being rather darker than that of the other eye; the pupil is dilated and sluggish, and vision, even at this early stage, is nearly or altogether lost. Slight external inflammation, lachrymation and intolerance of light, occur in some instances, but these are by no means necessarily, or even frequently, present in the early stages; more usually, the child shows no indication of pain, and has every appearance of health. As the disease advances, the cause of the metallic reflection becomes evident. It is now seen to be owing to a tumour springing up apparently from the fundus of the eye, of an irregularly rounded form, generally divided superficially into two or three lobes, and traversed by one or two small blood-vessels, which ramify on its surface; in colour, it varies from a deep orange hue, to nearly white, the most usual being, perhaps, a bright canary yellow. This growth gradually increases in size, approaches the front of the eye, causing absorption of the vitreous humour, and presses upon the lens, which becomes opaque, and in most instances, is ultimately absorbed; previous to this, however, it is pressed forward against the iris, which loses whatever brilliancy it may have retained, changes to a greyish brown colour, and is thrust forward against the cornea, both the anterior and posterior chambers of the eye being completely abolished. Towards the conclusion of this stage, the eye is undergoing considerable tension from the internal pressure: it feels hard, its motions are limited, there are frequent attacks of external inflammation, with epiphora, and the child evidently suffers frequent and severe paroxysms of pain, becoming restless, feverish and emaciated.

"Second Stage.—The external parts of the eye now undergo a change; the cornea expands and becomes opaque; the sclerotica thinned by absorption, allows the dark-coloured choroid to shine through it, and is irregular in outline, rising into small dark-coloured knobs, where the pressure has been most severe, or the absorption most rapid. In many instances, it is so densely covered with large varicose vessels that no alteration in colour is perceptible. The eye-lids now become oedematous, and the eye-ball prominent, and apparently, greatly enlarged in size. I say apparently, for though I admit that enlargement takes place to a considerable extent, I cannot conceive the possibility of the dense fibrous structures of the sclerotica expanding to two or three times its normal dimensions, as some authors have asserted. I agree, rather, with Dr. Argyll Robertson, that 'it is probable that, in most instances, these writers have been deceived by an unnatural prominence being given to the eye by the infiltration of the cellular tissue of the orbit projecting the eye-ball forward, thus giving it the appearance of being augmented in bulk.' This stage terminates in the escape of the fungus from the interior of the eye; and this may take place either through the cornea or the sclerotica. In the former case, the cornea becomes opaque and

ing the latter part of this stage, when the eye is subjected to great distension, the sufferings of the patient are acute; the pain generally comes on in paroxysms, extends over the whole side of the head, and even down the neck, and is accompanied by the usual symptoms of febrile excitement. Great relief is experienced when the fungus escapes, and the distension is removed.

"Third Stage.—The tumour, now released from pressure, rapidly increases in bulk, so as, in a short time, to distend the orbit and eye-lids, and efface all appearance of the eye. When it has escaped through the sclerotica, at some little distance from the cornea, it is, at first, covered by the conjunctiva, and this may remain entire until the tumour has attained a considerable bulk, when it is finally destroyed by ulceration and sloughing. In other cases of less frequent occurrence, the sclerotica gives way at the back part of the eye, so that the fungus escapes directly into the orbit. Under such circumstances, the eye-ball will be displaced in various directions, according to the position of the protruded mass, until finally, the latter has acquired sufficient bulk to extend beyond the margin of the orbit, when it overlaps and conceals any remains of the eye-ball that may have been visible.

"The fungus, on its first escape, is soft, and generally of a light red, or yellowish colour; this appearance, however, is rapidly changed as it increases in bulk; the surface becomes irregular and covered with ulcerations, which discharge profusely; large, livid and fungoid granulations form, which bleed freely on the slightest touch; interstitial hemorrhage takes place, forming clots in the interior of the growth, while large masses are detached from the surface by sloughing, giving rise to fresh bleeding, in some instances so profuse, as to prove the immediate cause of death. No diminution of the size of the tumour ensues from the separation of the sloughs; on the contrary, the growth seems to take place with increased vigour, and is limited only by the length of time that the patient survives. I have already spoken of the enormous magnitude to which encephaloid sometimes attain; while masses of the size of a large orange, or a man's fist, are not unfrequent. The skin of the distended eye-lids, and of the adjoining parts of the face, assumes a dark livid hue, and is traversed by large varicose veins; and the glands of the neck, and under the lower jaw, are enlarged; and, in exceptional cases, proceed to ulceration. Death finally ensues, in some instances, from extension of the disease to the brain, when it is preceded by coma and convulsions; in others, from constitutional irritation, and the exhausting effects of pain, hemorrhage and discharge."

We offer no apology for the length of our extracts, since the object of our notice is to give an idea of the work we are reviewing, and there can be no fairer way of doing this, than by using the author's own words as much as possible. On this subject, however, we must confine ourselves to reasonable limits, that we may have space left for a few remarks on one or two other sections of the book. We will, therefore, simply, in conclusion, give his views on the great question, (in a work like this,) of the value of operations in malignant diseases of the orbit. It will be right, however, to premise this with the statement that our author fully coincides in the opinion, now so ably maintained by his dis-

tinguished countrymen, as well as others, that cancer is a constitutional, and not a local disease. That it depends upon some, as yet, imperfectly understood, error in the early stages of development of the elements of nutrition, and is, therefore, not to be cured by an operation which can do no more than remove a deposit of these blighted elements, while it attempts nothing for the constitutional change, which, if it could be effected, would alone offer a chance of permanent cure. For the more malignant forms of the disease, such as encephaloid and advanced schirrus, the knife is not only a fearful, but a useless remedy. The disease will, most probably, be reproduced, either in the same or some other place. Though in some cases of melanosis, where the disease is circumscribed, an exception to this rule may be made. And perhaps the same may be said of schirrus, in its earlier stages.

We had marked some other passages for quotation, but find that we are overrunning our limits and must, therefore, omit them, and only make passing reference to a few matters of interest.

We would invite attention to the chapter on "Incisions of the conjunctiva and the subjacent cellular tissue in chemosis, from purulent ophthalmia." The treatment, especially of the gonorrhœal form, of this affection, is extremely judicious. Any light thrown upon the management of this intractable disease, is peculiarly acceptable. (p. 238.)

We are rather surprised to find Mr. Walton so stoutly denying the effect of inflammation in the production of conical cornea. Authors of the highest reputation, who have preceded him, give cases which prove, beyond a doubt, that such may be the origin of the disease. Our author is too ready to deny the action of certain causes in the production of diseases, because he has not been able himself to trace them. The direct testimony of an observer of established reputation, ought to be at once received as evidence of a fact. The extent to which facts may be generalized into causes, may be subject of doubt; but a properly recorded fact, from a competent observer, ought always to be allowed its just weight. (p. 368.)

The chapter on Cataract will be found interesting, but we think that the author must be rather loose in his expression when he says, at page 387, "In infancy it (lens) is nearly spherical." All authors describe it as approaching more to that form in infancy than in after years, but the expression used by Mr. Walton would lead a young reader to infer, that the shape of the lens was subject to a greater extent of change than is really the case.

In regard to the propriety of operating for cataract, our author lays down the rule as follows: not to operate so long as the eye may be rendered available for ordinary purposes, whether by the use of optical appliances, or by the habitual use of belladonna, for dilating the pupil. "The advantage to be derived from temporarily widening the pupil, is remarkable, and not less available when there is slight, yet general opacity, than when the opacity is partial but denser, and that, whether the lens or capsule or both, be involved. The benefits of the dilatation are usually more lasting in partial cataract, because the partial affection may be stationary. I have seen a house-maid, eighteen years old, with partial capsulo-lenticular cataract that almost blinded her, when the pupil was in the nat-

ural state, but when it was kept dilated by a solution of belladonna, applied three times a week, she executed her work so efficiently, that her employer was unaware of her defect. The belladonna had been used since childhood. It is one of the peculiarities of the drug, that its effects do not wear out; the same quantity always produces the same result, at any interval, or for any prolonged period. Its neutral salt, the sulphate of atropia, is the most elegant preparation." "Its employment for the purpose under consideration, need not be oftener than every second or third day." (p. 407.)

The formulæ he uses, is grs. ii. or iii. to water 3 i., a drop or two, applied three times a day, on the conjunctiva of the outer part of the lower lid, will produce all the effects of dilatation. (p. 43.)

With regard to the operation to be selected, he says, that displacement is essentially a bad one, because the cataract is then rendered a foreign body within the eye. It is only applicable when extraction would be dangerous, or is positively unsuitable, and ought, he thinks, to be regarded merely as an operation of expediency. (p. 409.)

The necessary preparation of the patient, as well as the circumstances contra indicating an operation, are set forth at page 442. We may mention that he does not consider age alone, as one of these, and mentions a successful operation on a patient between ninety and one hundred years old. He decides the question of operating when only one eye is affected, decidedly in the negative.

Hard cataracts should be extracted. Soft ones, after being lacerated or broken up, left in place; and capsular, as a rule, extracted, though they may, under peculiar circumstances, be torn up and left.

For his favourite operation of extraction, he uses a modification of Beer's knife, measuring from the point to the shoulder 8-10 of an inch, and across its broadest part, 4-10. He divides the cornea upwards, and takes his place behind the patient. We will not follow the operator in his steps, but call attention to the following remark. (p. 423.) "A small bit of the iris, especially of the pupillary margin, in contact with the edge of the knife, should not be regarded, and the operation ought to be completed without hesitation. If a second pupil be made, the isthmus should at once be divided by the blunt-point iris scissors. It is recommended by several surgeons, always to attempt to free an entangled iris, by pressing it between the cornea and the knife; but I have seen many such efforts made without a single successful issue."

We have thus endeavoured to do our author justice by noticing a few of the passages in his work which have most attracted our attention. In doing so, we have not thought it necessary to accompany him, step by step, in his progress over the ground he has travelled, but have taken him up at different points, as fancy suggested. We have thought that no better plan could be adopted of giving him a fair examination than by reading his book attentively, and jotting down what struck us as we

mentation we could make. We derived both pleasure and profit in reading it, and can, therefore, promise the same to others. For, even to the wisest, it is not without profit to see what they already know with the eyes of another; while, to the beginner, there is enough to occupy time both pleasantly and usefully.

Of the American editor, we have not much to say, because he has said but little. We will, therefore, dismiss the book here, with our hearty approval.

Of the second book on our list, it is not necessary for us to say much. It has been before the public long enough for them to have fully made up their estimate of it. What that estimate is, we may see upon the title page. The third American, from the second London edition, of a work upon a subject so largely written upon latterly, could only be called for by a very high appreciation of the merits of the book. We will not, therefore, attempt anything more than a simple reference to the welcome appearance of this edition of Mr. Lawrence's highly valuable work, and an expression of our unqualified admiration of the manner in which the American editor has accomplished his task. A work of the intrinsic merit of the original treatise, enriched by such additions as were necessary to bring it up to the improvements of the day, is just such a guide as should be placed in the hands of students, and we feel full confidence in recommending it to them.

The manner in which both these works have been published, are highly creditable to the well-known houses from which they issue. The plates, particularly, are deserving of commendation. We find a few verbal errors, but they are, for the most part, unimportant, and we may terminate our notice by an unreserved recommendation of both the works.

BIBLIOGRAPHICAL NOTICES.

ART. VIII.—*The Elements of Materia Medica and Therapeutics*. By JONATHAN PEREIRA, M. D., F. R. S., &c., &c. Third American edition, enlarged and improved by the author. Edited by JOSEPH CARSON, M. D. Philadelphia: Blanchard & Lea. 1854. 8vo, p. 1226. (Through Mr. John Russell.)

The first volume of this revised edition, appeared Nov. 1851, and the late appearance of the second, is thus explained by the American edition.

“As will be perceived from the preface to the first volume, the au-

the residue has been issued under the auspices of the distinguished editors, Drs. G. O. Rees and Alfred S. Taylor.

In executing the difficult task of completing what had been so ably commenced by the illustrious author, the editors say, they have endeavoured to act in accordance with his views, as embodied in a large collection of notes and memoranda which were entrusted to them for this purpose. They have, on their own responsibility, made such alterations and additions, as the present state of science appeared, in their judgment, to render necessary; and the changes made in the preparations of the London and Dublin Pharmacopœias, since the publication of the previous edition, have been duly noticed.

The American editor has made such additions as were necessary to conform to the United States Pharmacopœia, and has inserted more finished representations of plants, where an opportunity presented.

The illustrations are more numerous than in former editions, and the work is, in every way, improved and enlarged; being, in fact, a complete Encyclopædia of Materia Medica, superior to any work on the subject, hitherto published.

ART. IX.—*A Practical Treatise on the Diseases of Children.* By J. FORSYTH MEigs, M. D. Second edition, revised and enlarged. Philadelphia: Lindsay & Blakiston. 1853. 8vo., p. 711. (Through Mr. John Russell, King-street.)

The first edition of this work formed one of a series, entitled the "Medical Practitioner's and Student's Library." The present is much enlarged and improved. An introductory chapter of over thirty pages, is added on the clinical examination of children; and many of the articles are re-arranged, and some re-written, rendering the work still more worthy of that support which was accorded to the first edition.

It is to be regretted, that the author has entirely omitted mention of several subjects which are usually included in a treatise on the diseases of children. But it is to be hoped, that further opportunity will be afforded him (by the exhaustion of this edition) to embrace, in a subsequent one, every subject properly belonging to the department to which he has devoted his attention.

ART. X.—*A Treatise on the Venereal Diseases.* By JOHN HUNTER, F. R. S.

of the most important views of Hunter, particularly to his division of constitutional syphilis into two periods, and his belief in the non-contagiousness of secondary symptoms, "Hunter's Treatise on the Venereal Diseases" has served as a basis to a structure which M. Ricord has reared—complete in all its parts—as yet invulnerable to the assaults of an embittered opposition. It covers the whole ground of venereal diseases, and embraces all the views contained in M. Ricord's published letters.

It is got up in the publisher's best style, and is embellished with eight beautiful plates, illustrative of the anatomy and pathology of the generative organs, and the instruments employed in the treatment of stricture and other affections of these organs.

ART. IX.—*On the Use and Abuse of Alcoholic Liquors in Health and Disease.* Prize Essay. By WILLIAM B. CARPENTER, M. D., F. R. S. With a Preface. By D. F. CONDIE, M. D. Philadelphia: Blanchard and Lea. 1853. 12m. p. 178. (Through Mr. John Russell.)

On the first appearance of this work, in 1842, we presented our readers with an elaborate review of it, which superseded the necessity of more than a notice of the present cheap edition, which the publishers have issued with a view to extend its circulation, and place the work within the reach of every one.

The various technical phrases which occur in it are explained, to adapt it to the ready comprehension of the unscientific reader.

The publishers have prepared copies in *flexible cloth*, suitable for mailing, which they will forward, *free of postage*, to any part of the United States, on the receipt of fifty cents. To societies and others desirous of distributing the work, it will be furnished in the same style, fifteen copies for \$5.00; one hundred copies \$30.00 net cash, to be forwarded as the purchaser may direct, at his expense.

ART. XII.—*The Medical Formulary*: Being a collection of prescriptions, derived from the writings and practice of many of the most eminent physicians in America and Europe, together with the usual dietetic preparations and antidotes for poisons, to which is added an Appendix on the endermic use of medicines, and on the use of ether and chloroform. By BENJAMIN ELLIS, M. D. Tenth edition, revised and extended. By ROBERT P. THOMAS, M. D. Philadelphia: Blanchard and Lea. 1854. 8vo. p. 296. (Through Mr. John Russell.)

The above title explains, sufficiently, the contents of the volume; and the number of editions through which the work has passed, attests its merits and predict a favourable reception for this latest and revised

editions. It will prove particularly useful to students and young practitioners, as the most important prescriptions employed in modern practice, which lie scattered through our medical literature, are here collected and conveniently arranged for reference.

ART. XIII.—*A Practical Treatise on Inflammation of the Uterus. Its Cervix and Appendages, and on its connexion with Uterine Disease.* By JAMES HENRY BENNET, M. D., member of the Royal College of Physicians, &c., &c. Fourth American, from the third and revised London edition. Philadelphia: Blanchard and Lea, 1853. 8vo. p. 430. (Through Mr. John Russell.)

The doctrines advanced in the previous editions, have been so long before the public, and so generally adopted, that we will not supererogate by advocating them at this time. This edition has been carefully revised and altered, and various additions have been made, which render it more complete, and, if possible, more worthy of the high appreciation in which it is held by the medical profession throughout the world.

A copy should be in the possession of every physician.

ART. XIV.—*Fracture Tables.* By FRANK H. HAMILTON, M. D., Professor of Surgery in the University of Buffalo, &c. With a supplement by JOHN BOARDMAN, A. B., comprising, in all, an analysis of 461 cases of Fractures. (From the Author.)

These tables show the results of treatment in one hundred and thirty-six cases. One hundred and eighteen were cases of fracture of the long bones, in forty-two of which, shortening occurred, being over one-third.

Of eighteen cases of fracture of the clavicle, shortening occurred in nine cases: in two cases, one and a half inches; in two, one inch; in one, three-quarters of an inch; in three, half an inch; and in one, one-fourth of an inch.

Of fourteen cases of fracture of the humerus, shortening occurred in three cases: in two cases, one-fourth inch; and in one, three-quarters inch.

Of thirty cases of fracture of the radius and ulna, no shortening occurred.

Of seven cases of fracture of the neck of the femur, (one within the capsule,) shortening occurred in four cases: in one case, one and a half inches; in one, three-quarters of an inch; and in two, half an inch.

Of twenty-one cases of fracture of the shaft of the femur, shortening occurred in fourteen cases: in two cases, two inches; in two, one and a half inches; in four, one inch; in one, three-quarters of an inch; and in two, half inch.

Of six cases of fracture of the tibia and fibula, separately, no shortening occurred. But of twenty-two cases of fracture of both these bones,

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shortening occurred in twelve cases : in one case, one and a half inches ; in four, one inch ; in five, half inch ; and in two, fourth of an inch.

In a case of fracture of the neck of the femur, within the capsule, the shortening was one and a half inches, no splint being used. The surgeon was prosecuted. Verdict for defendant. In three other cases of shortening, prosecutions were resorted to. In one, the suit was withdrawn ; in another, the verdict was for the defendant ; the third was not decided when the tables were compiled.

The value of these tables must be evident from the recent prosecutions of surgeons in cases of shortening of fractured members. The profession are deeply indebted to the author for the same.

ART. XV.—*Improved Method for the Relief of Vesico-Vaginal Fistula.*
By J. MARIAN SIMS, M. D., of New York, late of Montgomery, Ala.
(Extracted from Amer. Journal of Med. Sciences.) (From the Author.)

In the January number of our last volume, when noticing the Report on Surgery, by Dr. A. Pope, as presented to the Medical Association of the State of Missouri, we transferred to our pages his condensed account of Dr. Sims' method for the relief of Vesico-Vaginal Fistula, to which we refer our readers, deeming further notice unnecessary.

• ABSTRACTS

FROM FOREIGN AND DOMESTIC JOURNALS.

ANATOMY AND PHYSIOLOGY.

On the Modus Operandi of Fecundation. By Dr. WALDO I. BURNETT. (Virginia Medical and Surgical Journal, from Silliman's Journal.) With every inquiring mind, there is a deep interest connected with the development of animal life. To watch the origin

dividual development—the mysterious conditions of fecundation. To observe, after nature has prepared the material, how she puts up a new structure, and to trace the adaptive idea in the laying of each part, require but opportunity united with careful diligence and patience. But to lift the veil, beneath which lie hidden the more than mysterious relations of individuality, this is to tread on the confines which separate the material from the immaterial world.

There is no question in physiology so difficult, and at the same time so interesting as—*How* is a new individuality started by the conjugation of the sexes; and where so little could be observed, there has been more scope for speculation.

In modern times, however, with certainly better instruments, if not better opportunities, we have looked for less talk and more knowledge; and in this respect, it may be justly said that we have approached pretty near that boundary, which, as it is the limitation of that which can be perceived by the senses, is the real confine between the known and the unknown in physical science.

As it would be profitless to notice the labours of those numerous men, who, in this department, have written upon what they really knew nothing, yet speculated much, we shall attempt to show the state of our real knowledge on this *ultimatissimum* of physiology—the *modus operandi* of fecundation.

Modern histological studies have, we think, pretty definitely settled two fundamental and important points: 1st, that the ovum is, morphologically, only a nucleolated cell; and 2nd, that the sperm cell is the true homologue of the ovum.

The ovum (fecundated) produces the embryo; the sperm cell the spermatocytic particle. The embryo and the spermatocytic particle are the correlative representatives of the female and the male sex. One is the metamorphosed nucleus (vitellus) of the one; the other, the metamorphosed nucleus (nucleus of the daughter cell) of the other. In both, the ovum and the sperm cell, the process of segmentation seems a necessary preliminary to the evolution of the new being.*

The strict correlation between the essential products of the sexes, is as wonderful as it is beautifully suggestive of the unity and simplicity of plan by which nature proceeds. This point, so seductive in all its relations, might be dwelt upon in detail, but we will continue with main and general facts. The ovum, as a nucleated or nucleolated cell, continues to grow, and whatever size it may attain to by the endogenous formation within its capsule of new cells, yet, when complete, it is,

We have already said that the sperm cell is the analogue, or more properly *homologue*, of the ovum; its origin and development, as we have traced them in all their details, are precisely the same as those of the ovum. The sperm cell increases to a definite size, its nucleus (vitellus) then regularly segments, 2, 4, 8, 16, &c., and the results of this segmentation, are daughter-cells. The condition of the sperm-cell at this moment, is like that of the ovum produced by the same process of segmentation. I mean the *mulberry-like* condition. But at this point, there is a digression, for with the sperm-cell the nucleus of each of the daughter-cells is changed into a spermatid particle, while with the ovum, the whole mass is metamorphosed into the new being by a process of substitution.

The spermatid particle, then, is only a metamorphosed nucleus of a cell, and, perhaps, were the analogy carried out completely, each daughter-cell would be the representative of a miniature ovum.

Physiologically, the phenomena we have thus briefly described, obtain equally in the vegetable kingdom; for, as recent discoveries have shown, even in the simplest cellular plants there is a conjugation of two kinds of cells, the product of which terminates in a new generation; in the other plants, the superior cryptogamia, and the phanerogamia, there are parts which, in a developmental as well as morphological point of view, correspond to the essential male and female products of animals.*

Throughout the organized world, therefore, the conditions which wait upon the true generative process are the same—the combination of the representative products of two distinct sexes—and these products, whatever may be said of their form, are always physiologically the same—they are cells or cell-products.

Here we would make a general statement which embodies a great deal of physiology on this subject: A true generation must be regarded as resulting only from the conjugation of two opposite sexes, from a sexual process in which the potential representatives of two individuals are united for the elimination of one germ. The germ power thus produced, may be extended by gemmation or by fission, but it can be formed only by the act of generation, and its play of extension and prolongation by budding or by division, must always be in a certain cycle, and this cycle is recommenced by the act of the new conjugation of the sexes.

In this discussion, we have satisfactorily reached this point that the ovum and the spermatid particle are the potential representatives of the sexes to which they respectively belong. From their union results the condition of fecundation; the grand question now is, what is the *modus operandi* of this fecundating act? Bischoff's view, based upon speculative probabilities rather than upon observation, is, that contact alone of the spermatid particle with the ovum being sufficient for fecundation,

tion in chemical conditions as enunciated by Liebig (see loc. cit., p. 425.) But if catalysis embodies conditions in the organic like those of its relations in the inorganic world, it falls very far short of affording the requisite explanation of these phenomena, as we hope soon to show. This field of probabilities and possibilities we shall enter upon again.

Newport's contributions upon the physical phenomena of this subject are far the most complete that we have, and being the results of a most trustworthy observer, they deserve our special attention.

Newport's experiments and observations show, in brief, that contact alone of the spermatie particles with the ovum is requisite for fecundation, that each ovum requires several particles; and that there must be *duration* of this contact. Here is a limit to observation of physical facts, and we regard these important data worthy of full trust, considering the source from which they come. This author discusses briefly the question of the impregnative power, and from the fact that the spermatie particles are sometimes seen to disappear on the surface of the egg-envelopes, he thinks it may be fair to conclude that the agency of this body is material in its operation; on the other hand, the fact of a mere *momentary contact* producing changes in the ovum, suggests in his mind the so-called catalytic power of certain known bodies. But he thinks that neither this last, nor endosmosis, are sufficient to account for the phenomena of this grand act.

The view of Keber, has at least the merit of being unique if nothing more. As long ago as 1838, Martin Barry* announced that he had observed spermatie particles without the ovum. It should be mentioned, however, that long previous to this, Prevost and Dumas,† in their researches, found these particles within the envelopes of the eggs of frogs. But Keber's alleged discovery is, that the introduction of the spermatie particles within the ovum, takes place through a special opening, a kind of micropyle, or an infundibuliform passage. This discovery was made upon the eggs of muscles (*Unio* and *Anodonta*.)

The announcement of the presence of such a structure on the ovum is indeed wonderful, and more especially so since other observers, whose attention has been particularly directed to the embryological study of these animals, have failed to notice it, although one would suppose that an apparatus of this kind must be very visible. Keber affirms that he has observed a like structure in the ova of some other animals which he has examined. But, however well fortified he has sought to make his observations, they certainly need more than the usual confirmation, and we cannot but regard it as far from being a settled fact in embryology, that the ovum has a direct structural communication externally for the ingress of spermatie particles to its interior.

After all this discussion of facts we revert to the primary question

riences peculiar changes, which terminate finally in the evolution of a new being possessing the characteristics of the male as well as the female parent. It is true that, as was observed by Prevost and Dumas, and as has since been confirmed by Barry, Newport, and others, the spermatic particles may force their way through the envelopes of the egg some distance into its interior, but we regard this as an unessential condition of the fecundatory act; adhering by their heads to the envelopes of the egg, the incessant action of the tails of these bodies would obviously tend to force them inwards, and especially through such homogeneous, soft tissues as the egg-envelopes.

By referring to the resultant phenomena of this fecundating process, we may perhaps gain some insight into the conditional, if not the real, nature of its agency. We have already said that the spermatic particle is the potential representative of the male; what signification is it to be attached to its mere physical form, that is, whether it is conical, globular, &c., we know not; and this seems the more hidden from our perceptions, from the fact, that exactly similar forms and sizes—in fact, physical relations apparently identical—belong to spermatic particles of animals as widely dissimilar as could be. This fact alone, of the correctness of which we are well assured from our own observations, should be sufficient to convince us that we have here to deal with no very simple relations or properties. But let us pursue the subject a little further. I scarcely need remark, that the offspring partakes equally of the physical peculiarities of both parents. It will at once be perceived, that in no way can the child receive those of the father except through the medium of the spermatic particles. And so, however strange it may appear at first, yet the conclusion is irresistible, that spermatic particles must contain, concealed within them, in fact must be the vehicles of, not only the general peculiarities of the father as an animal, but mental dispositions also, and as is too often true of our own species, morbid taints superadded to all. It is wholly insufficient to say with Bischoff, that these anomalous conditions belong to the catalytic action; or with Newport, that they may be the exemplification of a force, peculiar and *sui generis*. For there is something above and beyond the wakening of latent forces, of one particle that is positive, with another that is negative. The grand fact is, that the act of fecundation includes—whatever may be said of its also vitalizing the ovum—the communication or the transmission of the *individuality* of the male parent to the ovum; and the material organ of this transmission is, exclusively, the spermatic particle. We cannot see that these phenomena have an adequate explanation in any chemical relations of matter yet known, and what is more, we cannot conceive the possibility of this ever being the case, unless, indeed, chemistry gets beyond the domain of physics. Not but that chemistry involves points which are equally obscure, such as affinity, isomorphism, &c., but then the conditions of vitality, and especially the grand manifestation of it in question, certainly seem to us to include

organic life.* *Individuality* is the distinguishing feature of organization, and we recognize in it something more than a mere collocation of physical conditions; we regard it as an idea which exists before organization, which last is only the language in which the idea is expressed. The conditions of this process of fecundation which we have just reviewed, will accept no other explanation, say what physiologists may about the *unphysical* character of such a view; we must have something beyond mere *combination*, which lies with physics; this we have in *development*, which lies with life.

In conclusion, we may say, that as the domain of science lies with demonstrable phenomena, so its legitimate study is with the sensible and tangible. The conditions of immaterial agencies, and their relations with material forms, must be accepted as pure phenomena incapable of the analysis of ordinary scientific facts. But after all, how much more of an enigma is the process of fecundation than the essence, the primordial cause of everything connected with both the inorganic and organic world about us. Science should put out her long, tentacular arms in all directions, laying hold of the tangible and the sensible, but it should be remembered that the supra-sensible is beyond her pale, and that "*multa esse constant in corpore quorum vim rationemque perspicere nemo nisi Qui fecit potest.*"

PATHOLOGY AND PRACTICAL MEDICINE.

Quinine in Pregnancy.—By Dr. G. H. HENRY. (Boston Medical & Surgical Journal.) I have noticed in several other medical journals, and, also, in your own, reference made to the effect of quinine upon pregnant women.

Quite a number of physicians seem disposed to attribute to quinine an ectrotic action hardly inferior to that of ergot. According to my experience, they have mistaken the *cause* of the phenomena which they ascribe to the use of quinine; they attribute to the *remedy* what, I think, is a concomitant of the disease.

Every one who has had occasion to treat cases of remittent and intermittent fever, has noted that the spinal column, and especially the lumbar region, has been the seat of pain and uneasiness; and in the *enciente* women we would naturally expect the womb and its appendages to sympathize with the general derangement of health. Experience proves this to be the fact, and at the exacerbation of fever and the paroxysms of an intermittent, we almost invariably find the woman suffering from bearing-down pains, and other symptoms which, in the natural course, would be the precursors of labour.

It may, and will, happen that the administration of quinine will fail

that abortion follows, not as a consequence of the remedy used, but of the disease, which the remedy had not fully controlled.

My own conviction is, that the abandonment of quinine in the treatment of malarious diseases, and the use of mere palliatives in its stead, would prove fatal to the hopes of a pregnant female—and that the accident of abortion, now so rare, would become the rule, by ceasing to be the exception. I would not give quinine rashly and heedlessly; but when combined with a full sedative dose of opium, I believe it may as safely be administered to the parturient woman as a dose of senna or oil.

I have no doubt that these views are those of a majority of the profession; but I think it necessary, when we are wishing to discard our sheet anchor in the cure of fevers, that those who have weighed it in the balance, and found it *not* wanting, should express their disapprobation of sentiments, which, if encouraged, are calculated to shake our confidence in the treatment of a simple disease.

Opium in Irritable and Anæmic states of the Brain in Fever.—By HUMPHREY SANDWICH, M.D. (Iowa Med. & Surg. Journal.) “The employment of opiates in cerebral affections,” says Dr. Holland, “is a question of much interest and various difficulty;” and “there is a great scope for farther research on this subject, as on all that relates to disorders of the brain, and a strong presumption that opium is capable here of larger and more beneficial application than has yet been given to it.” His subsequent remarks, in the same article, “on the use of opiates,” embrace, but are not restricted to, its use in fever, and may be consulted with advantage. Meanwhile, the profession owe Dr. Latham a large debt of gratitude for his masterly sketch of those irritable and anæmic states of the brain in fever, which demand the cautious use of this powerful narcotic. His brief, but comprehensive, paper on this subject, published twenty years ago, is still a beacon to guide us in a path which his observant genius first irradiated.

The class of cases of purely irritable states of the brain is to be discriminated, as Dr. Latham shows, less by any *series* of symptoms flowing from the brain, than from the *single* symptom of a state of protracted wakefulness. Nor is the wakefulness pathognomonic *per se*, but, to warrant the use of opium, it must occur in combination with an irritable state of the nervous system, induced either by depressing moral agencies, or by the physically exhausting one of alcohol. The fever may be mild, and “exhibit a *sort of contrast* with the existing affection of the brain;” or it may correspond, in severity, with the sensorial dis-

been calculated to do an abiding injury to the nervous system, and who have been long actually suffering from such injury."

The same acute observer, however, recognises another variety of sensorial disturbance in fever, which is obviously associated with anæmia. We shall quote his words.

"Again, I have seen the sensorial affections incident to fever, which require opium for their cure, manifest themselves in another form. There has been high vascular action from the first; and *large depletion* has been required to subdue it, and to guard particular organs, and especially the brain, from injury. Under such treatment, all has gone on successfully, and the patient has reached the point of convalescence, with a soft pulse, a cleaning tongue, no pain, and refreshing sleep for two or three days; when, suddenly, (tongue, pulse, and all other circumstances continuing the same,) some strangeness of manner has arisen, and the wildest delirium, and then the unrestrained passage of the evacuations. I have known the transition from such a state of *convalescence* to such a state of peril, take place in a few hours; and I have known the patient again brought back to a state of convalescence in twenty-four hours, by a moderate dose of opium. This is a rare form of disease, but one in which, when it does occur, opium is eminently indicated."

Now here was anæmic condition of the brain, but not to so frightful an extent as in the case to which I beg leave now to call the reader's attention; and, I may add, that its very extent suggests not "a moderate dose of opium," but the liberal use of the remedy.

Case.—Mrs. T., a rather delicate woman, about thirty years of age, fell into fever, during the second week in November, 1848, when near the close of the third month of pregnancy. Her abode was in the vicinity of open and offensive ditches; and it was soon evident that the attack would prove severe. The case ran on nearly three weeks, with symptoms of grave and increasing disturbance of the sensorium, and other indications of low typhoid fever. By careful management, indeed, had she not been pregnant, the disease might probably have evinced no symptoms of more than usual danger. On the 2d day of December, the irritation of the great nervous centres probably interfered with gestation, and abortion was the speedy consequence. The process, unhappily, was accompanied with a very profuse hemorrhage, which ceased indeed with the expulsion of the fœtus and placenta, but which, in twelve hours, had brought the patient into imminent peril. Along with continued sensorial disturbance, there were the signs of

the fearful struggles of agina pectoris, and, reasoning from the analogy of its virtues in fever, when the circulation is, at the same time, depressed by antimony, as also in delirium tremens, when excessive and habitual intoxication may be presumed to have exhausted the vital energies of the brain, I determined on an attempt to steady the heart, restore the capillary circulation, and calm the irritation of the nervous centres, by a full dose of opium. Mr. Millin, the surgeon with whom I was attending in consultation, fully concurred with me in these views. We accordingly gave a draught, containing a hundred drops of laudanum. The effect justified our most sanguine hopes. Sleep ensued, the circulation rallied; every symptom of cerebral irritation subsided, fever broke up, and convalescence was speedily established.

The present short practical paper contemplates chiefly that condition of the brain in fever, in which an anæmic state of its vessels warrants us in availing ourselves of the stimulant properties of opium, with a view to maintain the equilibrium of the cerebral circulation. In such cases the use of the drug is salutary, chiefly as it favours an amount of congestion essential to healthy sleep. Not that its effects on vascular structure are its only merits; for it is fair to argue with Dr. Holland that, as "narcotic substances have effects, locally applied, on nervous sensibility," so, also, "there can be little doubt, that in sleep it is the same singular influence, extended more widely over this part of organization, and reaching through the cerebral parts of it, the higher parts of our being." But in anæmic conditions of the brain in fever, the use of opium seems to be salutary, we repeat, chiefly as it favours an amount of congestion essential to healthy sleep. "It is certain, that the states of sleep and coma frequently graduate into each other, in such way as to show that the proximate physical conditions are nearly the same in both;" for, "one degree of pressure seems essential to perfect and uniform sleep, while a greater degree, without other alteration of state, assumes, more or less, the character of disease." While such changes in the circulation of the head are obviously concerned in influencing the functions of consciousness and volition, it is equally manifest that an anæmic state of the vessels is precisely the one calculated to disarm the narcotic of its dangerous properties. But for that anæmic state, the cerebral capillaries might be goaded on by a full dose of opium into fatal coma. The necessity of a healthy amount of congestion and consequent pressure, however essential to the production of sleep, may be inferred, also, from considerations regarding the nutrition of the organ.

"in praise of the great God, the giver of all good things, who has granted to the human race, as a comfort in their afflictions, no medicine of the value of opium, either in regard to the number of diseases that it can control, or its efficacy in extirpating them!" After all, most of its value depends on the discrimination with which it is prescribed. When injudiciously administered, as in sthenic cerebral excitement, or in the improper arrest of diarrhoea in certain states of fever, it has been observed to produce phrenitis, epilepsy, and coma; so, also, when indiscriminately prescribed in delirium tremens, its employment has been followed by apoplexy. Great and marvellous, therefore, as are the virtues of opium, in a variety of diseases, and admirable as are its soothing qualities in several of the forms of cerebral disorder, in fever itself, yet let no man venture to prescribe it for the latter (whether in large or small doses) in the dark, or at random. It is a sharp edged tool, and of such fearful potency, that, if it fulfils not a curative intention, it will probably destroy life. There is no instance, in the whole range of practical medicine, more imperatively demanding a sure diagnosis. In short, our warrant to prescribe it hinges on our ability to ascertain precisely that condition of the brain which alone will admit of its safe employment.

It was a rare sagacity which led Dr. Graves to employ tartar-emetic in combination with opium in those cases both of idiopathic fever and delirium tremens, in which the narcotic would probably stimulate to over-congestion of the brain, but for the depressing action of the mineral on the heart and capillary circulation. This complex practice finds its parallel in the analagous operation of opium in states of anæmia in fever.

Medical science is more advantaged, perhaps, by defining the circumstances to which our known remedies are applicable, than by exploring the resources of nature for others. Not only is great discrimination necessary in deciding on the cases which demand the use of opium, but also in regulating the doses adapted to each variety. Little can be added to Dr. Latham's admirable directions. Our rule of conduct, indeed, is suggested by the degree of sensorial excitement. "Simple wakefulness may be gently lulled to sleep by a few drops of laudanum, but wild delirium requires to be mastered, and (as it were) forced into repose by a much larger dose." Opium, however, goes much farther; and, therefore, a much less dose is required in quelling asthenic sensorial disorder, combined with fever, than when it exists alone. Five minims in the milder cases, and twenty in the graver, may be considered minimum and maximum doses in fever. Much, after all, must be left to our vigilance in watching its effects, and to our discretion in judging when to desist,

ims of laudanum produce tranquil sleep, from which the patient has awoke quite a new man; but I have also seen the same quantity produce a fatal coma, from which he has never been roused."

Dr. Latham's recommendation, therefore, is in so dubious a case, and to avoid "striking a heavy blow in the dark," to administer a small dose at intervals of one hour or two, "so as to stop short of actual mischief at the first glimpse of its approach, or be lead by a plain earnest of benefit to push the remedy to its full and consummate effect." Judicious as is this advice, we cannot but think, that on this mode of treatment of cases, in which, on Dr. Latham's own showing, "the indications for the employment of opium are doubtful," any practice which aims at once to keep down vascular action and soothe the nervous system, is a real improvement. And such, we need scarcely add, is the complex method of Dr. Graves.

It only remains that we should revert to the *anæmic* type of sensorial disturbance in fever, in that milder variety of it which, as described by Dr. Latham, we placed in contrast with a much graver form, "a moderate dose of opium," he tells us, suffices to change a state of peril to one of convalescence. And his remarks do not contemplate so frightful a form of anæmia in fever, as that of which we have given an example, and which might result equally from a large intestinal, nasal, or bronchial hemorrhage, as from uterine. He speaks of heroic doses of opium, only "in extreme cases of delirium tremens," while twenty minims of the tincture, he asserts, is quite sufficient for the purpose of subduing "the very same symptoms, carried to the greatest extremity when combined with fever." But while a "moderate dose" suffices to relieve the less urgent anæmic forms of the disease, the case related above, as having occurred to my own observation, seems to sanction a bolder practice in the perilous cases, of an extremely ex-sanguine condition of the brain in fever, coupled with the exhaustion resulting from sensorial excitement.

External Stimulus in Cholera.—(Dublin Medical Press.)—The resources of professional judgment in the treatment of cholera, are not so numerous and efficient as to render a somewhat novel remedy unacceptable to those whose lot it may be to undertake the charge of many cases. I am indebted to a lady of heroic mind and great intelligence, for the hints which led me to adopt, in the collapse of cholera, the powerful external stimulus which I shall presently describe. It appears that in the most desperate cases, even when life has appeared extinct, the native

and the feelings of the patient are greatly improved ; vomiting will also generally be put a stop to. It is probable that the application may soon require repetition, the situation being somewhat varied. In one case, now convalescent, in which death was apparently close at hand, the most complete effect was produced by a third application along the spine in the lumbar region. Time will not permit me to give details of cases, but having now tried the "brandy blister" in seven or eight cases of total collapse, I repeat, that in three it has been entirely successful, and in others has had the effect of temporarily rousing the patients ; and if, as experience has taught me, in some of these it had been repeated with more energy, greater success might possibly have resulted. The patients now convalescent say, that it is a severe remedy, but are quite conscious of its beneficial effects, and attribute to its use, without any hesitation, their restoration from impending death.

Application of the Tincture of Iodine as an Ectrotic Remedy in Smallpox. By J. CRAWFORD, M. D. (Montreal Medical Journal).—It is now upwards of nine years since I first recommended the application of the tincture of iodine as an ectrotic remedy in smallpox, and, although I observe that the suggestion has been noticed by Dr. Copland, in his Dictionary of Medicine, and by Dr. Dunglison, in his work on Therapeutics, and also by some others in the United States, I would, nevertheless, desire again to draw the attention of the medical profession to the benefit that a more extended experience has convinced me would follow a general application of the remedy.

Epidemics of the dangerous malady of smallpox, have been, fortunately, rare amongst us, and therefore, the opportunity for further testing this remedy had not been offered me, till the latter end of the last year, and earlier months of the present, during which period I have had occasion to treat, both in hospital and private practice, several cases of very grave variolous disease, and would desire to lay my further experience before the profession, anxious that a fair trial and just estimate of the application should be made, while I feel fully confident that it will maintain the reputation that I have deemed it deserving of. I would here appeal to those who have seen much of the natural smallpox, or its effects, how few cases escape pitting and unseemly scars, when the disease is allowed to run its course without interference, and I would also, ask, how many attempts have been made in consequence, to supply an ectrotic remedy, and how difficult of application, or disagreeable, and even ineffacious, are any that have been hitherto recommended. The Herculean undertaking of cauterizing the several individual pustules, in severe cases, quite precludes its application. I have reason to think the compound tincture of iodine a very powerful and efficacious remedy, which has been tried with very satisfactory results in the Montreal General Hospital by my friend Dr. Campbell : but from my being under the

city, in 1844, that I was led to try it in smallpox, from the very marked benefit I had derived from its use in erysipelas, and various other cutaneous diseases, for several years previously. I was then satisfied of its *antiphlogistic* powers and *soothing* effects, and trusted that a more general employment of it in variola, would establish its claims to general confidence.

During the late epidemic of variola, I have had several opportunities of trying its powers, and my cases have been observed by many members of the profession, to whom the issue has afforded every satisfaction. I have reason, also, to know that several medical practitioners have followed my example with success, while others have made only a very *imperfect* and *insufficient occasional* application, which neither could afford a satisfactory result, nor determine the advantages derivable from it. I have been favoured with the opinion of several physicians of this city, of the highest standing in the profession, on the advantage of using this remedy, which I subjoin. The application I have used is a saturated solution of iodine in spirit of wine, *which is to be brushed freely over the face once or twice daily, from the earliest day of the eruption that is practicable, and continuing the repetition of the application daily, or oftener, during the period of the maturation of the pustules. The earlier the application is commenced, the more efficacious it proves. The inflammatory and ulcerative processes are controlled, and the intolerable itching relieved*, by which means *scratching*, and its evil consequences, are obviated. For some time I was disposed to confine the application to the face, as being the part most disposed to ulceration and pitting, as well as that most desirable to be preserved from marks. I have, however, on many occasions, applied it to various other parts, for the sake of experiment or contrast, and also to relieve the intolerable pruritus, and have even extended it over nearly the whole body, at the patient's desire, without any evil consequence or inconvenience from the most extended application. The relief it affords to the itching (if it conferred no other boon) would, of itself, be sufficient recommendation of the application. Its antiphlogistic and febrifuge properties, however, are very manifest, and I have no doubt *modify* and *moderate* the fever, and thereby operate in a most salutary manner. The medical treatment I have combined with it is so simple and mild, that a great deal cannot be attributed to it; being merely small doses of calomel and Dover's powder occasionally, during the day, and, at night, as a sedative. When pits are left, I have observed that they principally occur in the nose, and I am inclined to think that this may, in some degree, be owing to the insufficiency of the application to this sensitive part, or from the disagreeable vapour causing irritation of the subcutaneous membrane, or even which makes the re-

the contrast of the parts "painted," with those left "unpainted;" and frequently request an extension of the application.

Aphonia of Twenty Months Standing, Relieved by Iodine Inhalation. By EDWARD B. STEVENS, M. D. (Iowa Medical Journal).—In a communication to the American Medical Association, in its Vol. of Transactions, for 1850, Prof. Pancoast has given the record of two cases of loss of voice—the one of six, the other of seven months standing—both cured by inhalation of a dilut. chlorine vapour.

In connection with these cases, Dr. Pancoast remarks: "The form of aphonia here alluded to, is that which many practitioners must have met with, following an ordinary cold, without leaving any perceptible organic lesion in the pulmonary apparatus. The voice is reduced to a faint, hoarse whisper, distinguishable only at the distance of a few feet, and at continued attempt to talk, though it gives no pain, become quickly attended with a feeling of fatigue, as though there was some obstruction to the passage of the air through the larynx. In breathing merely, there is little or no difficulty, in these cases, as the individuals are capable of undergoing considerable exertion without very unusual signs of fatigue. The difficulty has appeared to me in the paralyzed condition of the muscles of the larynx, whose business it is to dilute the rima glottidis, during the act of articulation."

The conclusion of Dr. Pancoast is, that such agent as will excite a healthy and proper degree of stimulation in the affected structure, ought rationally to restore the power of articulation. He consequently used the dilut. chlorine vapour, with entire success in the two cases referred to, at the same time suggesting that iodine, or other similar agents, would doubtless produce a similar effect.

The following case of this kind lately occurred in my practice, chiefly remarkable for the long duration of absence of voice, being twenty months, in other respects similar to those related by Dr. Pancoast.

April 6, 1853.—Miss — applied for medical advice and treatment, in a case of loss of voice, of twenty months standing, supervening upon a slight attack of influenza. Has been subject to brief attacks of hoarseness, lasting for a few days at a time, for several years. General health delicate. Since the present attack, has been subject to a great variety of treatment, including the application of nit. silv. in strong solutions within the larynx by means of the sponge probang. Nothing, however, producing any effect upon the voice. I find, upon careful examination, no special evidence of disease in the fauces; there is an entire inability to produce sound of any description with the proper vocal organs; all attempts at speaking are made with the lips—*whispering*. But not being able to divest myself of the idea, that a follicular inflammation of

alysis of the muscles of the larynx, I now determined to try the iodine vapour. I accordingly selected an apparatus, consisting of a metallic vase or urn, with a close fitting cover, flexible tube, and mouth piece (used some years since for breathing medicated vapours in the treatment of consumption). And directed my patient, after filling the vessel half full with hot water, to drop in twenty drops *tinct. iodine*, and inhale the vapour produced by the heated water. Inhalation to be repeated once or thrice daily, according to the irritation or effects, otherwise produced. The first inhalation produced great nausea for a short time, and copious bloody expectoration, but accompanied by an almost immediate, though partial restoration of voice. The dose of iodine was directed to be reduced to 15 drops; and thereafter no unpleasant effects were produced. The voice continued to improve steadily under this treatment, until at the end of a week it had acquired the natural fulness and distinctness of tone.

June 15.—More than a month has elapsed since the restoration of voice; it continues distinct and natural.

On the Arrest of Continued Fever by Cinchonism. By Drs. FLETCHER, GEE and EDDOWES. (*Medical Times and Gazette* and *Lancet*, in *Ranking's Abstract*.)—In an article upon that subject, Mr. Fletcher thus writes :

I had long been convinced of the great good produced by the administration of quinia in the early stages of uncomplicated typhus fever. In the Manchester epidemic of 1847-48, its effects were highly beneficial, and I was led to the conclusion, that in all cases of fever, where there was a tendency to bowel complication, (a very general one in this district,) the early administration of quinia would certainly check, if not cut short, the disease.

Since I became aware of the views of Dr. Dundas, I have followed them out in all uncomplicated cases of typhus fever; and the result of my experience in eighty cases admitted into the Manchester Union Fever Wards, is briefly as follows :—

1st. In the majority of cases, cinchonism established a permanent convalescence within forty-eight hours.

2dly. In the cases where the typhoid character was established prior to the use of the remedy, five-sixths were convalescent within fourteen days.

3dly. In children and patients under puberty, suffering from uncomplicated typhus, nine-tenths were discharged cured within fourteen days.

4thly. In cases complicated with pneumonia, and attended with rose-coloured spots, the latter disappeared under cinchonism; but a more active form of fever often supervened, which required depletion and other active remedies.

5thly. In all cases where the complication was well established—whether pneumonia, ulceration of the bowels, or cerebral congestion—cinchonism produced very little good.

Dr. Gee and Mr. Eddowes give the following statistical results of their experience :

Of 16 uncomplicated cases, 11 were under treatment during the first

seven days; of these, 2 were free from fever on the second day after treatment; 1 on the third day; 2 on the fourth day; 3 on the fifth day; 1 on the sixth day; 1 on the seventh day; 1 on the eleventh day. In 24, the treatment was commenced during the second week: of these, 1 was free from fever on the second day after treatment; 2 on the third day; 5 on the fourth day; 7 on the fifth day; 5 on the sixth day; 2 on the seventh day; 1 on the eighth day; 2 on the ninth day; 1 on the twelfth day. In 26, the length of time that the fever had previously existed was not ascertained: of these, 5 were free from fever on the second day after treatment; 6 on the third day; 3 on the fourth day; 4 on the fifth day; 3 on the seventh day; 1 on the eighth day; 3 on the ninth day; 1 on the twelfth day.

The quinia was also given in 27 complicated cases, the chest being involved in 22, the abdomen in 2, and in 3 both the abdomen and the chest were affected. Of the 22 cases where the chest was involved, (the affection being, as before remarked, generally sub-acute bronchitis,) 15 were treated during the first week: of these, 1 was free from fever on the third day after treatment; 1 on the fourth day; 2 on the fifth day; 4 on the sixth day; 2 on the eighth day; 1 on the ninth day; 2 on the tenth day; 1 on the eleventh day; and 1 on the twelfth day. In 7 the treatment was commenced during the second week: of these, 2 were free from fever on the third day; 2 on the fourth day; 1 on the sixth day; 1 on the seventh day; 1 on the tenth day.

The remedy employed in the above complication, in conjunction with the quinia, was a large mustard jacket placed round the whole of the chest, both back and front, applied twice or thrice daily, and left on as long as the patient could bear it.

Two cases occurred of abdominal complication: of these 1 was admitted on the ninth day of the fever, and recovered on the sixth of treatment; the other was admitted on the fifth day of fever, and recovered on the seventh day of treatment.

Three cases occurred with abdominal and chest affections: of these, 2 were admitted on the fifth day of the disease, and were free from fever on the fifth day of treatment; 1 admitted on the eighth day of fever, was free from it on the tenth.

Thirteen cases in which the quinia was given proved fatal: of these, 5 died within forty-eight hours after admission. Of the remaining 8, one was admitted on the eighth day of the fever, and died on the eighth of treatment. One was admitted on the fifth day of fever, and died on the sixth of treatment; this man had persistent hicough, congestion of lungs, and had had a severe attack of fever seven months before. One was admitted on the seventh day of fever, and died on the tenth of treatment; this patient had been allowed nothing but barley water pre-

and died on the eighth of treatment: a woman æt. 76; this was the only fatal case where the pulse was reduced below 100. One was admitted on the fourteenth day of the fever, and died on the fourth of treatment: the quinia did no good. One was admitted on the eighth day of the fever, and died on the third of treatment; had involuntary motions when admitted.

The effect of the quinia on the pulse is, in favourable cases, sometimes very remarkable; in one case it fell from 104 to 72 in twenty-four hours; in another, from 140 to 84 in seventy-six hours; but generally speaking the reduction is gradual and steady. In cases which terminated fatally, the quinia, with one exception, never reduced the pulse below 100; it sometimes fell from 148 to 124, from 112 to 108, from 140 to 114, but not below 100. None of the fatal cases were under treatment before the fifth day. In complicated cases, the effect on the pulse is less marked than in those which are free from complication.

On a Tympanitic Sound, produced by Percussion of the Lung, when partially condensed. (Edin. Monthly Journal, in Ranking's Abstract.) Two *post-mortem* observations have lately fallen under Dr. Markham's notice, which illustrate, or rather demonstrate the truth of the fact, already noticed by him, *that the percussion sound of a partially condensed lung is clearer than that of a healthy lung.*

This is a fact of considerable importance, for, as Dr. Markham asks in a note at the end of his paper, "is it not a fact of constant observation, and corroborated by auscultation, that in the *early period of phthisis* the percussion sound is clearer in the subclavicular space over the affected lung than elsewhere." This is one illustration.

In the one case, the left lung was found reduced by the pressure of pleuritic effusion to about one-fourth or fifth of its natural size; its lower lobe being *completely* and its upper lobe *partially* consolidated.

In the other case, the *partial* consolidation was general throughout both lungs; it was caused by the effusion within them of the products of inflammation, excited by the rapid and extensive deposition of milary tubercles. Now, when, in these two cases, the lungs, thus differently circumstanced, as regards the nature of the disease affecting them, were removed from the bodies after death, placed side by side, and percussed, it was observed: That the *partially* condensed upper lobe, of the pleuritic case, and every part of the lungs invaded by inflammation in the other—especially the posterior parts, where the consolidation was most advanced, and the lungs contained the least amount of air—yielded a remarkably clear percussion sound, which, in both cases, as far as the ear could judge, was exactly alike in its characters; the sound was that to which, for want of a better, I have affixed the term "hollow," in the paper above referred to; it is clear, high-pitched, empty, of a tympanitic character, and somewhat metallic; the vibrations of sound producing it, appear superficially distributed, ceasing quickly, and not passing deeply. Perhaps it would be most convenient to designate the sound as "tympanitic," for I believe that this word is rarely in practice used to

indicate merely a drum-like sound, as its origin would require; the term "hollow" is objectionable, and for an evident reason.*

The left side of the thorax of the patient attacked by the pleuritic effusion yielded, two days before her death, a completely dull percussion sound at every part; and the heart was found beating to the right of the sternum. To relieve the great difficulty of breathing, induced by this copious and sudden effusion of serum, a very fine trochar was introduced into the pleural cavity, and about twenty ounces of fluid withdrawn therefrom, by the aid of an exhausting syringe. Great care was taken that no air entered into the pleura, and that none did, I am satisfied, having assisted at the operation. Temporary relief was thus afforded the patient; and now, immediately after the operation, on percussion beneath the cavicle, we found, instead of the completely dull percussion sound observed previously, a remarkably loud, clear, tympanitic sound—so marked, indeed, as to lead an observer to suppose that air had found its way into the chest. That there was no necessity for our thus calling in the presence of air, to give reason for the sound, we have the demonstrative proof after the patient's death, when the body was examined. No air escaped from the pleura, but on puncturing the left thorax, a large amount of fluid gushed forth, and when a certain amount had escaped, the partially-condensed lung floated forwards against the upper and anterior walls, and its percussion now, both within, and when removed from the thorax, yielded a character of percussion sound, *exactly similar to that which it had offered during life*, after a portion of the pleuritic fluid had been withdrawn.

I did not observe, during life, the nature of the percussion sound of the thorax in the case of partial consolidation, produced by tubercular inflammation, but it nevertheless well illustrates the fact I am alluding to.

The conclusions which I am justified in drawing from these cases are:

1st. That Skoda's assertion, that a *partially*-condensed lung yields a clearer and more tympanitic percussion sound than a healthily-inflated lung, is correct.

2dly. That Dr. Williams' mode of accounting for this "tracheal" sound, as he calls it, viz: by supposing that the upper part of the lung is compressed against the anterior walls of the thorax by the fluid behind, and thus being consolidated, transmits, when percussed, the hollow sounds of the large tubes, is not correct, at least in all cases, for here was an instance in which every part of the partially consolidated upper lobe yielded equally well the tympanitic percussion sound, and so, also, when divided, to show the absence of all hollowness or large tubes.

*The lungs were taken from two patients under the care of Dr. Chambers and Dr. Sibson, at the St. Mary's Hospital. I would remark, that the fact here men-

3dly. That the sound, so far from of necessity indicating the presence of air in the pleura, is a sign that the lung contains less than its normal amount of air.

One important practical deduction, if I am not mistaken, naturally flows from these facts, viz: that in certain cases of pneumonia, *if not in all*, when the consolidation of the lung has reached a particular stage, but not yet that of *hepatization*, the percussion sound over the affected portion, so far from being duller, is *actually clearer* than natural. The error of diagnosis into which a misinterpretation of this fact may lead the physician, is manifest enough; it may induce him at a critical period of the disease, viz: when the lung is on the eve of complete consolidation, to prognosticate a commencing return to its healthy condition.

On the Association of Iodine Injections with Paracentesis in certain cases. By M. ARAN. (Union Médicale, in Ranking's Abstract.)—In a series of articles published in this journal, M. Aran gives at length the particulars of three cases, of which the following are short abstracts:

CASE 1.—*Chronic pleurisy, with abundant effusion, dating from fifteen to eighteen months, treated successfully by a single operation.*—Br—, æt. 24, by trade a locksmith, was admitted into the Hôpital de la Pitié on the 20th of Oct., 1852. Before his illness, his general health had been pretty good. His illness began eighteen months ago in the form of catarrh and low fever. After eight months of indisposition, he had an attack of severe pain in his side, followed by scarlet fever, and abscess in one of his legs. He had been variously treated without any benefit. On admission into the hospital, his general condition was satisfactory; no fever, no dyspnoea, no pain, but with unmistakeable signs of abundant effusion on the left side—his heart beating under the right nipple, and his liver and spleen protruding below their proper places. The operation was performed on the 21st of October, when 2½ litres of greenish-yellow inodorous pus were evacuated, and an injection of 100 grammes of distilled water, 50 grammes of tincture of iodine, and 4 gr. of iodide of potassium introduced. The patient experienced no inconvenience, and after the syringe was removed, he turned about vigorously in all directions, in order that the injection might come in contact with all parts of the pleura. No fluid followed the removal of the canula. Immediately after the operation, feeble respiration and crepitation were heard all over the left side, where no sound had been audible previously. Two hours later, symptoms of iodism declared themselves, such as dryness of the nostrils, and profuse flow of tears, but these passed off

constant and harassing. The operation was performed in the seventh intercostal space, and 1800 grammes of phlegmonary pus evacuated. The symptoms of iodism were less marked than in the last patient.

26th.—The wound made by the trocar re-opened, and a considerable quantity of pus escaped, with some relief to the patient, but still the inflammatory symptoms continued.

28th.—A free opening was made into the pleura, through which the air entered freely, and 250 or 300 grammes of pus were evacuated. Afterwards 100 to 125 grammes of pure tincture of iodine were injected, and this injection continued night and morning afterwards. There was no improvement, however, and the pulse quickening, and vomiting supervening, the patient sank on the 5th of May. On examination, about an ounce of sero-purulent fluid was found in the affected pleura, free from all sanious odour, and signs of peritonitis with puriform effusion were found in the abdomen.

CASE 3.—*Hydro pneumothorax of more than three months' duration ; operation ; removal of the effusion ; puncture without injection ; operation twice repeated after an interval of three months ; marked amendment.*—The patient in this case, was twenty-six years of age ; six years previously, he had had pleurisy. The existing symptoms of pleuritic effusion had continued for three months, without any relief from the usual treatment. The operation was performed on the 19th of September, and 2½ litres of yellowish serum evacuated ; severe symptoms of iodism followed—frontal cephalalgia, swelling of the eyelids ; injection of the eyes, dryness of the throat, salivation, &c., but these subsided after a little time. Immediately after the operation, the lung was found to have expanded to a considerable extent. The effusion, however, returned, and necessitated two repetitions of the operation during the course of the following three months ; but eventually the symptoms were mastered, and now the patient may be considered as cured.

Dr. Borchman on the treatment of Croup by iced applications to the neck. (Journal für Kinderkrankheiten, in Ranking's Abstract.)—Dr. Borchman proposes to employ this mode of treatment instead of leeches in the early stages of croup, and the proposition seems to be one of considerable value. Three illustrated cases are related. In one of these—that of Dr. Borchman's own son, aged two years—there was some slight diphtheritic exudation on the tonsils, but not in the other two ; still there is little reason to doubt that the three were cases of true croup.

The abstract from which we learn these facts is not very copious, but from it, it appears that in the first case in which this treatment was tried, the little patient had had two violent paroxysms of cough, and that the head was hot, and the deglutition difficult. A linen handkerchief folded as a cravat was dipped in iced water, and wrapped round the neck, a small bladder containing fragments of ice, was applied on each side of the neck, the limbs at the same time being wrapped in

severed in from 3 A. M. to 8 P. M., when the bladders of ice were discontinued. The next day the child seemed to be quite well, but, notwithstanding this, the treatment was persevered in until the end of the day following, the cravat being wetted at more distant intervals.

The result was the same in the other two cases, the relief being equally immediate and effectual.

SURGERY.

On Traumatic Effusions of Serosity. By M. MOREL LAVALLEE. (B. and F. Med. Chirurg. Rev., from Archives Générales.)—The author here draws attention to contusions, in which, the subcutaneous effusion has consisted, not of blood, but of serosity; and as the subject is quite unnoticed by systematic writers (except accidentally by Velpeau, in his 'Closed Cavities'), he gives a brief history of it, derived from the 12 cases he has collected.

The *causes*, in the great majority of cases, have been the oblique pressure of a wheel in motion, brought in contact with the part, and by its rapid motion causing extensive detachment of the skin from the adjacent aponeuroses, without inducing any breach in its surface. The pressure of a wheel of a carriage should always raise the presumption of a production of this lesion. As yet the occurrence has only been met with in men; whether from their being more exposed to external violence, or from the greater quantity of adipose tissue under the skin in women, rendering its detachment less easy. The *nature* of the lesion seems to consist in the rupture of minute or capillary vessels, the bruised extremities of which allow only the thinnest portion of the blood to escape. It resembles the serous discharge that issues from a wound that no longer bleeds, or which is imbibed by the first dressing after amputations. The *effused fluid* is usually abundant, amounting to more than a *litre*, being generally limpid, and of a light citron colour. It may, however, assume a reddish or even a blackish colour; and this has led to its being mistaken for blood prevented from coagulating by the contact of living tissues. It undergoes no coagulation or any perceptible change under the influence of time. Its permanent fluidity quite distinguishes it from blood, as it never coagulates except when mixed with blood, simultaneously effused. The examination of its composition by Lebert and Robin, leads to its being considered as analogous to the serosity of the blood, containing some fatty globules, and some almost colourless blood-corpuscles. On standing, about 1-20th of its quantity is deposited, consisting of colouring-matter in the form of minute granules.

Symptoms.—There is nothing constant in regard to pain. The skin usually preserves its normal colour, though occasionally it is ecchymosed. The collection may assume the form of a tumour, but usually it remains flattened and undefined. The fluid is spread out in a large cavity, which only becomes gradually filled, and generally, never indeed, becoming quite full. The commencement of the collection is thus often very ob-

scure, and its amount is much greater than external appearances would lead one to suppose. Owing to the partial fulness of the sac, a tremulous movement, visible to the eye, is produced on the motion of the parts, and sometimes a strong current of air suffices to induce its undulation. To produce true fluctuation, the skin has to be stretched, or the fluid forced into one part of the sac. In most cases, a more or less distinct ridge surrounds the base of the tumour.

Diagnosis.—It has often been mistaken for an effusion of blood, especially when the fluid has been of a dark or red colour; and when it has been accompanied with pain and fever, it has given rise to the belief of suppuration.

Treatment.—All topical applications have failed; and opening the tumour is the only efficacious procedure. Still, experience thus far shows, that success is proportionate to the delay with which an aperture is made. In general, M. Morel prefers puncturing the tumour by means of a trocar to subcutaneous incision; though, in some cases, the latter may be preferable. Compression is to be afterwards employed to induce adhesion of the walls of the sac, and flying blisters will be found of utility. If the fluid is reproduced, the subcutaneous incision may be resorted to, a little charpie being also employed to keep the aperture open. When simply puncture and well-regulated compression do not succeed, and the lesion assumes a chronic form, we need not hesitate to employ iodine injections. A remarkable example of the success of this practice is given.

Felt Splints. By Dr. FRANK H. HAMILTON. (Buffalo Medical Journal.)—Some years ago, I think in 1845, felt splints were brought to me by an agent of the manufacturer. The felt was sold in sheets, and also in pieces, modeled so as to be readily adapted to the form of the limbs. In some respects it was superior to gutta percha, and I am inclined to think that, on the whole, it was the best splint I ever used. I cannot learn that these splints are now manufactured in any part of the United States, and I will, therefore, inclose you the recipe for making them, which was kindly given me by the agent, and which I have frequently used myself:

Felt Splints.—Dissolve three pounds of gum shellac in two quarts of alcohol. It should be dissolved in a tin vessel, furnished with a tight cover to prevent evaporation. Spread a piece of old or new woollen cloth on a board, and with a clean brush saturate both sides of the cloth with the solution. Hang it up until it is thoroughly dried. Lay it again upon the board and apply a second coat of the solution to one side only of the cloth. Dry again, and apply a third coat to the same side. There will now be three successive layers upon one side and one on the opposite. While the last coat is yet fresh, fold the cloth so that the side having the three coats shall be applied to itself. Now, with a hot

New Operation for Lithotomy. By Mr. LLOYD. (*Medical Times and Gazette*).—Chloroform having been administered, the boy was tied in the ordinary lithotomy position, a curved staff, with a rather deeper and wider groove than usual, was introduced into the bladder, and the stone was again distinctly felt. The staff being taken charge of by Mr. Paget and held in the usual position, Mr. Lloyd next introduced into the rectum a metallic *speculum ani*, which as it differed somewhat from those in general use, we here figure. Its peculiarity consists in its having a constricted neck, by which its retention within the contracted sphincter is rendered easy. This instrument having been introduced with its aperture turned upwards to the front commissure of the gut, its handle was given in charge to an assistant, and Mr. Lloyd next proceeded to the incisions. The angle formed by the junction of the perinæum and rectum was transfixed and divided with a narrow-bladed scalpel, the knife being pushed with its edge directed downwards into the median line of the perinæum, about half-an-inch from the verge of the anus, allowed to enter the front wall of the rectum, at about three-quarters of an inch up the gut; and then, by a single sweep, made to cut its way out downwards into the cavity of the speculum. The next step consisted in dissecting upwards and inwards to the membranous urethra, the operator keeping his left forefinger in the wound, and feeling for the staff. Having found the groove in the latter, the finger was placed, in order to serve as a guide to the knife, on the anterior border of the prostate, at which point the knife was made to enter the groove, and by cutting *forwards*, division of the whole length of the membranous urethra was accomplished. A pair of small straight polypus forceps were next passed along the groove in the staff, into the neck of the bladder, and the dilatation of the prostatic urethra was accomplished by gently opening their blades, and then closing and re-opening them several times. During this process, which occupied perhaps nearly a minute, several gushes of urine escaped. The dilatation having been effected to such an extent that the finger could be very readily passed, Mr. Lloyd withdrew the staff, leaving his finger as a guide, on which a pair of slightly-curved extracting forceps were immediately introduced. The stone was at once found, but apparently, from its occupying the upper and front part of the bladder, it several times eluded the grasp of the forceps, but when once firmly seized, it was extracted with great ease. In order to prevent the roughness of the stone from injuring the important structures in the floor of the prostatic urethra, Mr. Lloyd endeavoured, as much as possible, to hold the blades of the forceps with their convexities directed upwards and downwards, while passing through that part. The calculus proved to be about the size of a small damson plum, being quite smooth on one side, in which its nucleus, probably lithatic, was displayed, but very rough on the other, from irregular phosphatic incrustations. Having examined the bladder with his finger, and ascertained that no other concretions were present, Mr. Lloyd introduced through the wound, into the bladder, a short gum-elastic canula, and attached it by means of a suture to one edge of the incision. The quantity of blood lost had been unusually small, and the operation had occupied but a short space of time.

Removal of a Ring from a Young Lady's Finger. By A. C. CASTLE, M. D.—An interesting young lady, about 17 years of age, had presented to her a gold ring, which she forced over the joints of her middle finger. After a few minutes the finger commenced swelling, and the ring could not be removed. The family physician, Dr. —, was sent for, but could do nothing. The family, and the young lady especially, were now in the greatest consternation. A jeweller was sent for. After many futile attempts to cut the ring with cutting nippers, and to saw it apart with a fine saw, and after bruising and lacerating the flesh, warm fomentations and leeches were applied, but all without affording the slightest benefit. Dr. — requested my presence, with the compliment that “perhaps my mechanical ingenuity might suggest something.” I at once proceeded to the house of the patient, and found the young lady in the most deplorable state of mental agony, the doctor embarrassed, and the family in a high state of excitement. I procured some prepared chalk, and applied it between the ridges of swollen flesh, and all round the finger, and succeeded in drying the oozing and abraded flesh; then, with a soft piece of linen, succeeded in polishing the ring, by drawing it gently round the finger between the swollen parts. I then applied *quicksilver* to the whole surface of the ring. *In less than three minutes* the ring was broken (by pressing it together) in four pieces, to the great relief of all the parties.

In a similar manner, without the chalk, I sometime since extracted a small brass ring from the ear of a child, who, child-like, had inserted it into the cavity of the ear. The operation was more painful and tedious, but was equally successful.

The Modus Operandi.—The quicksilver at once permeates the metals, if clean—with the exception of iron, steel, platina, and one or two others—and amalgamates with them. It immediately crystallizes and renders the metal as hard and as brittle as glass. Hence the ease with which metals amalgamated with quicksilver can be broken.—*Boston Med. and Surg. Journal.*

MATERIA MEDICA.

Uses of Creosote. By M. ARENDT. (B. and F. Medico-Chirurgical Rev.)—M. Arendt (Froriep's Tagesberichte, Nos. 691 and 627) states that the great advantage he had arrived from the use of creosote in asthma and bronchitis, an account of which he published in 1848, induced him to employ it in various other affections.

in non-pregnant women, and in some cases of hæmorrhage prior to delivery, due to *placenta prævia*, it has been found very useful. Indeed, it is a valuable hæmostatic agent, whenever the bleeding proceeds from small vessels, and especially those of mucous membranes. In some of these cases, a more concentrated mixture is required, as ten to twenty drops to the ounce.

New Mode of Inducing Vomiting. (Southern Med. and Surg. Jour.)—The New-Hampshire Journal of Medicine reports a case of poisoning by opium, in which, after the failure of the usual means for inducing vomiting, the patient was made to drink two glasses of vinegar and water, immediately followed by 3 ij. of carb. potassa in water. A powerful effervescence took place, which instantly produced copious vomiting.

On the External Application of Belladonna in Delirium Tremens. By JAMES GRIEVE, M. D., Consulting Physician to the Crichton Institution, Physician to the Dumfries and Galloway Royal Infirmary. (The Medical Chronicle.)—I believe it accords with the experience of every one conversant with the management of this peculiarly interesting malady that, during the second or developed stage, so graphically described by Dr. Blake, the pupil is always more or less contracted. It occurred to me, while reflecting upon this fact, that by dilating the pupil, we might so influence the disturbed visual sense as to dispel, or at least modify, those "false creations proceeding from a heat-oppressed brain" which characterize this disease, and thus conduce to the comfort and tranquility of the distressed patient. I resolved, therefore, to avail myself of the first opportunity to test the reality of this idea.

On the 25th of March last, I was called to attend D. W., æt. 49, a man, naturally, of a robust constitution, but who, of late years, had been much given to intemperance. On inquiry, I found that he had been more or less intoxicated for the last three weeks, that he had slept none for several nights in succession, and that the present was his fourth attack of delirium tremens. I found him suffering under great nervous excitement and commotion; labouring under all sorts of optical delusions, fancying that lizards, centipedes, and other entomological horrors were crawling in and around his bed, from which he was convulsively making vain efforts to dislodge them. His pulse was upwards of one hundred and twenty, soft and compressible; his whole body was bedewed with a cold clammy perspiration, and the pupils of both eyes were much contracted. Having obtained some ext. belladonna, I rubbed a little on the eyelids, and remained by his bedside to mark the result. My expectations were soon more than realized, for no sooner was the physiological effect of the drug manifest in the dilated state of the pupils than the spectral illusions gradually became less and less distinct, the nervous tremor and excitement began to subside, and he a-

gether his condition, mental and corporeal, was much ameliorated. On interrogating him about his recent hallucinations, he replied, "they were all stuff and nonsense; I see no more of them."

From data so limited, it is neither legitimate nor safe to draw any conclusions as to the actions of a remedy, still I cannot help thinking the belladonna has, probably, some specific effect in soothing the cerebral irritation, and inducing a quiescent condition of the nervous system. If further experience confirm the accuracy of the above observations, belladonna may yet prove the best hypnotic, not only in delirium tremens, but also in all those diseases of the cerebro-spinal system in which the primary object is to tranquilize and obtain sleep.

Plaster of Paris in Spasmodic Asthma. By J. P. Root, M. D. (Boston Med. and Surg. Journal).—Not having noticed anything in the medical works of the day, touching the use of the above named article, in the treatment of spasmodic asthma, I am led to believe it is not in general use. In my hands it has proved more effectual than any, or even all other remedial agents, in the treatment of asthma.

Some three years since, I was called to see a severe case of this disease in the person of a young lady of eighteen. After going through with the articles commonly used in such cases, with but little effect, I accidentally hit upon the use of "plaster Paris," in mixture, with almost magic-like results. The only thing previous to this, that gave her much relief, was the smoking of stramonium leaves. Since using the plaster, however, she has been constantly improving; in fact, for the last two years or more, she has nearly forgotten what formerly alarmed not only herself and friends, but troubled her physicians. I have used this article ever since, with similar results.

About two months since, I was called to witness the agony of a little girl of twelve years, lately moved into this vicinity—more to console her friends than to relieve the sufferer, as they had given up the idea of ever seeing her cured or even made better; for, to use their expression, they had "been to all the doctors, and they didn't do her any good." She had not been in a recumbent position for a week. I immediately commenced the use of my favourite remedy, with results as before.

The mixture should be prepared similar to lime-water, and used freely, diluted in water or milk, on each recurrence of the spasm.

I am in the habit of prescribing for my asthmatic patients, cold sponging about the neck and chest every morning, followed by brisk friction. The *modus operandi* of this remedy, I leave for others to enlarge upon.

On the Therapeutic Properties of Yellow Jessamine. By Dr. BATCH-
LOR. (Iowa Med. Journal).—The yellow jessamine (*Bignonia semper-
virens*, of *Linnaeus*; *Gelsemium sempervirens*, of *Jussieu*) possesses

The gelseminum has a slight narcotic odour, and possesses narcotic, nervine, anti-spasmodic and sedative powers. In full doses, it produces a sort of intoxication, languor, dizziness, double-vision, inability to raise the eye-lids; and in an over-dose, complete muscular prostration. As a sedative, I consider it superior to digitalis, sanguinaria, or veratrum viride; it possesses the advantage of being by no means unpleasant to the taste, and of not producing nausea.

A nostrum called the "electric febrifuge" is prepared from this article, distinguished as essence of wintergreen.

I prepare the medicine in the following manner: a bottle is loosely filled with the bark of the fresh root; equal parts of whiskey and water are added, and the bark is macerated for fourteen days. Twenty to sixty drops of this infusion may be used at a dose, alone or combined with quinine.

The "Tincture of Yellow Jessamine," "Gelseminum," as it is generally called, forms the basis of several patent fever remedies, much in vogue in the South-Western States. Its power as a febrifuge was discovered by accident. It is said that a negro was sent to dig for some well known root much used in fever and ague. *By mistake*, he dug the root of yellow jessamine. Its effects thus becoming known, it passed through the usual course of every valuable remedy, being seized upon, prepared and sold as a patent medicine in various forms. One of these is the "Mississippi River Tonic."

The Thompsonians, (or Eclectics, as they now call themselves,) make great use of it. Hence, the great demand for it in Cincinnati, where their principal school is situated.

Many physicians in regular practice, employ it extensively, and speak highly of its effects. As stated above, it may be used alone or with quinine.

Quinia and Tartaric Acid in Intermittent Fevers. By Drs. BARTALLA and BASTILLE. (Medico-Chir. Review, in Ranking's Abstract.)—"Two years since, M. Bartalla brought forward his plan for the more economical treatment of ague, by administering equal parts of sulphate of quinia and tartaric acid. He now (*Bull. de Thérapeutique*, tom. xlv., p. 49) reinforces his former statement by new facts, having treated altogether 208 cases in this way, 196 of these being simple intermittents, and 12 pernicious fevers.

"Dr. Bastille, who has been pursuing the same experiments, states, as his conclusions (*Gaz. des Hôp.*, No. 87.)—1. That sulphate of quinia, given with equal parts of tartaric acid, is more active than the simple sulphate; 2. As a general rule, half the quantity of quinia --

and Robin. He gives the Bichromate in pills of extract of gentian, in the proportion of one gramme ($15\frac{1}{2}$ grains) to the eighty pills. He begins with one pill a day, and then goes on to two, three, four, five and six a day, increasing one every fourth day, and at the end of a month, when the cure is generally complete, he has given two grammes of the bichromate, or 160 pills. He adds a little opium, if there is any nausea or vomiting. It must be stated, however, that an abstemious diet was insisted upon, and that sarsaparilla and guaiacum were given at the same time, so that it is not quite clear, from these experiments, how much credit is due to the bichromate.

OBSTETRICS.

Stricture of the Fundus Uteri as a cause of Dangerous Hæmorrhage. By Dr. DIEZ. (B. and F. Medico-Chirurgical Rev.)—Under this new title, the author alludes to a description of hæmorrhage that most experienced practitioners are familiar with as arising from partial contraction of the uterus. After the separation of the placenta, and while globular contraction of the uterus can be felt above the pubes, alarming hæmorrhage comes on. As there is good contraction of the uterus, and pains, often of considerable severity, occur, this is at first inexplicable. On passing the hand through the widely-opened os, we enter a tolerably spacious cavity filled with coagula, whose walls are firm and unyielding, and remain quite passive to the pressure of the hand. Pushing on farther, we perceive a more or less narrow, often cartilage-like stricture, separating the fundus from the body of the uterus. It is exactly the same as is observed in strong incarceration of the placenta, only, as a general rule, the contracted part is inclined to one side (usually the right,) and takes on a more oval form.

The means calculated to check hæmorrhage under other circumstances, are here useless or mischievous. Increasing the pains by means of ergot, cold, external pressure, friction, &c., does injury by increasing the abnormal contraction, and by favouring the flow of blood from the non-contracted part. Of a more useful character are such means as tend to arrest hæmorrhage without exciting pain, as compression of the aorta (taking care, however, not to compress the fundus uteri,) acids, alums, cinnamon, ipecacuanha, warm astringent injections, &c. The most efficacious means, however, is the attacking the pathological condition upon which the hæmorrhage depends by the employment of belladonna, in

come under my notice in hospital and private practice, instances are not unfrequently met with where no satisfactory explanation can be given for the increased catamenial flow. Of course, amongst these, patients suffering from general plethora, from hæmorrhage caused by polypi either within or without the uterus, or from that dependent upon fibrous tumours, uterine hydatids, ulceration (simple or malignant), or upon a watery condition of the blood, are not included. In the instances alluded to, I have almost always been unable, after careful examination, to say upon what the menorrhagia depended, since no physical alteration could be detected in either the uterus or ovaries; it has only therefore been practicable to assert what were not the causes.

The symptoms usually presented, are briefly these: the catamenia appear regularly every twenty-eight days, and are at first only of the proper quantity; but, instead of ceasing after a duration of three or four days, they continue unabated for ten or fourteen, and occasionally even for three weeks. The general symptoms which arise from this debilitating discharge are just such as might be expected. There is general weakness, languor, mental depression, with pains in the head, loins, and so on; the patient suffering, it is to be remembered, not from any diseased condition, giving rise to the hæmorrhage, but merely from the loss of blood itself. In other instances, the discharge continues a less time, but the flow is more abundant, clots being frequently discharged; this variety is generally followed by leucorrhœa. My own experience tends to show that these forms of menorrhagia occur more frequently in unmarried than in married women; but, for many reasons, and especially because of the class of patients from whom my observations are for the most part deduced, I would not say positively that such is the case.

Ailments of this class are at all times troublesome to cure, but those I am considering are particularly so, from the absence of any special indications for treatment. In some of them, it appears at first sight, only necessary to keep the patient quiet, to administer astringents, especially such as act particularly upon the uterus, and to regulate the diet, in order to give the desired relief. But it will often be found that these means are quite inefficient, and the acetate of lead, gallic acid, the ergot of rye, oxide of silver, sulphuric acid, tincture of sesquichloride of iron, and similar remedies, may be employed without any avail. In thinking over these cases, I was led, from a remark made by Dr. Pereira, to try the use of cinnamon. In the last edition of the "*Elements of Materia Medica*," this gentleman says, when speaking of cinnamon, that "some writers regard it as acting specifically on the uterus," and reference is then made to the writings of Sundelin and Wibmer. As far as I know, these are the principal authors who have recommended the use of this agent; for I am not aware that mention is made of it by gentlemen who have written on obstetrics and the diseases of woman in this coun-

may possess from the tannic acid which the bark and leaves of it contain in common with all the lauraceæ order of plants, is, I think, certain; and partly in confirmation of this view, it may be mentioned, that in a case of labour in which I employed it, it *appeared* not only in a marked manner to increase the severity and rapidity of the pains, but the patient, who had in her previous labours suffered severely from flooding after the birth of the placenta, on this occasion lost only a very small quantity of blood. That this fortunate circumstance was due to the administration of the cinnamon I do not of course pretend positively to assert; the case is merely mentioned to give some colour to the opinion expressed that this agent acts specifically upon the uterus. Much clearer evidence of its value in such cases of menorrhagia as I have described, is, however, easily obtained. The principal points I would now refer to are, that it acts after the failure of other astringents; that it is most efficacious given alone, uncombined with other medicines; that if its employment be discontinued too soon, the discharge of blood returns; while in a few instances, it has been found necessary, after an apparent cure, to resort to its employment for the two succeeding catamenial periods, when the menstrual flow, after continuing for three or four days, has not given any signs of abatement, and when the patient has begun to suffer from mental depression and the early symptoms of general debility. The form usually employed, and which appears to be the best, is that of the tincture, in drachm doses, using cinnamon water as the vehicle; it should be taken about every six hours, but not more frequently, as it is apt to give rise to nausea and vomiting. It is also better to continue its administration for about fourteen days after the symptoms which called for its employment have disappeared; and even then, if the case has been an obstinate one, a draught composed of it should be taken once daily for a month.

In concluding these brief observations, I trust I shall not be understood as recommending cinnamon as an infallible remedy in all examples of menorrhagia. All I would wish to assert is, that in a certain class of cases, I have found it to possess properties of great value; and though I might have waited until I had been enabled to test its properties on a larger scale, yet I preferred giving the results of my experience at once, in order that your readers might have the opportunity of employing this agent, and ascertaining its value for themselves. If they will do so and record the result, we shall soon possess satisfactory data for determining the action of cinnamon upon the uterus.

above tables, in the cases prolonged to the fourth week, or beyond it, is the fact, that though impregnation usually takes place within a few days after the last catamenial discharge, it may not necessarily do so. For conception may, in fact, occur at any date previous to the recurrence of the next menstrual period. This gives an uncertain limit for the actual date of impregnation of twenty-two or twenty-three days—the average interval between two menstrual periods. But the possibility of variation from this circumstance will by no means account for the great amount of variation in gestation which is so frequently observed. For there has now been recorded a long comparative series of observations upon the duration of pregnancy among our domestic animals, and particularly upon the cow, in which these sources of variation and fallacy in regard to the length of gestation, were avoided by the fact that the exact day and date of impregnation in these experiments, from a single intercourse with the male, were, in each instance, accurately noted and fixed. Thus Lord Spencer has reported the date of the delivery in 754 cows, in which the day of impregnation, from a single coitus with the bull, had been carefully registered. M. Tessier had previously published 572 observations of a similar kind upon the same animal. If we omit a number of cases in each of these two series of observations, in which delivery supervened earlier than the thirty-seventh week, we have the result of Lord Spencer's and M. Tessier's observations presented in the following table:

TABLE I.
Periods of Gestation in Cows.

Weeks.	Days.	Spencer.	Tessier.
37th	From 252d to 259th	12	6
38th	" 260th to 266th	4	8
39th	" 267th to 273d	21	51
40th	" 274th to 280th	124	166
41st	" 281st to 287th	392	202
42d	" 288th to 294th	175	105
43d	" 295th to 301st	16	27
44th and upwards	} " 302d to 321st	7	7
		751	572

"This table shows that, in the cow, even when the day of impregna-

form, as we have already done in Table II, in relation to the cases in the human subject, recorded by Dr. Merriman, Murphy and Reid. The following table shows the actual proportion of weekly deliveries in Lord Spencer's and M. Tessier's observations on the cow, as calculated from the known date of impregnation of the animal; or, in other words, the proportion of cases in every 100 in which the gestation was normal, abridged, or prolonged:

TABLE II.
Percentage of Different Periods of Gestation in Cows.

Weeks.	Days.	Spencer.	Tessier.
37th	From 252d to 359th	1.60	1.05
38th	" 260th to 266th	0.55	1.40
39th	" 267th to 273d	2.80	8.91
40th	" 274th to 280th	16.53	29.02
41st	" 281st to 287th	52.27	35.31
42d	" 288th to 294th	23.18	18.36
43d	" 295th to 301st	2.12	4.72
44th and upwards	" 302d to 321st	0.93	1.22

In making any comparison between the results of the period of gestation in the cow, as shown in table II, and the period of gestation in the human female, it is necessary to remember, that, while the normal period of gestation in the human female is probably from 274 to 280 days, that of the cow extends, according to Lord Spencer, to 284 or 285 days. And hence, while we find that the largest percentage of deliveries of the cow, takes place from the 281st to the 287th day of gestation, the largest percentage of deliveries in the human female, as calculated even from the last day of the catamenial discharge, occurred in Dr. Merriman's and in Dr. Reid's observations, from the 274th to the 280th day; though in Dr. Murphy's series of cases, his largest weekly percentage occurred in the week following the 280th day from the last catamenia—an occurrence probably to be explained by the circumstance of impregnation not following till two, three, or more days after the last menstruation.

Amongst the various observations which might be elicited by a comparison of these tables, let me state one more: that though the period of gestation in the cow is found liable to considerable variations, and is by no means so precise and determined as many authorities have sup-

the last day of the catamenia does not by any means, in the human mother, fix determinately the actual day of conception.

"Occasionally, however, in the human mother, pregnancy occurs under circumstances which fix the date of conception to a precise day. Dr. Reid, for example, has collected from the observations of various authors, and from his own practice, the history of forty cases, in the human female, where impregnation was the result of a single intercourse, the date of which was accurately known; the cases being all of them 'instances of single women who date from one coitus, or of married females whose husbands had been absent for a considerable time before the last intercourse,' and all of them being selected with a view to avoid as far as possible every source of deception. Below I have thrown these forty cases of gestation into a tabular form. This table demonstrates the varying duration of pregnancy, and consequently the variable date of parturition and delivery in the human female, even when the date of impregnation is previously accurately established and known. The last column in the table presents in the percentage form (as far as can be calculated from so small a number,) the relative proportion delivered at different dates from the day on which conception occurred.

TABLE III.

Periods of Forty Deliveries in Human Mothers—Impregnation being calculated from a Single Intercourse.

Weeks.	Days.	Total Number of Cases.	Percentage.
38th	From 260th to 266th	5	12.50
39th	" 267th to 278d	7	17.50
40th	" 274th to 280th	18	45.00
41st	" 281st to 287th	6	15.00
42d	" 288th to 294th	4	10.00

"This table shows that in the human subject, and with the day of impregnation known, pregnancy in nearly one-half, or in 45 per cent, extended from 274 to 280 days; that in 15 per cent. it was protracted from the 281st to the 287th day; and in 10 per cent. it was prolonged from the 288th to the 294th day. Three only, out of these forty cases, or 1 in 13, were delivered on the 280th day of conception. Seven, or 1 in 6, were delivered on the 275th day.

"Admitting, then, the possibility of the occasional protraction of human gestation beyond its common normal period, another question has sometimes been asked, viz: what is the extreme limit of this protraction, or, in other words, to how many days beyond the normal period of gestation is it *possible* for a woman to carry a child? I confess that some of the late cases recorded, particularly by our American brethren, appear to me to be beyond the bounds of this possibility. Thus, Dr. Meigs of Philadelphia, has reported a case on 'facts' which he conceives to be 'trustworthy,' where a patient supposed herself pregnant in

July, 1839, quickened on the 20th of November, had spurious labour pains on the 10th of April, but her child was not born till 'the 13th of September, 1840, about daylight;' the pregnancy thus, according to Dr. Meigs, having 'endured nearly fourteen months, on 420 days.'

"Professor Atlee, of Philadelphia, has published two cases of protracted gestation occurring under his own observation. In the first of these, the patient lost her catamenia on March 22d, 1832—quickened August 5th of the same year—and was delivered with forceps March 22d, 1833. It was her fourth pregnancy. In Dr. Atlee's second case, the patient saw appearance of menstruation for the last time, on August 6th, 1832—quickened on December 25th—but was not delivered till August 13th, 1833. Dr. Atlee, who states that he has not "the least doubt of the truthfulness of the evidence in the above cases," imagines, from the date of the quickening in each, that both mothers became pregnant shortly after their last catamenial periods, and that consequently, in both instances, the patient went pregnant nearly the entire circle of the calendar, or 365 days.

"Without stopping to inquire as to the various sources of fallacy in these and similar observations, let me merely add, that I believe, our best criterion for fixing the "legal limit," or ultimate possible period of gestation in the human female, will be derived from careful and repeated observations upon the ultimate period of gestation in the cow; allowing always of the difference of four or five days of excess in the normal period of pregnancy in the cow, as compared with the human mother. Lord Spencer, in one instance, found the period of gestation in the cow to extend to 313 days from the date of impregnation; and in one of his observations on the same animal, M. Tessier states, that parturition did not supervene till 321 days from the date of conception.

"Such direct experiments and observations upon the lower animals afford evidence, which necessarily, I think, forces us to admit, that in exceptional cases in the cow—and hence, also, as we certainly may fairly infer, in the human female—the period of gestation may be prolonged 30 or 35 days beyond its normal and usual duration. And it is not impossible that further accurate and repeated experiments of the nature of those performed by Lord Spencer and M. Tessier, may yet establish, by the same kind of proof, even a more extended limit as the *ultimum tempus pariendi*.

"Those obstetricians who maintain that the period of human gestation is a fixed period, and can never, by any possibility, exceed the fortieth week (as was sworn to by the five London accoucheurs who gave evidence in the Gardner peerage cause, and by the six medical practitioners who more lately gave similar evidence in an analogous cause before the American Court at Lancaster,) have none of them adduced any reason why the period of pregnancy should be thus stable and invariable.

animal kingdom to suppose that such variations did not occur in regard to the function of gestation."

2. Dr. Reid is also of opinion, that the period of pregnancy is not altogether determinate. According to him—

The deviation is not to any great extent; the only *certain* data for calculating, are those depending on the *known time of conception*.

The *average* duration of the pregnant state when calculated from this event, is about 275 days, or it may have a range from the 270th to the 280th day.

There is no satisfactory evidence of gestation having been prolonged beyond 293 days.

The code of Napoleon, which allows 300 days, and the Prussian law, which fixes the extreme limit at 301 days, may be regarded as liberal.

The *catamenial period* must generally serve as our guide in default of some exact knowledge. It is, however, often fallacious, and is only a means of approximation as to the probable time of parturition. The 40th week after the last appearance is the most likely period, and the 41st the next.

HYGIENE.

Shaving the Beard. (Boston Med. & Surg. Journal.)—The more I reflect upon the mysteries of neurology and animal chemistry, the more confident I am that, while we are the least suspecting it, trifling errors in our daily life are producing important effects upon our corporeal systems; and I declare it, as my deliberate conviction, that the habit, which may almost be styled American, of using the razor upon the face, is sufficient to cause a large proportion of the lamentable evils which affect the human race in this country.

It appears, by experiment, that the beard, if shaved, grows from four to five times faster than if unshorn. In this calculation, an item is omitted, which it is difficult to estimate, i. e., the stimulus given the beard by the first applications of the razor in adolescence—the experiments being made upon beards after they have acquired an unnaturally rapid growth. The effect of this early stimulus may be fairly counted at double the natural growth; then reckoning the difference in size and weight of the fibre, which is treble, and we find the frightful truth to be that we raise thirty times the natural quantity of beard! Thus it is evident that the true beard is exhausted at a very early age, after which the system is forced to supply a substitute. Now nature will not submit with impunity to extraordinary demands upon her vigour, and that which requires her to produce in a life, thirty times as much beard as

revenge she will take for being compelled to labour without rest under an uncompromising task-master !

2d. The chemical laboratory of man furnishes, in just proportion, the ingredients required to deposit in suitable quantity the bones, skin, hair, nails, etc., and it is obvious that a superstraining of those chemical elements which enter into the composition of the beard must deprive of their just due all the other tissues which are wholly or in part composed of the same elements. Such injustice to other structures they must inevitably feel, and the entire system must suffer from a disturbance of the balance of power which was requisite to a healthy action of its various parts.

3d. The proper calorification of the body is one of the most essential conditions of its healthy action ; and the non-conducting properties of the beard ought to be a caution against trifling with so powerful an agent, more especially when one considers its intimate connection with the calorific organs of the brain, and with the respiratory organs. The popular notion that, as women are beardless, men may be or not, as they please, is founded in misapprehension. A man and a woman form one specimen of the *genus homo*, and, from a physiological point of view, must be considered one and the same. The absence of beard in woman is countervailed by some other differences in her constitution, which it would be needless to point out, even if we knew them. It suffices to know that nature is perfect in her work.

4th. The errors of the father shall be visited upon the children unto the third and fourth generation—the tree being known by its fruit, for a corrupt tree cannot bring forth good fruit ; which, simplified, is, “like begets like.” No person who feels the force of this law in all its fulness, can expect to transmit to his posterity vigorous pulmonary organs, if he has done the best he could to ruin his own. Daughters and sons are by nature equally their father's heirs, and if consumption of the respiratory organs spares more men than women, the out-door exercise of men must, in part, account for the difference.

The mania which has ever possessed man for disfiguring himself is astonishing. Not satisfied with God's most perfect handiwork, different tribes and nations variously undertake to beautify it, thus fairly making themselves laughing stocks for each other ; but it is to be hoped that the “pioneers of civilization” will come out from the category of those who tattoo the skin, flatten the skull, shave the crown, taper the waist, stint the feet, circumcise, and slit their ears and noses.

It is with difficulty that old habits are renounced, even when one is convinced that life can be prolonged and made happier thereby ; but it is a question for young men seriously to consider, whether, on starting in life, they will addict themselves to a habit which at once wastes the time, sours the temper, is against nature, and, consequently, involves their health and that of their offspring.

MISCELLANEOUS.

On Smallpox and Vaccination. By Mr. F. MARSON. (Lancet, in Banking's Abstract.)—In a recent communication to the Medical and Chirurgical Society, the author stated, that during the period comprised within this analysis, smallpox had been epidemic four times—in 1838, 1844, 1848, 1851; and rather more than half the patients admitted into the hospital, had been previously vaccinated. Much anxiety about the degree of security afforded by vaccination had begun to be felt, and the author thought the profession would be interested, and much useful information elicited, by classifying and arranging the patients admitted during the above-named period. The analysts referred principally to the following points:

- i. Natural smallpox.
- ii. Smallpox after smallpox.
 - a. After natural smallpox.
 - b. After inoculation.
- iii. Smallpox after vaccination.
 - a. Number of cicatrices.
 - b. Character of cicatrices.
 - c. Vaccinated, but without cicatrices.
- iv. Febrile eruptive diseases mistaken for smallpox.

A remarkable difference was observed between the vaccinated and unvaccinated patients, and also, between the vaccinated cases themselves—some patients having the smallpox in a mild form, wholly devoid of danger, whilst others had it in great severity, scarcely, if at all, lessened by the previous vaccination. Under these circumstances, the author thought that the causes of this remarkable difference might be sought for among the antecedents, in respect to the vaccination of each individual admitted, with a view to explain the extreme mildness of some cases—the danger, unmitigated course, and even death of others. Smallpox in the unprotected, remains to this day, as virulent as it ever was. Vaccination, when performed in infancy, affords almost complete security against the fatality of smallpox, up to the period of puberty; and the general experience of the Smallpox Hospital shows that smallpox did not usually occur after vaccination, until several years had elapsed. The most trustworthy evidence of the perfection of vaccination, was to be obtained from the cicatrices, and this evidence, he would be able to

in each case, and the result, with the rate per cent. of mortality under each division. He there gave the ages of the unprotected patients, and the rate per cent. of mortality calculated at different periods of life, for every five years up to thirty, and even ten years afterwards. The fourth table exhibited, separately, the leading particulars of 3094 cases of smallpox after vaccination, showing, from a careful examination of each patient, the number and character of the vaccine cicatrices, the form of the variolous disease, and the result, with the rate per cent. of mortality from smallpox, after deducting the cases of superadded disease. It also showed the rate of mortality from smallpox in patients having one vaccine cicatrix, particularizing whether good or indifferent, and the averages. The fifth table showed, in quinquennial periods, the ages of the vaccinated patients, when attacked by smallpox, when they were vaccinated, and the rate of mortality. The sixth table stated, in periods of five years, the ages of the patients at the time they were vaccinated, who had subsequently been admitted with smallpox into the smallpox hospital. Observations on the results, accompanied each table; and in relation to table IV, it appeared that 3094 patients with smallpox, reported themselves to have been vaccinated at some period of their lives. 1357 had one vaccine cicatrix; and of these, four and a quarter per cent. died with a good cicatrix, and twelve per cent. with an indifferent cicatrix; mean, seven and a half per cent. 888 had two cicatrices; two and a half per cent. died with good cicatrices, seven and a quarter with indifferent cicatrices: mean mortality, four per cent. and a fraction. 274 patients had three cicatrices: average mortality, one and three-quarters. 268 patients had four cicatrices; and three died with good cicatrices under one per cent.: with indifferent cicatrices, none, the average being only three-fourths of one per cent. The author described a good vaccine cicatrix as distinct, foveated, dotted or indented, in some instances radiated, and having a well, or tolerably well defined edge. An indifferent cicatrix is indistinct, smooth, without indentation, with an irregular and ill defined edge. The author's opportunities of examining, with regard to previous vaccination, the foreigners admitted with smallpox at the hospital, and comparing them with each other, and with the same class of persons in this country, had led him to the conclusion, that vaccination was performed in the best manner, generally, by the Danes, Swedes, Norwegians, and Germans, judging them by the standard shown in table IV, to afford the most efficient security. Then came the Italians; and from the few he had seen, the Spaniards; then the Scotch; then the Irish; and lastly, the English and French. He

the well vaccinated, was not the only evil result to be regretted of bad vaccination. Proportionate to the mortality, was the severity of the disease, and to those who escaped death, there was damaged health, disfigurement for life perhaps, and the discredit brought on vaccination which was in no degree due to it intrinsically, but was owing solely to the want of proper knowledge of the subject, and of the necessary care with which the operation should be performed. Great judgment and caution should be exercised in the selection of vaccine lymph, for in this lay one of the principal causes of failure in vaccination. Lymph for use was in its best state on the seventh day of the progress of the vesicle, the day week from the vaccination; and the author described the character of the vesicle, and the indications of the stage when it was most favourable for the collection of the lymph, and some very valuable and instructive observations followed on the mode of conveying the lymph, the mode of vaccinating, the mode of preserving lymph, and on re-vaccination.

The author's conclusions, from the foregoing facts and statements, were—

1st. That natural smallpox destroyed about one-third of all whom it attacked.

2d. That smallpox after smallpox was of comparatively rare occurrence; that a second attack of natural smallpox was rare, but not often fatal, and that protection seemed to be the law. That after inoculated smallpox, an attack of smallpox had more frequently led to fatal results; but there is reason to presume that the virus used for inoculation, like a great deal of the lymph used at the present day for vaccination, was often taken at too advanced a period of the disease, and thus did not afford the full measure of protection it was capable of affording, if taken at a proper time.

3d. That vaccination performed in infancy afforded almost complete protection against the fatality of smallpox, to the period of puberty; that a variety of circumstances conspired to make it almost impossible to ascertain exactly, in what proportion to the vaccinated cases of smallpox subsequently occurred, or might occur, if all persons lived to an advanced age.

4th. That, as a matter of safety, it would be well for all persons who were vaccinated in infancy, to be vaccinated at puberty; this measure being more especially requisite for those who were either indifferently, or doubtfully vaccinated in infancy, and still more necessary for those who, though vaccinated, had no cicatrix remaining. Finally, as a matter of precaution, it would be desirable that all persons should be re-vaccinated on smallpox existing in the house where they were residing—a precaution, however, that will cease to be necessary to advise, when all persons have the benefit of proper and efficient vaccination.

1. That three cases re-vaccinated within seven years were not again susceptible to vaccine.

2. That vaccine, after the interval of from eight to seventeen years, does not prevent the reception of vaccine again, except in 22-174 per cent.

3. That the distinctness or imperfection of the vaccine cicatrix does not materially alter these results.

4. That variola does not prevent the formation of the vaccine vesicle, except in about 23.53 per cent.

5. That the occurrence of smallpox subsequently to vaccination does not destroy the susceptibility of the human system to again receive the vaccine poison except in about 10 per cent.

6. That in all the previous cases, whenever the secondary vaccine vesicle assumed its perfect form, its subsequent history was the same as if the system had not previously laboured under vaccine, variola or varioid.

7. It is probable that the protective influence of vaccination has diminished in consequence of repeated transmission of the vaccine matter through the human body.

8. It is desirable that re-vaccination should be extensively followed, as one means of giving additional protection to the masses.

9. That when possible, the stock of vaccine should be renewed by going back to the original source.

On the Behavior of Alcohol in the Animal Organism. By Dr. DUCHECK. (Prag. Vjhrschr., translated for Medical Examiner.)—There are now six kinds of alcohol well known, the æthyl, methyl, amyl, and acryle alcohols, the cetyle and the glyceryle. To each of these alcohols, possessing the general formula ($C_nH_x + O + HO$), there is a corresponding aldehyde ($C_nH_x - H_2 + O + HO$) which differs from the alcohol by possessing two equiv. less of hydrogen; and also a group of characteristic acids having the formula ($C_nH_x - H_2 + O_2 + HO$). Each of these acids contains two equiv. more of oxygen than its corresponding aldehyde. To the æthyl-alcohol belong the acetal-aldehyde and the acetic acid; to the methyl-alcohol, the formic aldehyde and the formic acid; to the amyl-alcohol, the amyl aldehyde and the valerianic acid, &c., &c.

In order to change the alcohol into aldehyde, two equivalents of oxygen are absorbed, forming two equivalents of water, then the aldehyde again absorbs two equivalents more, forming water and acetic acid.

Alcohol $C_4H_9O_2 + O_2 = 2HO + C_4H_7O_2$ aldehyde.

Aldehyde $C_4H_7O_2 + O_2 = 2HO + C_4H_5O_2$ acetic acid.

The process of oxidation, however, does not end here:

Acetic acid $C_4H_5O_2 + O = HO + 2C_2HO_2$ formic acid.

Formic acid $C_2HO_2 + O = HO + C_2O_3$ oxalic acid.

Oxalic acid $C_2O_3 - O = 2C O_2$, carbonic acid.

It is apparent from this, that the process of the transformations of alcohol takes place by the continued absorption of oxygen, whereby, finally, water and carbonic acid are formed. The process of oxidation in the other kinds of alcohol is the same as in the æthyl alcohol.

The author, by means of a stomach pump, introduced into the stomachs of three dogs, large quantities of absolute alcohol (from half 3 to 60 grammes,) partly pure and partly diluted; he also injected into the rectum of a fourth dog $1\frac{1}{2}$ 3 of fusel oil diluted with water. The phenomena exhibited in all the four cases were, 1. Intoxication more or less strong, and death sooner or later, according to the quantity of alcohol introduced, and to the slow or rapid manner of administration. 2. These phenomena occurred in the same manner, whether the alcohol had been introduced into the stomach or the rectum. 3. The blood appeared somewhat darker, was of an alkaline reaction, and was distinguished by an odour like aldehyde, that was diffused through all the organs. 4. In two cases aldehyde was detected in the blood; while, 5th, no further product of oxidation of the same was found. 6. Alcohol was not once detected in the blood. 7. The stomach, even a short time after the introduction of alcohol, contained only a very small amount of it. 8. The urine, as well as the fluid in the cerebral ventricles, exhibited a peculiar but evanescent odour of ether. 9. No important anatomical change existed in any of the organs. 10. The action of amyl-alcohol appeared far more decided than that of the æthyl-alcohol. From these facts it follows: that the alcohol becomes absorbed and chemically changed, and that these two actions occur so rapidly, that, even after a very short time, we can only detect the aldehyde. The impossibility of detecting alcohol in the blood is opposed to the prevailing view, that alcohol, in an unchanged atomic combination, remains for a short time in the blood. This view rests upon the observation that, after the use of a large quantity of spirituous liquors, the breath is charged with a kind of alcoholic odour, or, rather, that peculiar to the drink that has been taken; and, also, that in opening the bodies of those who have suddenly died, an odour of alcohol is diffused from the inner organs. These cases, the author considers, are all founded upon incorrect observation. In dissection the odour of alcohol is confounded with that of aldehyde, and as for the breath, it carries along with it, besides the peculiar pulmonary exhalations, that portion of the liquor that remains in the form of vapour in the cavities of the mouth and throat. Other substances, besides aldehyde, are contained in the breath, and exhaled undecomposed at the same time, such as the peculiar aromas of the various liquors, which can be distinguished from the pulmonary exhalations. They originate from the various substances associated with the alcohol, viz., the different kinds of ether; the œnanthic ether, (from wine and cognac,) the butyric ether, from rum, and, also, ethereal oils, various acids, &c. The odours of these substances are generally characteristic of the specific drink, and they are identified with that of alcohol, and it is then assumed that this is exhaled, and therefore remains undecomposed in the blood. If it be granted that alcohol is oxydized in the system into aldehyde, then it must be admitted that this oxidation takes place either in the stomach or in the blood. Oxygen is, in every case, necessary to the transformation of the alcohol; in this respect, therefore, the formation of aldehyde meets with no impediment in these places. The change, however, cannot take place in the stomach, since, first, we can never detect the presence of aldehyde in it: and, second, since the conditions re-

quisite to the formation of aldehyde in the stomach are the same as in atmospheric air, the change of the alcohol in the stomach taking place slowly, as when exposing a large amount of it to the atmosphere, so that intoxication would occur much later. This view will finally appear untenable, if we but consider that intoxication, consequently the formation of aldehyde, occurs equally if the alcohol be introduced into the rectum, whose cavity, as is well known, contains no oxygen. If, now, the alcohol remains unchanged in the stomach, its transformation must necessarily take place in the blood. The question arises, how does the alcohol enter the blood? Many experiments which the author made of the penetrability of the membranes, (hog's bladder,) to alcohol and aldehyde, place it beyond doubt that the walls of the stomach can be penetrated by the alcohol, and thus be taken up by the vessels. Another question now arises: does the transformation of the alcohol take place after it has been carried unchanged to a remote part of the body, or immediately after its absorption by the blood vessels? Several facts are opposed to the first supposition. Whenever alcohol, in great amount, comes in contact with blood, coagulation necessarily occurs. This is proved by the daily experience and unsuccessful attempts of earlier experimenters, who have studied its action whilst injecting alcohol in the veins. Such coagulation will then occur if, as in the first three experiments of the author, a great quantity of alcohol penetrates through the walls of the stomach without oxydizing, that is to say, without becoming changed into aldehyde. And yet, in the bodies of men or animals, that have suddenly died from intoxication, there has never been found any trace of coagulation, that had taken place during life. Experiments show that only a very large amount of aldehyde, introduced rapidly and in an undiluted condition into the veins, can produce coagulation; that this, therefore, never takes place when introduced slowly, in small quantities, and tolerably diluted. From this, therefore, we conclude, that alcohol, only after it has been changed into aldehyde, can remain any length of time in the blood. Besides, the rapid penetration of aldehyde into all the tissues, even in one or two minutes, can be more readily explained by its greater capacity of imbibition, than by assuming the same of alcohol, and supposing the change into aldehyde to take place only after the imbibition of the former into the organs.

These reasons, and the impossibility of chemically detecting alcohol in the blood, make it probable that the alcohol enters only the absorbent vessels, and that, on the appearance of even the smallest amount in the interior of these, oxidation immediately follows. This view of the rapid formation of aldehyde, is supported by the large surface and fine porous condition of the walls of the stomach. Since, then, it is assumed that, intoxication is not produced by the action of alcohol upon the stomach, it must, therefore, commence with the formation of aldehyde, and this, consequently, must be the intoxicating principle. The author, to prove

vessel, produced mechanical coagulation of the blood. 3. After the passing off of the narcotic effect, acetic and oxalic acids were found in the blood; from this it appears, that aldehyde is fitted for excretion by the absorption of oxygen. 4. If there is at any one moment, a large amount of aldehyde in the blood, a portion of it can be thrown off, unchanged, with the exhalations from the lungs. Hence arises the odour of the breath, that has been confounded with that of alcohol. This symptom only appeared when a great amount of aldehyde had been rapidly injected. The lengthened respiration indicated the necessity for oxygen; the animal heat appeared to keep parallel with the increased process of combustion.

If it now be granted that intoxication commences with the formation of aldehyde, it may be asked whether it ends with its disappearance from the blood, or, whether the symptoms of intoxication are connected with the presence of the further process of oxidation, acetic acid, &c. The author's experiments show that acetic acid is never present in the blood of animals that have died during intoxication, although it has been found after this has passed off. Besides, the author made the direct experiment of injecting diluted acetic acid into the veins of a dog—no narcotic effects followed. These facts lead to the conclusion that the period of intoxication is limited to the disappearance of the aldehyde, that is to say, to its conversion into acetic acid. Consequently, the above stated view, that intoxication is due, properly, only to the aldehyde, receives further confirmation. In two experiments, oxalic acid was found in the blood, as a further product of the oxidation of alcohol. The ultimate products of this continued oxidation must, of course, be carbonic acid and water. For the determination of carbonic acid and water, in the pulmonary exhalations, after the use of alcohol, the author made three experiments upon dogs, in which, at the same time, the amount of expired air, the animal heat, the number of respirations, and arterial pulsations, were taken into account. He obtained the following results: 1. After the use of alcohol more air is inhaled, indicating an increased necessity for oxygen. 2. In the expired air is found, (with equal amount and equal number of inspirations), *less carbonic acid and less water* in the pulmonary exhalations. (A result which Vierordt and Böcker also obtained.) 3. The number of respirations and arterial pulsations, together with the animal heat, consequently the process of oxidation, were greatly augmented.

When the process of alcoholic combustion, by the continued absorption of oxygen, takes place, the blood is in this way deprived of a great amount of oxygen, which would otherwise be applied to the combustion of other substances. The blood, therefore, must undergo a change, and the normal course of metamorphoses be impeded; and the quantity of

or less amount remain unconsumed, at least for a time. That this actually takes place, the author proves, by his investigations of cases of sudden death, by alcohol and aldehyde, as well as by a long continued use of alcohol, in which cases, he always found a large quantity of sugar in the blood, and, indeed, larger than in the blood drawn from two dogs in the course of the experiment, *before* the first intoxication. If alcohol ($C_4 H_6 O_2$) is burnt, the proportion of carbonic acid to water is as 4 : 6 or 1 : $1\frac{1}{2}$; in the combustion of sugar ($C_{12} H_{12} O_2$) as 1 : 1. In the first case, therefore, there is more water and less carbonic acid formed. If, now, whilst using alcohol, it alone is consumed, the sugar remaining unchanged in the blood, then the proportion of carbonic acid to water, that is expired, is changed, and this is the reason why, *after the use of alcohol*, less carbonic acid is expired. Concerning the second product of combustion—water—in spite of its outweighing the carbonic acid, its amounts are very inconsiderable. Since, now, the kidneys are, especially, designed for the secretion of water, then it is evident why its amount is greatly decreased in the exhalations from the lungs. That a great part of the water passes off into the urine, is shown by the great increase of the latter after the use of spirituous liquors, as well as by the peculiar kind of ethereal odour in it, which can be produced though, only by the secretion of a yet unknown secondary product of the alcoholic combustion, which, with the water, is carried off also by the kidneys. The question now arises : During the general combustion of aldehyde, what is the behaviour of the remaining blood in the body, whose metamorphosis into carbonic acid and water is so retarded ? The retarding of oxidation affects principally the sugar. If this is not consumed, it must finally either accumulate in the blood, or be ejected as such (diabetes) with the excrement, or it must undergo some other kind of transformation. We cannot imagine an accumulation of sugar to remain for any length of time in the blood ; the author never found it in the urine of any animals experimented upon ; it must undergo, consequently, some other transformation. In relation to the appearance of milituria, the author begged attention to the fact, that many had declared that diabetes was frequently found among drunkards, and considered it possible that, in such cases, it was induced by the above mentioned causes.

The further metamorphosis of sugar is, however, most important. If it is not transformed into carbonic acid and water, it must change into fat, to which, probably, the excess of oxygen breathed, is applied. Herein doubtless lies the cause of the excessive formation of fat among drunkards

balanced by the increased respiration from the necessity for oxygen ; as was observed by the author in his experiments, and as is known in life. During intoxication, purer air is requisite ; therefore, in cold climates, or in winter, a larger amount of alcohol can be taken than in warm climates, or in summer. It seems, therefore, that the rapid absorption of oxygen performs here, another part. Those bodies which act in the same or somewhat similar manner, which either absorb oxygen rapidly, or impede its access to other substances, exhibit similar actions. The breathing of carbonic acid, and of hydrogen gas, produces insensibility and sleep. The intoxicating effect of protoxide of nitrogen is known. The action of opium, on the contrary, can not be explained in the same manner. In opposition to these substances, bodies, though of a like or similar combination, which are incapable of being oxydized, show no action analogous to that of alcohol.

Of the alcohols, the æthyl, methyl and amyl-alcohols alone act similarly, possessing a like affinity for oxygen. The æthel, a kind of fatty substance, which oxides slowly, acts, on the contrary, differently on the organism, as the author proves by experiments. Acetic acid, as above shown, is also wanting in that intoxicating principle, though it must finally be removed from the system by further oxidation. It is, therefore, not improbable that the rapid withdrawal, or the great want of oxygen, is essentially the fundamental cause of *most*, if not all the phenomena of intoxication. The trembling, the transient paralysis of the lower extremities—which we know to be the case with men, and which were not wanting in any of the author's experiments, cannot, therefore, depend immediately upon the chemical action of alcohol, nor their disappearance upon the inhalation of much oxygen during intoxication, which was observed by the author in his experiments upon birds and dogs. Death, in consequence of the so called alcoholic poison, must occur, agreeably to the above, when with the presence of a great amount of aldehyde in the blood, there exists not at the same time a sufficient amount of oxygen to transform it into acetic acid.

To establish the phenomena that occur after *long continued use of alcohol*, the author instituted several experiments upon dogs. In order to obviate the difficult application of the stomach pump—and to prevent the fluid from getting into the windpipe from the restlessness of the animal—he used gelatin-capsules which contained each, 5 grmms. of absolute alcohol. To a dog two years old, rather thin, and of small species, were daily given 15 grmms. of absolute alcohol, which was followed

probably contributed. The symptoms of weakness in the hinder limbs were also here apparent. The blood contained no sugar. To another young dog, corn brandy was given for thirty-two days. The same symptoms occurred. The blood contained much sugar. To a large setter, corn brandy was given daily for ninety-three days, from two to three spoonfuls. Upon this, he became very fat, and was well, when on the above named day, he was accidentally killed. The blood contained much sugar. There was much fat on the under skin of the cellular tissue, otherwise no anatomical change.

The anatomical dissection in the cases of sudden death by alcohol presents no change of any organ, of the brain or its membranes—in the lungs only, were found a spurious cedema. Emaciation was shown in long continued habits of intoxication; in the brain though, no change, not even that of thickening of the membranes and dropsy in the ventricles, quoted by Huss. In two cases, there was blennorrhœa of the external auditory canals. The stomach and its membranes were, in opposition to others' account, always normal. Only in one case, where but little amount of brandy had been taken, was much fat found. In conclusion, the author abridged his obtained results, as follows:

1. Alcohol undergoes in the system, continued combustion, of which, the intermediate products are found in the blood.
2. Intoxication is dependent upon the existence of aldehyde in the blood.
3. The action of aldehyde upon the blood, is the rapid withdrawal of its oxygen.
4. Therefore, the combustion of other substances, consequently, their metamorphoses are prevented or retarded.

On the Chemico-physiological Effects of Coffee. By JULIUS LEHMANN. (Medical Examiner).—After the conclusion of these investigations, I endeavoured to discover the mode of action of empyreumatic oil upon the system. For this purpose, I placed G. M. again upon the normal diet, then gave him, instead of water, the empyreumatic substance of coffee. To obtain this, I distilled a certain quantity of roasted coffee beans with water, until the residue had lost all odour. The distillate possessed the precise taste and smell of the decoction. G. M. received four glasses of it daily which contained the empyreumatic oil of two ounces of roasted coffee.

Investigation after the Urine of the use of the Empyreumatic Oil of Coffee.

Date.	Cub.	Cent.	Reac't	-

The average per day was :—urine, 1,789 ; phosphoric acid, 8,479 ; chloride of sodium, 10,807 ; urea, 20,271.

This produced a pleasant excitement and gentle perspiration, whilst, as was the case in using the decoction, the depressed feeling entirely passed away. The reason more than the imagination, exhibited the stimulating effects of the oil, which fact was afterwards confirmed by several experiments. The watery contents of the urine increased, whilst the phosphoric acid, and more especially the urea, greatly decreased. It appeared to have no effect whatever upon the relative proportion of the chloride of sodium. The considerable decrease of the solid constituents proved that the oil retarded the metamorphosis to a greater degree, than the caffeine, and therefore, to it particularly, must be ascribed this peculiar action of the decoction. Congestion, profuse perspiration, and sleeplessness, were the consequences of a double dose of the distillate. It produced on two other persons who drank it, besides the above effects, diarrhoea, thus showing that the increased peristaltic action must also be ascribed to it. If we now consider the results of all the investigations and experiments, we arrive at the conclusions :—

1st. that the use of the decoction of coffee produces two important actions in the system, which are difficult to harmonize, viz : stimulating the vascular and nervous system to greater activity, and at the same time considerably retarding the metamorphosis.

2d. That the stimulating effect above mentioned, so valuable to us from its reviving the wearied spirit, and disposing to thought, and promoting a feeling of comfort and cheerfulness, is due to the opposing modifications of the action of empyreumatic oil and caffeine.

3d. That the retarding of the metamorphosis is mainly due to the oil—the caffeine showing this *only*, when it exists in large quantities.

4th. That increased activity of the heart, trembling, dizziness, delirium, intoxication, &c., are the effects of caffeine.

5th. That the increased action of the perspiratory organs, of the kidneys, of the peristaltic action, and of the brain, are the effects of the empyreumatic oil.

If the decoction be too strong, that is to say, if it is prepared in such a way as to contain too great a quantity of both substances, for the system, then occur the effects peculiar to them both, trembling, activity of the heart, congestion, &c. &c. If we consider these two chief actions of coffee, which are peculiar to some other substances, such as tea, cocoa, spirituous liquors, &c., though in a modified degree, we observe that they are at variance with the general law, that the greater the expenditure of mental and bodily activity, the greater the waste—or rather, the metamorphosis of the system. Whether this excitement of the vascular nervous system, occasions more rapidly the destruction of the process of life, or how these two opposing effects are to be explained, remains for the future to discover.

Concluding Observations. One who is familiar with the general law

nction with the very small amount of actual nourishment that they are able to obtain. And, in fact, the existence of such persons would have been inconceivable, if no means of supporting their health and strength had been provided, beside that small amount of actual nourishment. Limited to that alone, there would have been a great disproportion between the amount received and that thrown off; the tissues must have suffered a continual waste, even to the dissolution of life. But instinct has taught them to make use of a substance, which is capable of making a too scanty nourishment sufficient to the system, and by preventing, in this way, the otherwise unavoidable disturbances of the balances of life.

It is particularly coffee, tea, cocoa, empyreumatic oils, and alcoholic liquors, that possess this peculiar influence upon the system, and most of these agree in producing that excitement of the nervous system of so great importance in social life.

If we now reflect upon the distribution of these substances, we find that one of them is used by the people as a common article of food, while the others are enjoyed by the higher classes as articles of luxury. In those countries where one or other of these substances is cultivated, its culture is considered as a matter of chief importance by the people; thus, in Arabia, coffee; in China, tea; in the wine producing countries, wine. On the contrary, where this is not the case, the choice has fallen almost exclusively on coffee or tea. The reason why the preference is given to these, particularly in Europe, is owing principally to the fact that they exert a far less injurious influence upon the system than alcoholic drinks, and, also, on account of their valuable action upon the nervous system from the united effect of coffee and empyreumatic oil. For while, by stimulating the reason and imagination, these prepare man for intellectual and bodily exertion; spirituous liquors, exciting only his imagination, which degenerates by slight excess into confusion of thought, cause, by irritating the nervous system, general debility. These latter, therefore, can be used with much less safety than coffee or tea.

An attentive consideration of the quality and quantity of the composition of coffee and tea, the various modes of preparing them, as well as their action upon the system in connection with the kind of food used, will probably account for the peculiar preferences of nations, for the one over the others. If the composition of tea leaves be compared with roasted coffee, we find that they both possess those substances that are of such great importance—thein, etherial oil, and protein substances. The only difference between them is, that the coffee contains an aromatic substance, and the tea a greater amount of thein, but especially of etherial oil. Three most important effects are produced by these constituents: 1. The retarding of metamorphosis produced by the thein; but more particularly by the aromatic substance in coffee. 2. The lengthened activity of the brain, produced by the special action of thein and etherial oil. 3. Serving as an actual means of nourishment, by the amount of protein substance contained in them. Coffee influences, more directly, the metamorphosis, and tea the nervous system. In order to obtain completely these effects of the two drinks, they must

be partaken of in substance. This occurs, however, only among a few nations, and is, probably, dependent upon the kind of food used, and their mode of life. The Orientals and Arabs regard coffee as a necessary article of food, and from their custom of drinking it with the *draga*, thus making the large amount of protein substance and inorganic constituents serve as nourishment, have unconsciously rendered their very frugal diet less sensible to them. If the plastic ingredients are not of themselves sufficient, yet here comes to their assistance the indirect nourishing property of *cafein* and the aromatic substance—equalizing their necessities, and, at the same time, exciting their nervous system.

The Central Asiatic inhabitants of the Steppes, the Buratians, the Mongolians, etc., etc., make use of tea as a common article of food. They prepare it, first, by rubbing the leaves together, then boiling it in water, adding a little salt to it; after they have poured off the decoction from the *drags*, they add to it butter and milk, and meal if they have any, which they roast before adding to the decoction. A person takes per day from twenty to forty cups. But even without meal, and only with a little milk, this tea often serves for weeks long, as the only means of nourishment. We see here again a people instinctively directed, as it were, in the peculiar mode of preparing tea; unconsciously adding those substances which are of such great importance, viz. the protein, that become soluble by boiling in salt water, and, also, a great portion of the inorganic constituents. It is probably here the large amount of thein that influences the metamorphosis. Of course they lose entirely the valuable property of the united action of etherial oil and thein, since by boiling the former is dissipated. This could better be spared, though, than the other action of tea, which serves as a direct nourishment, which is of greater importance.

The mode of preparing these drinks among Europeans, is very different—not placing much value upon the nutritive substances contained in them, but merely upon those which are capable, indirectly, of nourishing, and those which induce greater activity of the nervous system and the brain. The mode of preparing coffee among the Germans enables them to obtain, besides the empyreumatic oil, as much *cafein* as they can possibly obtain from it; while, in the preparation of tea, less attention is paid to the amount of thein, than to the etherial oil—the whole amount of which passes over to the decoction. Hence, with them, tea acts only as a stimulus to their brain and nervous system, whilst coffee retards the metamorphoses, and, also, though not in so great a degree as tea, stimulates the nerves. The English, who produce so much meat in their own country, and can generally obtain so much of it that it is even possible for the poorer classes to partake of it daily, besides the protein substances, have less need to prepare their coffee in such a manner as to obtain from it indirect nourishment. Since, however, they feel the want of spirits and excitement, their choice has fallen upon tea, which promotes these better than coffee.

We find among the poorer classes of Germany, where meat is so rare and considered so great a luxury, that they are obliged to substitute for it such poor food as potatoes, etc., which consists of substances which indeed satisfy their hunger, but yet are not fitted to prepare them

for very great exertions. The feeling of mental and physical lethargy experienced by them, and produced by this spare diet, becomes more evident if they suffer from the want of coffee. The rapid diffusion and yearly increase of the consumption of coffee, is an evidence of the great importance it has attained in social life.

Whilst, at the beginning of the last century, it was considered a luxury by the higher classes, it has now become a necessary article to all classes, wherever food is scarce and dear. Of its total production, which amounts annually to 600,000,000 of pounds, two-thirds are consumed by the Europeans, whose exertions appear so disproportioned to the actual means of nourishment. In the Zollverein states of Germany, the consumption of it amounted, in 1851, to one hundred million of pounds, or one sixth of the total production. Upon each reduction of duty, millions of pounds more were consumed by the people. The consumption of coffee and potatoes, from the period of their general introduction, have gone hand in hand. We see the poor instinctively valuing coffee more, the more they are limited to potatoes as their chief food.

The Substitute (Surrogate) for Coffee.—At the time of the continental embargo, the want of coffee was felt for the first time in Germany, and the necessity of possessing some such drink, led to the adoption of other substances produced there, to which, by roasting, a taste similar to that of coffee could be imparted. Notwithstanding the general introduction of coffee again, and its greatly increased production in the colonies, and the reduction of duty, which naturally made it cheaper, the consumption of the substitute was not lessened, but increased, so as yearly to amount to 100,000 cwt. The opinion that Knapp, as well as other authors, have started, that this substitute is entirely deficient in those substances, so valuable to the poorer classes, can be refuted, if we examine its composition, and remove the idea that caffein is the only efficacious substance in coffee. The materials which they have chosen as substitutes, and which may be increased indefinitely, all undergo the same process as coffee, viz., roasting; by which the aromatic substance is evolved, and by which is effected that retardation of the metamorphoses of such great importance to the poorer classes. The roasting thus produces not only a taste similar to coffee, but also imparts to it one of its most important properties. Still we cannot be indifferent to the great deficiency that exists in the substitute, viz., the total absence of caffein, thus losing that valuable influence upon the vascular and nervous system, and upon the brain. Since, however, Surrogate is about from five to eight times cheaper than coffee, and possesses a part of the peculiar action of the latter, we cannot wonder at its having attained, among the poor, to such a general use.

Infantile Leucorrhœa. (B. and F. Med. Chir. Review.)—In several numbers of the "Medical Times and Gazette," for September 10th to October 29th, will be found the *History of a recent epidemic of Infantile Leucorrhœa, with an account of five cases of alleged felonious assaults.* By W. R. Wilde, F. R. C. S., &c. "Considerable excitement," Mr. Wilde states, "has prevailed among all classes in Dublin during the

last month, owing to the circumstance of no less than three cases of felonious assaults upon children under ten years of age having been brought forward by the crown at the late commission before the chief justices."

A correspondence has been published in the "Freeman's Journal," between Dr. Ireland, physician to the police, upon whose information the cases were sent for trial, and Mr. Wilde, who had one of the accused persons defended. Most of the leading members of the profession in Dublin gratuitously tendered their evidence in court, "in what they considered the cause of truth, science, and humanity." The occurrence of this form of *vaginitis*, in an epidemic form, as shown by Mr. Wilde, is perfectly well known to most practical physicians and surgeons.

Mr. Wilde notices the delusion, which is extensively prevalent in Ireland, to the effect that a man can get rid of an obstinate gonorrhœa by having connexion with a virgin; and as the easiest and surest mode of effecting that, a child of tender years is selected; hence the felonious assaults occasionally attempted, and for which men have been most justly convicted, and most righteously punished. But in all such cases it has been proved, that the men, after the commission of the crime, still laboured under gonorrhœa or venereal, although the popular opinion among the lower orders is, that the disease is not only completely, but instantaneously, transmitted from the male to the female. A similar superstition, it appears, is found to exist among the Arabs, as stated by Mr. Duchesne, in his recent work on "Prostitution in Algeria." It is the knowledge of this wide-spread superstition, which leads the mother at once, on the appearance of vaginal discharge, to jump to the conclusion that impure connexion has taken place, and possibly she may be confirmed in this idea by some medical men not conversant with the true nature of the affection.

Mr. Wilde dwells upon the suggestions, insinuations, and threatenings, which is usually had recourse to in these cases, in order to extort a confession of connexion, and very justly remarks that it is not likely a child, who has neither passion nor love to influence her, will conceal the fact from her parents or near relatives, when hard pressed. In Mr. Wilde's contribution, it is stated that one mother, on her own statement before the jury, actually threatened to cut her child's tongue out if she did not confess to the connexion. Besides the three cases already referred to as having become the subjects of trial at the late Dublin commission, Mr. Wilde cites six other instances of epidemic leucorrhœa that have been brought under the notice of the profession, either at public institutions, or through the medium of the medical journals. For the details of these cases, and of the trials, we must refer our readers to the "Medical Times and Gazette."

Poisoning with Nux Vomica, Recovery. (London Lancet.)—The patient, a labourer aged twenty, was admitted into the royal free hospital, under the care of Dr. Hussell. Three-quarters of an hour before admission, he had taken about one drachm and a half of powdered nux vomica. When admitted, he was in a profuse perspiration; the skin of

the face, neck, and chest, was greatly congested, the eyes suffused, the pupils slightly contracted, the pulse hard and excited. A few minutes after admission, a tetanic paroxysm came on suddenly, the man was thrown into a state of opisthotonos, all his muscles became rigid, and respiration was for a time suspended. The fit lasted about half a minute, when the muscles became relaxed, and he was again able to answer questions. He had had several of these fits before admission. The first occurred about ten minutes after he had swallowed the poison. They gradually decreased in severity; altogether he had five. On the following day crampy pain in his limbs were complained of. Emetics were given without effect. The stomach-pump was used, and removed some greyish powder. Aperient and saline medicine was given. On the second day he was discharged well.

The exact quantity taken could not be ascertained. Enough, however, evidently was swallowed to produce serious symptoms. Dr. Alfred Taylor quotes an instance where five grains were fatal: and another instance, of recovery after half an ounce had been taken.

Oxygen an Antidote for Chloroform. (B. and F. Med. Chir. Review.)—Dr. Abrahams, of New-York, has successfully employed the inhalation of pure oxygen gas in the case of a young man asphyxiated by the vapours of chloroform, and apparently *in articulo mortis*. Dr. Abrahams suggests the employment of this agent in suspension of life from drowning, from inhalation of the fumes of charcoal, from the fixed air of pits, mines, &c.

Chloroform in Midwifery Practice. By Dr. ROBERT LEE, M.D.—Dr. Robert Lee read a paper before the Royal Medical and Chirurgical Society, in which he gave an account of seventeen cases of parturition, in which chloroform was inhaled with pernicious effects. In the cases related, the injurious effects of the inhalation of chloroform were as follows: In seven cases, insanity and great cerebral disturbance followed its exhibition. The use of the forceps rendered necessary in five cases. In two, the contractions of the uterus were arrested, and the operation of craniotomy was performed. Peritonitis or phlebitis ensued in four cases. Epilepsy or dangerous fits of syncope supervened in two instances. Many analogous cases had been confided to the author by friends; and public rumour swelled the size of the chapter of accidents: but he wished merely to give accounts of those which had come under his own observation. The author strongly animadverted upon the levity and thoughtlessness which had accompanied the use of this subtle and dangerous poison. Soon after its discovery, before the amount of its power, and even its composition had been fully understood, he had been horrified by the announcement of its application to midwifery, and he prophesied that deplorable results would ensue; a prognostication which

of those who pointed to the evils that might occur. The author expressed his opinion that the most serious effects which arose from the inhalation of this agent, were, first, languid and inefficient uterine contractions; secondly, a greater susceptibility to the risks that arose from inflammation and fever. In spite of the fact that grave and experienced physicians had expressed their concurrence with the author's views, yet the question whether chloroform should be inhaled, had become almost extra professional; as silly and ignorant women of fashion chose to set the example of using it, the cause of science and humanity, thus being in the hands of the most weak and frivolous portion of the community; and, as there was a systematic concealment of the truth by the physicians who used it, he feared that young and inexperienced mothers would still be lured to their destruction. In conclusion, the author expressed a hope that his essay might tend to rescue the profession from the dominion of an ignominious and disgraceful practice.

Poisoning from Atropia. (Association Journal.)—The "Gazette des Hôpitaux" gives an account of a case of poisoning with atropia, remarkable from the small quantity employed, and the place of application, viz., the healthy conjunctiva. Three or four drops of a solution, containing about one grain of atropine to two ounces of water, acidulated with acetic acid, were instilled into each eye of a patient with double cataract. Half an hour afterwards the patient suffered from vertigo, &c.; three-quarters of an hour later he manifested all the symptoms of poisoning by belladonna. The symptoms gradually passed off, but the patient did not recover his usual health for three or four days.

Poisoning from Fruit of Belladonna. (Jour. de Chimie Medicale.)—Two children, respectively seven and nine years of age, alarmed their parents by walking in the night, both in a state of violent maniacal delirium. On investigation, it was discovered that they had eaten an unknown quantity of the berries of belladonna. Two adults had also eaten some of these—the one a man, who had swallowed about a dozen, became as if in a state of inebriation—the other, a woman, having eaten only three, suffered severely from vertigo. Under medical care they all recovered.

Poisoning with Ink. (Pharm. Journal.)—A case is quoted from the Austrian "Journal of Pharmacy," in which severe symptoms of gastro-enteritis were caused by drinking a glass of ink by mistake for a glass of porter. The patient recovered, after having suffered for many days from weakness and trembling in the limbs, headache, &c.

Poisoning from Laburnum Berries. (Morning Herald.)—Several

EDITORIAL AND MISCELLANEOUS.

Dr. Alex. Turnbull.

IN answer to numerous inquiries which have been made concerning the scientific attainments of Dr. Turnbull; and, also, to correct the erroneous impressions which have gone abroad through the medium of the public prints, we undertake an exposition of his career; in doing which, we are deviating from our accustomed course, and giving undeserved importance to one who would not otherwise have occupied our attention. The unblushing assumptions of this person to perform impossibilities, and the rumours of his pretended cures, have so impressed the minds of the credulous, that it becomes our duty to warn our readers, who may have friends or patients labouring under diseases of the eye or ear, from crediting the recommendatory advertisements which have been in circulation, respecting his "simple and painless processes," by which diseases hitherto beyond the resources of art are speedily and permanently, cured.

Many an anxious parent has been subjected, on the faith of these advertisements, to serious inconvenience and considerable expense, in the anticipation that soon their deaf-mute children would be restored to the healthful use of their dormant faculties. From day to day they were encouraged to hope, until, wearied by a longer attendance, or being assured that time alone was required to perfect a cure, they have departed in trembling anxiety, too soon to have the unwelcome truth forced upon them, that the objects of their solicitude were as hopelessly incurable as ever.

Some time since, Dr. Turnbull came to this country, and soon rumours reached us of his curing all manner of *deafness and blindness*; in due time Dr. T. reached our city, and took rooms at one of our hotels, where he announced himself as being prepared to treat the diseases of the eye and ear. For a time, he attracted very little attention; but, after treating divers cases, two were announced in a respectable city paper, (*Charleston Courier*), as having been cured by his peculiar processes.

"One, a boy from Georgia, now nine years old, who had lost his hearing from fever in his third year," and been since that time com-

in the slightest degree restored; and, on seeing the above advertisement, the father became indignant, and immediately telegraphed the Augusta papers not to copy it.

"The other case is a youth from this city, aged 18, whose infirmities (of hearing and speech) being in some sort hereditary, and of a most decided character, afforded a crucial test of Dr. Turnbull's processes. In this interesting case, also, the progress of improvement has been rapid and uninterrupted, and the patient is now hopefully undertaking the education which is necessary to give him the due use and appreciation of his new born senses."

This case is the only one, that we know, which has been benefited. But there is no miracle here. There was only an absence of secretion in the external meatus, and the sense of hearing was obtuse, and was rendered less so. The patient has been under treatment for several weeks, and is not yet discharged.

We are informed, in the same Journal, that Dr. T. has contributed largely to the leading Medical Journals of England, and that "his professional status has been recognized and approved by the most competent European authority, and the value of his discoveries and contributions, in medical resources and remedial agencies, has been cheerfully admitted and abundantly attested."

Before commenting on these statements we will sketch the history of the Doctor, and present the opinion of the profession in England concerning him.

In 1835, there appeared, in London, a duodecimo of 171 pages, "*On the Medical Properties of the Natural Order, Ranunculaceæ and their Alkaloids, Veratria, Aconitine, Delphinia und Sabadilline.*" By A. TURNBULL, M.D.

The reviewer of this work, in the "British and Foreign Medical Review," says that "if one quarter of what it contains be true, (and surely that would not be an unreasonable requisition,) Dr. Turnbull's alkaloids are sufficient to change the face, not only of medicine, but of society."*

"Of nine cases of affections of the heart, veratria is said to have cured or relieved all: of thirteen cases of neuralgia. to have cured

the text, with the words 'leaving no trace of the affection behind, neither has any renewal of it taken place?' (p. 70.) The note, however, a traitor to the text says—'In this instance the veratria has completely failed in giving permanent relief,' &c." (*B. and F. Med. Review*, vol. ii, p. 499.)

These alkaloids were given a fair trial by the profession of that day, but no such effects followed their use as had been attributed to them by Dr. Turnbull. Veratria and Aconitine have been extensively employed down to the present time, and, though they sometimes are found useful as topical applications in neuralgia, like other medicines of their class, they are, internally, still of doubtful efficacy.

A "*Treatise on Painful and Nervous Affections, and on a New Mode of Treatment for Diseases of the Eye and Ear*," by A. TURNBULL, M.D., appeared in 1837. The treatment of the affections of the latter organ, comprised in a few pages, consisted in the application of the alkaloid veratria to the external meatus and the adjoining parts. The "electro-stimulation," as the author styles it, having proved so efficacious in curing deafness, he determined to experiment upon the deaf and dumb, and soon cures were announced as having been effected by this means. The possessor of this remedy betook himself to Scotland to operate upon the deaf-mutes of that country, but he was not so successful as might have been expected; still rumour said that he had cured several cases in the Deaf and Dumb Institution of Edinburgh, and it was not until some time had elapsed, that Mr. ROBERT KINNIBURGH, the intelligent head master of that institution, publicly asserted that no cure had been effected.* His cures were also questioned, and his statements severely criticised in *Chambers' Journal*, for September, 1839; but, three years afterwards, we find the *Edinburgh Journalist* retracting his former strictures, and lauding the achievements reputed to have been performed by Dr. T. in his tour through Scotland.

The editor tells us, that wishing to arrive at a thorough conviction, he "studied the most recent and approved works on aural surgery, in order to ascertain in what respects Dr. Turnbull's practice differed from that which was general in the profession." Now it is impossible to understand, or appreciate the treatment of a class of diseases, until the anatomy, physiology and pathology of the organs involved, have been thoroughly studied. An unprofessional man cannot be convinced of his error, or justly estimate the merits of a cure, or the causes of failure.

Here is what this observer relates :

"Experiment in time showed that in cases of deafness arising from low nervous energy, a class to which nearly all the cases of the deaf-

* Vid. Wilde's on Diseases of the Ear; Am. Ed., p. 55.

dumb belong, the organ may be more or less successfully treated by applying to it a weak alkaloid. This he rubs gently on the tympanum by means of an instrument tipped with chamois leather, and generally in ten minutes the effects are manifest.

"But the greater number of diseases of the ear arise from cold, producing acute and chronic inflammation, and diminishing or altogether obstructing the flow of wax, whereby the tympanum and other parts of the outer ear, from being exposed to the air, are thickened, and so deprived of sensibility. Cold also produces inflammation and consequent accumulation of mucus in the passage called the Eustachian tube, which communicates between the internal ear and the back of the mouth. Finding cured persons relapse in consequence of the defect of wax, Dr. Turnbull was prompted to use his ingenuity in endeavouring to discover the means of sustaining that secretion. He reflected that the application of the mouth of the child to its mother's breast, by removing the pressure of the atmosphere, causes the milk immediately to flow, and he conceived that a similar result might follow with respect to the wax of the ear, if he could by any means remove the pressure of the atmosphere from the external parts. For this purpose he at first used a syringe with an India-rubber mouth, exactly fitted to the aperture of the ear. The plan was successful; the blood vessels resumed a free circulation, and the flow of wax recommenced. It strikes us that we have rarely known a more beautiful instance of a simple natural principle being taken advantage of by human ingenuity for a human end."

Had this writer profited by his studies, he would have known that there is no analogy between the mammary gland and the follicles of the meatus, or between their respective secretions. The flow of milk from the former, by the removal of atmospheric pressure, is in obedience to a physical law, while the secretion of the latter depends upon the normality of the secreting follicles, and is poured out independent of any atmospheric condition.

Again:—"The clearing of the Eustachian tube, for which no means formerly existed but the application of medicines to the bowels, (!) or the *dangerous* use of the catheter, was effected by Dr. Turnbull by the same simple means."

Here we have a veracious, but misguided man, stating his convictions, and imparting to the world the value of remedial agents, about which he really knows nothing; showing too clearly, by his laudation of Dr. of T., that he was the dupe of that person. Catheterism of the Eustachian tube is now well known to be a safe and simple operation. From the time of its first performance by Guvot. to the pre-

have never known any unpleasant consequences ensue. But we need not be surprised that the friends of Dr. T. should call it a *dangerous* operation; for the only two cases recorded of injury having resulted from this simple procedure, occurred to Dr. Turnbull himself, *two of his patients having fallen victims to it, and on both of whom coroner's inquests were held*. One of these, aged 68, was, almost immediately after the operation attacked with a violent swelling in the throat, of which he died on the tenth day; the other, aged 18, in the enjoyment of perfect health, *died whilst undergoing the operation*. Messrs. Liston, Quain and Savage, were present at the post mortem examination, and stated, as their opinion, that death was caused by the forcible injection of cold air into the tympanum.*

After this sad affair, Dr. Turnbull resorted to a more "simple means" of clearing the Eustachian tubes, as indicated above. This he does by means of an air pump, in connexion with a flexible tube, "introduced into the mouth of the patient, and applied to the orifice of the Eustachian passage. A communication is thus opened between the previously rarified air in the receiver and the orifice, from which a discharge of mucus is made to enter the tube, which is then withdrawn."

In 1841-'43, Dr. Turnbull is again heard of in new fields of discovery. The Literary Gazette, whose glaring advertisements, on a former occasion, attracted the attention of the Edinburgh Journalist, is again in the van; and we read of *cures of blindness by the fumes of Prussic acid*.

We will let the writer speak for himself:

"The various stages of cure, advanced in our presence, by the simple application, for about half a minute, or until a little warmth was felt by the patient, of the vapour of hydrocyanic acid in a small phial, held up to the eye, with an aperture fitting the form of that organ; the various nature of the diseases so assailed—opacities of the cornea, inflammation, cataract, amaurosis, iritis, &c., &c.; the various stages of relief which the patients had reached, with sometimes one eye opened to sight and pleasurable to look upon, and the other left nearly blind and in its pristine deformity, to show what had been achieved; the various appearances of films removing, cataracts breaking up and being

began to sound Dr. T's praise abroad. Their puffs and notices, spread over many pages of their Journal, may be viewed as a popular report on Ophthalmic and Aural Surgery.

Dr. T., emboldened by such commendations, published a short letter in the *Lancet*, (Sept. 28th, 1841,) containing a few "hints on the effects of hydrocyanic acid upon the Eye." His pamphlet, a small octavo of eighty-nine pages, on the "*Treatment of the Diseases of the Eye, by means of Prussic Acid Vapour, and other Medicinal Agents*," appeared two years later. Dr. Turnbull having had a little practice in authorship, now indulges in theory, and, in his own (original) way, attempts an explanation of the *modus operandi* of the medicaments he prescribes.

"It is a well known fact," he says, "that the eyes of those who have been destroyed by hydrocyanic acid, for a length of time after death show none of the usual symptoms of dimness; on the contrary, the eye is clear and the pupil much dilated. This satisfied me that the acid exerted a specific action upon the eye, which might be made available, as a medical agent, for relieving many of the diseases to which that organ is so subject"—(p. 4.)* The condition of the eyes, after death, of those who have died from the effects of Prussic acid, are well known to be as Dr. T. describes, but the inference drawn from the fact is preposterous and absurd; for, as those who die from the effects of carbonic acid, or those who expire in a fit of apoplexy, present the same appearance of the eyes, we may, by the same analogical mode of reasoning, infer that the fumes of charcoal, and slight congestion of the brain, are specific remedies for diseases of these organs.

The explanation of the therapeutic process is as follows:—"The plan I generally adopt is to put into an ounce phial a drachm of the acid, and hold it in close contact with the eye, the eyelid being open, for the space of about half a minute, or until such time as the patient feels a little warmth, or the person holding the phial sees the pupil greatly dilated and the vessels of the eye injected with blood, which is the invariable effect of the application of the acid. The patient is not sensible of pain from this peculiar state being induced, which appears to me to result from the powerfully sedative influence of the acid, thereby showing two opposite powers, to wit: stimulating and sedative, are exerted at the same time, and thereby the uneasiness, arising generally from a

*person who holds the acid to the eye should be careful not to allow the patient to smell it."**

This is original indeed! Pain is avoided, while incipient inflammation ("the vessels being greatly injected") is produced, which cures cataract, iritis and amaurosis. Diseases essentially different, resulting from diverse causes, are thus cured by stimulation, carried further than could otherwise be done by the sedation, which is also an effect of the vapour! But why, we may inquire, is the bottle-holder warned to remove the phial, for fear of too great an impression being produced, if the two forces are antagonistic and of equal power? In accordance with the above theory, it may be suggested that the forces will necessarily counteract each other, and no effect will be produced.

Dr. T. uses the essential oil of bitter almonds in the same diseases, feeling convinced, he says, that it is more soothing, relieving pain without dilating the pupils or causing much redness of the eye, and being useful, also, in taking away the heat occasioned by the hydrocyanic acid. We may ask, if this harmless remedy fulfils all the required indications, why use the most dangerous? and why, on the same page, assign the same reasons for preferring and rejecting each alternately? But the Doctor is not satisfied yet; he has discovered *newer* remedies, and must consequently apologize for their introduction, which he does by disparaging his former favourite.

One reason why he did not rest satisfied with the great effects produced by the Prussic acid was, that its action, like that of all other medicines, decreased in power by continued application, thereby rendering it necessary to have occasional recourse to other medicines, in order to ensure a more *speedy recovery*. Another reason was, the reluctance of many individuals to submit the eye to the action of so potent a medicine. Well might a prudent person object to allow the vapour of concentrated Prussic acid to be brought in contact with his eyes, when such authorities as Pereira and Christison state, that, if a drop of the pure acid be placed on the throat of a dog, or applied to the eye, death takes place in a few seconds; and that, inhaling the vapour, decidedly produces death more quickly than any other mode of using the acid.

bon causes an intense prickling sensation, yet for reasons known only to Dr. T., they all cure "*blindness*."*

The following is the ingenious explanation of the *modus operandi* of these substances when applied to the skin: "The action of these medicines, which contain so large a share of carbon, arises from the carbon in the vapour permeating the cuticle, and coming in contact with the oxygen in the vessels, which is conveyed through every part of the frame by inspiration and otherwise, thereby forming carbonic acid gas, which evolves heat in the ratio of the quantity consumed by the oxygen." On page 70 of his work last noticed, he says, "the rationale, in my opinion, is, that the large quantity of carbon in some of the essential oils, and its solubility in alcohol, permits it, by friction, to pass through the cuticle and unite with the oxygen."

The long received theory of the cure of amaurosis by stimulating the nerves of the skin, is then no longer tenable after the above lucid explanation.

We have seen that Dr. Turnbull's "simple means" for the cure of deafness, was sufficient to cause the death of two of his patients. But since he has resorted to the vapour of concentrated Prussic acid, as a local application to the delicate surface of the eye, we hear of nothing more serious than frequent failures, some of which necessitated the refunding of his fees.†

From our references, it has been seen in what repute Dr. Turnbull is held, and what has been considered the value of his medical contributions, by his own countrymen. We have not particularized the unprofessional and disreputable acts of Dr. Turnbull, which we find recorded in the medical periodicals of his country; for so numerous have they been, that it would look like persecution to repeat them here; and our readers would have cause to complain of the wearisome length of the sketch we have drawn.

But before dismissing the subject, we must advert more particularly to some of the pretensions of Dr. Turnbull, and the results of his practice as it has come under our observation. He not only attempts to

covered the use of the organ of hearing as to be able, under all circumstances to communicate with other persons in an unrestrained manner.*

But, when this condition has been produced by some mechanical impediment, or by some pathological state of the organ or derangement of its function, it may be removed. And if the disease has been of long standing, a cure may sometimes appear miraculous, indeed, to unprofessional persons.

There is more room for deception, in respect to the sense of hearing, than may be generally known. A deaf-mute can be taught to speak and answer questions addressed to him, without hearing a single note. Only two years since, when visiting the Institution for the Deaf and Dumb in Berlin, we had the opportunity of satisfying ourselves of this fact. By a judicious system of training, the inmates of this institution are not only taught to understand spoken language, by construing the movements of the lips of those who address them, but so to appreciate the movements of their own, by the muscular sense entirely, that they are enabled to articulate distinctly, and thus communicate with others in a satisfactory manner.

Instances may be adduced, where some of Dr. Turnbull's deaf patients actually asserted that they heard the ticking of a watch at a greater distance than persons in full possession of the sense of hearing could do. Without wishing to deceive, these persons, with their imaginations highly excited, could readily mistake the conception arising in their own minds for the sensation produced by an external agent. And those most interested in the success of the treatment, are not in a condition to detect the imposition, even were they able to apply the philosophic explanation to the phenomenon.

But it is only to the public, and not to the profession, that Dr. Turnbull makes these pretensions—implying that *he* can do what no other *mere* man can do. If the power of giving sight to the blind, hearing to the deaf, and speech to the dumb, was imparted to only *one*, who was more than man, then such power was miraculous, and cannot be arrogated by any erring mortal.

As intimated before, however, we doubt the efficacy of the means he employs, and, from our observation of the cases which have been subjected to his treatment, we must regard him as decidedly less successful

many have left for their homes without having experienced the slightest amelioration of their infirmities.

[We regret that the following paper from Dr. Marshall Hall, detailing his experiments before the members of the medical profession of Charleston, was received too late to appear under the head of original communications.]—Eds.

Experiments on the Spinal System in the Alligator (Alligator Mississippiensis). By Marshall Hall, M.D., F.R.S.

HAVING been kindly provided by Dr. Miles and Dr. Michel with two alligators, I was happy to seize the opportunity of demonstrating the spinal system before my medical brethren of Charleston, S. C., experimentally, adding a brief commentary.

Dr. Miles, at my request, first divided the nervous centres, just below the encephalon, leaving within the cranial cavity a portion of the medulla oblongata. Immediately *all spontaneous* movements of the trunk and limbs *ceased*.

We placed the animal in positions which would have been the most painful, if sensibility still remained, yet it retained those positions unmoved.

1. There was no movement, then, the result of suffering.
2. There was no movement of a spontaneous character.

We concluded that *sensation*, or *volition*, or both, in the *trunk* were *extinct*. We now applied various irritants, freely, to the integuments, such as the heat of a taper, pricking, pinching; there were then movements of the most energetic kind; the *power* to move—the *excito-motor* power—remained in full force. It was observed that some of these movements were *similar*, in form, to those performed in the perfect animal under the influence of pain and as acts of volition; a limb would be brought near the part irritated, *apparently* to remove the cause of irritation—therefore, with *design*. But we had concluded that sensibility and volition in the trunk and limbs were extinct. Where then was the truth, where the fallacy in this matter?

The following observations were offered:

1. In every voluntary act in the perfect animal, two powers are exerted—the first is volition, the second, excito-motor power. These usually *coincide*, and cannot then be distinguished; but sometimes, in disease, they do not so coincide; the *excito-motor* power acts in ano-

other manner and direction; the pen, for example, in writing, however, guided by volition, is frequently turned into an erroneous position, and the patient cannot write. In such cases we see the action of volition interfered with by the excito-motor power, and we perceive the distinct agency of the two powers.

2. If, in experiment, we remove volition, but leave the excito-motor power unaltered, it is obvious that its action will be *similar* to those of volition, with which, in the human subject, we observe the entire exhaustion of sensation and volition, with the persistence of the excito-motor function; and we have, in cases of perfect paraplegia, human testimony to the absence of consciousness and design, when actions *resembling* those of design are produced; so in the perfect animal, they would coincide;—that is, there will be the *appearance* of the *design* manifested by acts of volition.

1. We now removed a portion of the integument from one posterior extremity, and we irritated both extremities—one on the spot denuded of its skin; the second, on the corresponding spot; the former limb remained unmoved, the latter moved as before.

2. We divided the crural nerve in the other limb.

This limb had now lost its reflex actions in its turn.

It was concluded from these experiments that the reflex, or excito-motor act is—1st. *originated* in the integuments; 2d. proceeds along the ingoing or isodic nerve to the spinal centre, whence it is reflected through out-going or exodic nerves to the muscles excited into contraction, demonstrating a new kind or form of anatomy.

3. We now removed the head and *eviscerated* the animal entirely; the phenomena remained identically the same. We now separated the tail; the phenomena were still the same in the head, the trunk and the tail. If a taper were brought near the tail, this turned to the side irritated, again recalling the *idea* of *design*.

We now irritated the upper portion of the spinal marrow; the animal was thrown into convulsions; this experiment presented the type of direct or centric epilepsy, as the former reflex actions had presented the type of eccentric epilepsy.

We now denuded the dorsal portion of the spinal centre by removing the body of one of the vertebræ; irritation of this portion of the spinal marrow induced movements of *all* the extremities; its action was both *retrograde* and *direct*.

When the portion of the spinal centre left within the cranial cavity was irritated, the maxillæ closed by retrograde action. When the upper portion of the spinal centre within the tail was irritated, movements of direct action were again produced.

It may be received as an axiom in physiology, that each and every muscular contraction is the result of a *distinct* stimulus. The stimulus, in the experiments detailed above, is a *fresh* or *renewed* excitation. *Continued* excitation does not produce any motor phenomena. Continued movements, of whatever character, are, in reality, the result of repeated excitations. Continued pain excites distinct acts of volition; but a wound, here the most painful, excites spinal movements, however complex, but once.

The American Journal of Pharmacy, Ourselves and Quackery.

In an editorial, with the caption "Professional Quackery," in the January issue of the American Journal of Pharmacy, after quoting our notice in our last November number, in relation to the Tincture of *Veratrum Viride*, as prepared by Dr. Norwood, the editor comments upon it as follows :

"We have, on another occasion, (vol. xxv., page 109, of this journal,) taken the opportunity to bring forward Dr. Norwood's statements with regard to the curative properties of *Veratrum Viride*, and then published his formula for the tincture from the *Southern Medical and Surgical Journal*, January, 1853, which consisted of eight ounces of the root in sixteen ounces of alcohol .835 sp. gr. macerated two weeks and expressed. It now appears that Dr. Norwood's preparation proves ineffectual unless *he* prepares it, and that his claims as a *pharmaceutist* are upheld by one of the most prominent medical journals of this country, as above. As addressed to Southern medical practitioners, the above article may not be considered very objectionable; but when it is known that Dr. Norwood has the tincture prepared for him "*at the North*," and that he has copyrighted his labels, (one of which is before us,) and has placed the general agency of his tincture in the hands of a house largely engaged in the preparation and sale of secret medicines, we cannot but look upon the notice of the Charleston journal as abetting a species of quackery, which, like McMunn's Elixir, and other similar preparations, is not less quackery because the active ingredient is known. Dr. Norwood, as a citizen, has a perfect right to make and sell his tincture, and to seek a monopoly of its sale, but if he does so under the declaration that it is *peculiarly* valuable, whilst the recipe for making it has been published by himself, he not only cannot expect to retain the position of a disinterested member of the medical profession, but by thus upholding the spirit of quackery, however unintentionally, he runs

the risk of being classed with those physicians who have sold their professional birthright for a mess of pottage."

Our respected *confère*, the editor of that Journal, has entirely misapprehended our motives in inditing the notice in question, and consequently has done us unintentional injustice. The charge brought against us seems to be predicated upon the fact, that we *knew* that Dr. Norwood's Tincture was prepared "*at the North*" and *not* by himself, at his residence in Abbeville, S. C. The whole matter hinges upon this point. The fact is, that we, all-along, have been of the impression that, although, the root was gathered at the North by Dr. Norwood, or his agents, yet it was prepared *by himself*, at his residence, and *not by a Northern house* largely engaged in the manufacture and sale of secret medicines. We were led to believe (although it is not so stated by Dr. N.) by Dr. Norwood's communications to our Journal, and others at the South and West, as well as by his private letters to us, that the preparation of the tincture was carried on by himself, and by him directly distributed to certain druggists whom he appointed his agents throughout the cities and towns of the South.

The editor objects to us, that we consider Dr. N.'s preparation as superior to every other, simply because it was prepared by himself, notwithstanding the circumstance that he has given the recipe for making it, and that others can prepare it as well as he himself. We never maintained that Dr. Norwood is a better *pharmaceutist* than others who are properly instructed; but we advised our readers to obtain the *Veratrum Viride* immediately from him, or from his authorized agents, for the great and obvious reason, that the tincture manufactured at the North and sold by our druggists could not be relied on; in other words, it proved to be by no means so certain or efficient in its action, as that purporting to be prepared by Dr. N. If necessary, we can produce the certificates of druggists and physicians, in this city, to substantiate that fact. The difference in the virtue of the different preparations, we presumed, was due to the circumstance that those who gathered the root, and made the tincture at the North, either did not get the right species, or that, if they did, it was collected at an improper season, and was, perhaps, not well dried.

"The very head and front of (our) offending, hath this extent, no more."

The shaft of our contemporary appears, after all, to be directed rather

the most politic plan of effecting a ready sale of his preparation, or that his conduct was such as to receive the unqualified approbation of the profession. We now think that Dr. N.'s offence is much palliated by the reasons assigned for such proceeding, in a card published in our November number of vol. vii.

AN EPIC POEM, by a Western Medical Editor.

A society—composed of students in attendance on the medical lectures at the University of Nashville—published, in the February number of the “Nashville Journal of Medicine and Surgery,” a series of resolutions, denunciatory of a Notice, which recently appeared in the “Southern Journal of Medical and Physical Sciences,” of an “Address” delivered at one of their meetings by a “Western Medical Editor.”

We read the notice alluded to, and, from the reviewer's earnest manner, and strong expressions of indignation, we were persuaded that the subject of it must be of an extraordinary nature.

We soon received a copy of the “Address,” with the author's request to “read it with a critic's eye.” It proved to be a poem—a sort of Hudibrastic epic—mythologically delineating the origin and progress of quackery. The author, with considerable ingenuity, allegorically accounts for the spread of this vile system, and, had he continued to the end, the same strain of lively satire which pervades the first half of the poem, we would heartily have commended his production. But he has unfortunately overstepped the bounds of propriety, and, in satirizing the vulgar agents of a polluted system, he has degraded himself by the portrayal of scenes of the grossest obscenity, in language unbecoming himself and his audience.

Had we penned these lines at the time of reading the “Address,” we might have condemned it in stronger terms than we now have done. The reviewer above alluded to, though harsh, is, we think, justifiable in his censure, and will undoubtedly be sustained by the profession.

Dr. J. Marion Sims.

the treatment of diseases incident to Parturition, such as Vesico-vaginal Fistula, Rupture of the Perinæum, &c. Being forced by the cause above stated, to exchange his southern for a northern residence, he has established himself in New-York city, where he has opened an institution for the reception and treatment of females afflicted by these diseases. It is well known to the members of the profession that, two years ago, Dr. Sims published a paper in the *American Journal of the Medical Sciences*, in which, by the employment of new instruments and modes of operating devised by himself, he claims to have achieved a large amount of success in the cure of the disgusting infirmity, Vesico-vaginal Fistula. Other cures, effected since that time, have confirmed the statements then made; so that Dr. Sims is now fairly entitled to a place in the front rank of American surgeons.

He will be pleased to receive patients labouring under these diseases, that his brethren of the South may be disposed to recommend to his professional care.

His address is, No. 79, Madison Avenue, New-York City.

Palæontology of South-Carolina.

Fossils of the Kiawah; or Palæontology of South-Carolina.—By M. TUOMEY, Prof. of Geology, University of Alabama, and F. S. HOLMES, Prof. of Geology and Palæontology, College of Charleston.

The above is the title of a work about to be published by Mr. John Russell, of this city. It will be issued in twelve or fifteen monthly numbers, of quarto form, each containing plates of figures drawn on stone, with the requisite text annexed. The plates are most beautifully and artistically executed, equal, if not superior, to any thing of the kind ever got up in this country. The first number will appear early in April. Price, per number, to subscribers, two dollars, payable on delivery.

We insert the prospectus:

"Besides the difficulty, if not impossibility, of obtaining complete sets of Conrad's valuable works, the descriptions of the Tertiary Fossils of the South are scattered through so many scientific journals of our own country, and even of Europe, that it is not without much labour and expense the student of Southern Geology can obtain access to the information that is indispensable at every step of his progress.

"Since the publication of the Report on Geology of South-Carolina, which was issued without the figures of the fossils which were to have accompanied it, a large number of interesting specimens have been collected by the subscribers from the rich and extensive deposits of this

State; and we now propose publishing full descriptions of these Fossils, illustrated by drawings on stone. The originals from which these drawings are made, have been presented to the Museum of Natural History, of the College of Charleston, to be preserved as authentic specimens for reference, and will be numbered to correspond with the figures in the plates.

"Professor Agassiz, Professor L. R. Gibbes and Dr. Leidy, have kindly volunteered their aid in describing the Fish, Crustacea and Mammalia; and we feel assured of the support of our own people, in the publication of the first work of the kind at the South."

Ranking's Half Yearly Abstract, from July to December, 1853, has just reached us, containing over three hundred pages of very valuable matter, selected from the principal medical periodicals of both continents, which have been published since the date of its last issue. (From Lindsay & Blakiston, publishers.)

Dr. Needham's Breast Pump.

Of modern inventions, designed for the relief of suffering, Dr. Needham's apparatus for drawing milk from the breast, and developing the nipple, is one of the most useful.

It is immeasurably superior, in our opinion, in efficiency to the old-fashioned pumps, etc., and possesses, at the same time, the merit of being very simple in construction. It consists of a bellows, which is fixed in a box; an India-rubber tube passing from the bellows and terminating in a metallic female screw; a glass screwed into the latter, and blown into a receptacle for the milk, at its under part, (as in the ordinary gum-elastic ball, with glass,) to which is attached a cup-shaped piece of India-rubber, which is applied to the breast over the nipple.

The degree of exhaustion is regulated by the length of time the bellows is worked; and by means of a gentle withdrawing motion of the hand which holds the glass, the process of child-suction is well imitated.

It is for sale, in this city, by Stonev & Wiltberger, druggists and

has resigned the editorship of the *Medical Reporter*, and is succeeded by Dr. S. M. Butler, who is well qualified for the duties he has assumed.

The Stethoscope has changed hands since the issue of the last number, having become the property and organ of the Medical Society of Virginia, and is edited by a committee of that body.

New Medical Journal.

American Medical Monthly is the title of a new journal, to be published monthly, as its name implies, at \$3 per annum. It contains eighty pages, and is to be conducted by the Faculty of the New-York Medical College, and to be edited by Dr. E. H. Parker, formerly editor of the *New-Hampshire Journal of Medicine*. The two numbers which have reached us contain much valuable matter, and impress us very favourably with the work, as an addition to the medical literature of our country. We wish the editors the success they deserve.

Death of Dr. Hester.

It is with feelings of the deepest regret that we record the lamented death of Dr. Hester, editor and proprietor of "*The New-Orleans Medical and Surgical Journal*." He died of epidemic cholera, on the morning of the 1st December, 1853, aged forty years. He was a man of high character, deserved reputation in the profession, and a writer of much ability and acuteness. His labours in behalf of medical science and literature have been productive of most beneficial results, and claim for him the esteem and respect of the medical profession of this country.

In 1844, in connection with Dr. Fenner, he established the *New-Orleans Medical and Surgical Journal*; and after the withdrawal of the former, in 1848, he continued to conduct it alone, with signal ability and success, until his late untimely decease.

In his death the medical profession has lost one of its ornaments, and the editorial corps an esteemed and valued member.

ERRATA.

Our readers will please note the following errata in our January issue :

In Dr. Boling's paper on the Shoulder Presentation :

On page 18, 1st line, for "the course to proceed," read "the course of procedure.

On page 19, 4th line from top, for "Actius" read "*Actius*."

On page 20, 18th line from top, place a period (.) instead of a comma (,) after "preferred;" and let the following word, "where," commence with a capital.

On page 28, 8th line from top, for "srotum" read "sacrum;" and after the word sacrum, a comma (,).

On page 28, 10th line from top, after the word "lowered" place a period (.)

In the review of Campbell on the Sympathetic Nerve,

On page 101, 13th line from top, for "its" read "it."

On page 102, 15th line from bottom, for "medice" read "medicine."

On page 103, 16th line from top, for "inoculation" read "inoculation."

On page 103, 17th line from bottom, for "splauchnic" read "splanchnic."

On page 104, 28th line from top, for "Phracio" read "Phrenic."

On page 104, 30th line from top, for "these" read "the."

In the editorial on the "People's Medical Gazette:"

On page 121, 8th line from bottom, for "possible" read "impossible."

On page 123, 6th line from top, for "*artacula*" read "*articulo*."

On page 126, 20th line from top, for "entain" read "entertain."

On page 126, 8th line from bottom, for "directed" read "dictated."

On page 127, 13th line from top, for "Pierian" read "Pierian."

TO READERS AND PUBLISHERS.

Our Foreign Correspondence has come to hand too late for insertion in this number.

CHARLESTON

MEDICAL JOURNAL AND REVIEW.

Vol. IX.]

CHARLESTON, S. C., MAY, 1854.

[No. 3.]

ART. I.—*Cases of Inflammation and Ulceration of the Cervix Uteri.*
By JAMES M. GREEN, M. D., Macon, Ga. (Continued from p. 55
of Vol. VIII.)

No. 8.—Case of intractable chronic inflammation, with ulceration, hypertrophy and induration of the Cervix, finally resolved by several severe cauterizations with Caustic Soda.

Mrs. ———, a tall, thin, delicate woman, aged 36, the mother of three children, the youngest five years old; health feebler ever since last birth, and has become very much impaired in the last year, with leucorrhœa, debility, constant headache, frequent nausea, epigastric pain, anorexia; and latterly, severe pains in the loins and hips, which extend down the thighs, and become aggravated when she remains long in the sitting posture; she also suffers a great deal from pains in the left ovarian and hypogastric regions, and a heavy pelvic bearing down that soon becomes intolerable in the erect position; her menstrual flow returns every two weeks, and she has always had intense sensitiveness of the vagina. All her disordered sensations are increased at each menstrual period. Her bowels are generally costive, and defecation is attended with an increase of the uterine and lumbar pains, and nausea, and micturition is almost always painful.

Digital examination.—Vagina intensely sensitive and painful; cervix low, indurated, and very much tumefied. *Specular*—vaginal lining vividly red, and presenting in its upper half, a coarse, shaggy appearance, with dark purple splotches; cervix dark red, and so much tumefied that the anterior lip could not enter the tube; a thin transparent mucus streaked with blood, flowing from the open os; no external ulceration, but on separating the lips, perceived a bright red ulceration,

extending up the canal. The os was not much dilated, but hard and puckered.

Treatment.—Solid nitrate of silver to the tumefied cervix, and to use laudanum and flaxseed injections every morning and evening; at the same time, to take five grs. hyd. potass. in syrup sarsap. thrice daily, with an occasional opiate enema, and to remain in the recumbent posture as much as possible. The introduction of the speculum was so painful as to throw her into a spasmodic state, that continued until evening. In a few days she was directed to substitute alum for the flaxseed injections, and, as opium in every form was found to disorder her head and stomach very much, the laudanum injections and enemata were discontinued. On account of the irritation produced by the first manipulation with the tube, Mrs. ———, although suffering greatly, declined any more efficient medication, than two or three leechings to the perineum, for two months, when her anguish became so severe that she herself demanded a more energetic treatment. On examination, the vagina was paler and not so sensitive; but no other marked change. Scarified and leeches the cervix, which seemed for several days to have had a good effect, but in a few weeks she was as bad as ever—the only beneficial change being the gradual diminution of the vaginal redness and sensibility. The cervical swelling slowly increased, and the ulcer emerged from the canal and spread out round the os. The scarification was repeated, and acid nitrate of mercury used to the ulcerated surface, followed as before by argt. nitr. every week or two, as the unmanageableness of the patient permitted. In about four months the ulceration was nearly healed, and there was some diminution of the hypertrophy, but it must be confessed there was little or no improvement in the general health, or in the local or sympathetic pains. All her symptoms, every now and then, were severely exacerbated, without any evident cause, and she again became so discouraged as resolutely to decline any further treatment. By this time, however, the intense vaginal sensibility was completely removed. This effect was due, in some degree probably, to the curative pressure of the speculum (acting as a bougie) upon the inflamed and congested membrane, as well as the action of the caustic agents. The inefficacy of the treatment, at that time, I was inclined to attribute to the obstinacy of the patient, who would not confine herself sufficiently to the recumbent posture, and continued to make improper exertions. Finding herself, however, retrograding, she sent for me again in about three weeks, when I found her labouring under an increase of most of her old symptoms, and at this time she had a nervous chill, followed by a hot fever. These attacks occurred every now and then, through-

out her whole case. The hypogastric pain, pelvic bearing down, and dysuria, were a good deal diminished in intensity. On examination, I found the cervix much paler, the external ulceration nearly healed again, and very little discharge, but the anterior lip more enlarged than at first. Two weeks afterwards, perceiving that the hypertrophy was not giving way, I applied a piece of pure caustic soda near the os, creating a pretty deep eschar, as large as a dime—having previously applied the argent. nitr. to the os and posterior lip. This cauterization produced a good deal of constitutional and uterine reaction, with an increase of the gastric disturbance, and all the usual pains, followed by a coloured discharge for six or seven days. The eschar was completely thrown off in ten days, and the ulcer healed up in three weeks. Nothing was done now in the way of treatment for over two months, except two or three applications of nitrate of silver to the internal cervical ulceration, which threatened to increase. Her health, in the meantime, fluctuated weekly from better to worse—the local indications of disease, however, began and continued steadily to improve—the leucorrhœa stopped, the cervix softened, and lessened in size so much as almost to admit the end of a large speculum. In a few weeks she was much better—the pains had almost left her—her flesh and strength were considerably increased, and she had every appearance of getting well. But, in a few weeks more, these fair prospects were blasted by a return of the old pains, debility, sallow complexion, an increased swelling and induration of the cervix, and by the breaking out of a new ulceration on one side of the os which was followed, in a few days, by another on the opposite side. The hypertrophy continued to increase, and in a short time, reached its maximum in this case, and, in fact, the greatest extent I have ever witnessed in any case. Her condition, at this time, from embarrassed urination, defecation and pelvic pressure, was very miserable. I now made another thorough application of the caustic soda, over twice as large a surface as before, followed by the same train of constitutional and local disturbance, only more severe and continuing longer. After this, she began to improve both locally and generally; and in two months from the last cauterization, health was apparently once more within her grasp, her strength and flesh increased considerably, and a good colour and cheerful countenance gave reliable token of a real change for the better. The cervix was restored almost to its natural softness, reduced in size so completely this time, as fully to enter the tube; all remains of ulceration had disappeared, and there was no leucorrhœa. The cervical canal, however, was remarkably dilated up to the internal sphincter, though apparently uninflamed and healthy. The os, for a long time back, had been hard, puckered and contracted; the

present change, though unusual, was very gratifying. On account of her abdominal thinness, it was easy to ascertain accurately that the body of the uterus was not enlarged. This flattering condition did not last longer than five or six weeks, and in two months she was *suffering* pretty nearly as much as ever, with the exception of having very little pelvic pressure, dysuria, or pain in defecation, and no leucorrhœa. The cervix again became somewhat hypertrophied, and the os closed up.

To draw to a close perhaps too long a story, I will merely observe that I found it necessary to make two more energetic cauterizations with the caustic soda, before the chronic inflammatory hardening and hypertrophy of the uterine neck, could be entirely resolved. The patient had many exacerbations of her complaints, and several times despaired of any benefit from medical treatment. Her present condition, now more than twelve months from the first cauterization, is by no means one of perfect health—for she still suffers occasionally, from pain in the left ovarian, lumbar, and left hypochondriac regions, from headache, &c., particularly just before a menstrual period; and she is delicate and unable to take much exercise. Her local disease is, however, nearly, or quite subdued; the uterus has ascended to its natural position; the cervix is quite soft and natural, with the exception of a little hardness at the point where she was last cauterized—three months ago. She has had no leucorrhœa, and the vagina has recovered its natural tonic contractility.

Remarks.—This case was, no doubt, very much protracted by the unmanageable character of the patient, and by the absolute incapability that she laboured under, of taking narcotics or stimulants in any form whatever, with the single exception of chloroform, which she frequently used internally, and by enema, and with benefit. But the most important cause, undoubtedly, was a very feeble constitution, with a marked predisposition to uterine disease—a uterine cachexia, in fact. Iodide of potassium, which I think had a markedly good effect, and carb. of Iron, were used several times during her case. Blisters to the loins, appeared to exert a good effect upon the pains.

November 24, 1853.—At this date, now nearly two years from the commencement of the treatment. I have the pleasure of stating that the

her only child, ten years ago, has had leucorrhœa, and been in delicate health—menstruation has always been natural, and she has never suffered from uterine, ovarian or lumbar pains, gastric, urinary or rectal embarrassments, or any of the usual evidences of inflammatory cervical disease. The only marked indications of disordered health in her system, has been the liability to palpitation of the heart, upon any considerable mental excitement or physical exertion, and frequent returns of general exhaustion and epigastric sinking. Her colour and general appearance are remarkably good, and by no means those of one in ill health. Two years ago, she was treated for cervical ulceration, by a Northern obstetrician, and pronounced cured. Some months, however, after her return home, the leucorrhœa made its appearance, and, after a while, the palpitation and exhaustion. On making a careful examination, both digital and specular, the vaginal canal was found to be unusually long; the cervix—anterior lip principally—enlarged and hardened, of a dark purple hue, and with marked cicatricial appearance; the os dilated, red and inflamed, and extending up the cervical tube a bright red ulceration presented itself. The ordinary treatment was then commenced, of periodical cauterizations, astringent lotions, &c., and as it proceeded, a regular and marked improvement was observed in all her symptoms, except, that at first the ulcer emerged from the interior and spread itself for some distance upon the cervix and all round the os. But after this, it gradually healed, and retired to the interior; the cervix grew smaller, paler and softer; her palpitation, debility, and epigastric sinking subsided to a great extent, and she seemed to be rapidly getting well. It was noticed in this case, that each menstrual period was preceded by an exacerbation of the palpitation and depression; and each cauterization, the day after, was followed by the same effect. At the end of about four months from the commencement of the treatment she contracted a violent cold, (cold in the head, soreness of the flesh, pains in the bones, &c.) attended by a marked increase in the palpitations and debility, and, at the same time, the anterior lip of the cervix became so much tumefied, and so completely retroverted into the *cul de sac*, that it could not be exposed without a great deal of difficulty. The ulcer also again made its appearance upon the anterior lip, and afterwards was apparently more intractable and difficult to heal than before—so much so, that I determined to try the stronger alterative influence of

condition. The palpitation and sinking subsided, and general strength was correspondingly benefitted. At the end of this period, she had a similar attack of catarrh, followed, as before, by a sudden tumefaction and turgescence of the anterior lip. When the swelling was sufficiently reduced for the os to be displayed, I perceived the ulceration recommencing round it, and immediately made another light application of the *pot. c. calce.* The ulcer, consequent upon this, healed very slowly, (and the irritability of her heart improved with still greater tardiness,) leaving the cervix pale, but hypertrophied and hard. This patient had two more severe attacks of catarrh before she finally recovered. In the first, I found it necessary to use the caustic potash on each side of the os, but in the second it exercised no beneficial effect upon the cervix. The treatment lasted nine months, leaving her quite well locally and generally, except some irritability of the heart.

Remarks.—From the fact that there were no notable signs of physical disease of the heart in this lady's case, except, perhaps, those indicating a slight degree of dilatation, (such as a greater than usual cardiac dulness and an extension of the sounds)—and that she could take some kinds of exercise—horseback and carriage—quite energetically, not only without increasing the palpitation, but with positive benefit—I was inclined to attribute the cardiac symptoms to a transmitted, or sympathetic nervous irritation, originating in, and kept up by, long continued cervical disease. That there was a certain amount of dilatation of the heart, is by no means improbable, as long continued palpitation is one of its most strongly operating causes.

No. 10.—Chronic ulceration and induration of the neck of the womb, complicated by a vascular polypus and menorrhagia.—Cure imperfect.

In the winter of 1850, Mrs. ——— placed herself under my care for long continued uterine disease. She gave the following account of her ill health: menstruated at 14; married when entering her 15th year; menstruation was more profuse than usual before marriage, became exaggerated in quantity afterwards, but was never attended with much pain; recollects always to have had some leucorrhœa since the menstrual function commenced; eight months after marriage she had an abortion, accompanied by such severe flooding as to confine her to bed for a month. From that time forth—a period of over ten years—her life has been rendered miserable by a succession of hemorrhages, more or less severe, occurring at intervals, varying from a few days to three months, but generally not absent more than a week at a time; and at the end of every three

months, very nearly, she has had a profuse flooding, throughout this protracted period. The short intervals, between these sanguineous flows, were occupied by a leucorrhoeal discharge. Her sufferings, from debility, cephalic, lumbar, ovarian, hypogastric and urethral pains, and dyspeptic embarrassment of her stomach, at length became so great as almost to banish her from society for several years. During this long time the usual routine of tonics, alteratives, styptics, counter-irritants, preventive bleedings, change of climate," &c., &c., was exhausted, without benefit; but no proper local treatment was ever attempted until recently. It is only within the last six months that she has submitted to an examination with the speculum, by an obstetrical professor, in a city north of this, who discovered an ulcer of the cervix, which he treated, by the weekly application of the nitrate of silver, and partially cured, during a two months' treatment. Of an observing mind, and having acquired a good deal of information in reference to her disease, she has been long convinced that she was the subject of polypus uteri, and urged this view upon her medical attendant; but he being unable to detect it with the speculum, said she must be mistaken. His failure to discover it, she, very correctly, attributed to its retrocession into the os, when examined in the recumbent posture; this suggested an examination in the erect position; when, projecting about a third of an inch from the open os, I perceived a small cylindrical polypus; it was very movable, and slipped round the finger or up into the os, when it was touched or attempted to be grasped between the fingers. The cervix was low, tumefied and indurated; the finger could easily pass some distance into the os, pushing the polypus before it, but the ulcerative feeling was not strongly marked. On using the speculum, next day, I found the vagina rather narrow, some leucorrhoea in the canal, and, as expected, a good deal of tumefaction of the cervix, with a superficial, but red and angry ulcer, emerging from the os and spreading out upon the surface of the rim. On raising the anterior lip, the polypus could be seen lying between the two lips, and was readily caught hold of with a long pair of forceps, and retracted after being twisted round several times. It was a vessel.

subided, leaving her, apparently, somewhat improved. The patient, after this, was subjected to the ordinary treatment of periodical cauterizations and astringent lotions; declining, however, any internal medication, having, she said, learned, from experience, that every thing of that kind increased her gastric embarrassment, and made her worse. Under this plan her health, both local and general, continued steadily to improve, and she gained eight or nine pounds in flesh; but, in the month of May, she was seized with one of her "regular three months' floodings," attended by the expulsion of tough coagula from the uterine cavity, and strong bearing-down pains. On examination, with the speculum, I found quite a profuse hemorrhage going on; the os very much dilated, and presenting several tough coagula, which were grasped firmly by the internal sphincter. At this time hardly a vestige of ulceration was left, exterior to the cervical canal. Although the local disease continued slowly, but regularly, to improve, under the influence of the above treatment, yet it was nearly two months before she entirely recovered from the effects of the flooding; and during this time she had two or three attacks of vaginitis and one slight show. After various alterations, from better to worse, on the 8th of April, I made the following note of her condition: "appetite, strength and general appearance a great deal better; little or no headache, backache, hypogastric or ovarian pains; no pelvic bearing down; vaginitis gone; cervix paler and soft; no remains of ulceration, or so small that the caustic stick could not enter, and nearly natural in appearance." In six weeks afterwards, her condition was as follows: "After being, to all appearance, well, and having passed through a *natural* menstrual period, ten days ago, and preparing to return to her family, in the county of ———, Mrs. ——— was seized with a profuse and exhausting flooding, preceded and accompanied by severe headache, and nausea, and some backache, but no expulsive pain, as usual. Os open; cervix tumefied and congested; but no appearance of ulceration."

Remarks.—What was the cause of this unexpected hemorrhage? was there another polypus? To ascertain this I carefully explored the cervical canal, with the index finger, as far as possible, and with a ringed sound to the internal stricture, but could detect nothing. Was it internal metritis? This supposition would seem to be contradicted by the absence of any uterine pain, or tenderness; by the curative process, that had been going on in the organ for some time; by the previously natural and closed state of the os; by the absence, for a considerable period, of sanguinolent discharges; and by the closed condition of

the internal constriction. Or was it the result of a long continued hemorrhagic habit, or menorrhagia? The latter seemed to me, in view of all these considerations and of the profuseness of the discharge, the most probable diagnosis. There was no perceptible enlargement of the body of the uterus, to justify a suspicion of fibrous polypus, either in the substance or cavity of that organ.

I had an opportunity of examining this patient carefully about six months after the preceding observations were written, and found no evidence of local disease, except slight remains of hypertrophy and induration of the anterior lip; the cervix was soft, smooth and natural. But in general health and liability to flooding she remained the same. I saw this lady again in 1853—health not in the slightest degree improved.

No. 11. Case of inflammation, ulceration and tumefaction of the Cervix Uteri.

This patient, in humble life and accustomed to hard labour, commenced menstruating, according to her account, at the late period of 19 years of age. She has always been regular, the menstrual flow lasting from four to five days—and says that she has had whites “off and on” all her life, though her health has been, generally, excellent. She is the mother of four children, and since the last (now nearly two years old) has had an increase of the white and yellow vaginal discharges, accompanied by lumbar pains, hypogastric pressure, &c., &c., all very much aggravated during the menstrual flux, which was greater in quantity, and occupied a longer time, than formerly. She also has a good deal of nausea, which is increased during defecation, and accompanied by uterine pain. On examination, found the cervix greatly increased in size, considerably indurated, with a large, coarsely granular, ulcer encircling the enlarged os and passing into the canal. There was a good deal of muco-purulent matter in the vaginal tube.

Without entering into a tedious detail of treatment, it is only necessary to state, that in about three months the patient had realized a large amount of improvement, both in her general health and in the local symptoms; but there still remained a certain degree of swelling and induration of the cervix, with a little ulceration immediately within

of three or four cauterizations, with caustic potash, at intervals of three or four weeks, with applications of nitrate of silver between those times. This, when its effects completely subsided, completed the cure of the cervical inflammation; leaving it (the cervix) in a perfectly healthy state, except a strong adhesive band connecting the opposite sides of the os, in the centre, and thus forming a double mouth to the canal. This connecting band was divided with a bistoury, and separated several times with a wooden spatula, and eventually healed; the os being of a natural shape and appearance, though large. The only remaining symptom of the disease, that now gave any annoyance, was the pain in the loins; and this was finally dissipated by an energetic blister, which was followed by several large boils in the same region.

The patient, ultimately, recovered her rude, vigorous health, though she continued six months under treatment.

No. 12.—Case of inflammation and ulceration of the Cervix in a patient who became pregnant while under treatment—the patient getting well, and going on to labour with an occluded Os.

February 20th, 1852: Amanda ———, a delicate negress, accustomed to sedentary occupations; mother of two children—the youngest 14 months old; has, since the last labour, been in declining health, with profuse leucorrhœa, excessive pain in the back, pelvic bearing down, &c. The menses, since their return, have been disordered—sometimes profuse, at other times scanty, and always attended by an exaggeration of the other symptoms. She has had a great deal of nausea and headache; her general health is poor; and she is feeble and emaciated. *Touch.*—Cervix very small; little or no hypertrophy; ulcerative feeling at the end of cervix. *Speculum.*—White granular discharge in the vagina; cervix and os very small, but covered at the end by an irritable looking ulcer, which extended into the very short canal. The whole uterine organ seemed to be on a very small scale. There was no induration of structure, except immediately under the base of the ulcer, and the rest of the cervix was covered by its natural unctuous secretion.

The usual treatment was regularly pursued in this case, (periodical cauterizations with nitrate of silver—substituting acid nitrate of mercury when the healing stopped) but the cure proceeded slowly though

apparent, and the patient, after suffering a great deal from the usual irritation of that condition, was taken to bed on the evening of January 15th, 1853; thus proving that her pregnancy had commenced about the middle of the treatment, and while undergoing the weekly application of caustics to the cervix. Several times, during the summer, I examined her with the speculum, and each time the os was firmly adherent and closed by a white cicatricial line. The midwife, who was called to attend her labour, finding that no progress was making, and that the pains were entirely abortive, requested my assistance; I found her in this condition, and after a vain effort to use the speculum, succeeded, with considerable difficulty, in finding the cervix in the upper and posterior part of the vagina, and then, with the aid of the index and middle finger, by continuous and persevering effort, separated the adherent sides of the canal up to the bag of membranes. The os soon dilated to the size of a 25c. piece; the edges growing thin upon the presenting head. The case having now assumed an ordinary character, I left her in the hands of the midwife; and she went on naturally to the conclusion of her labour, and was delivered of a healthy living child.

No. 13.—Case of severe ulceration in a pregnant woman, threatened with abortion. Abortion prevented, and the ulceration cured, with occluded Os.

At 11 o'clock, P. M., July 10th, 1852, I was called to see Mildred, a mulatto woman, aged 20, who was strongly threatened with abortion—having frequent severe uterine pains, but no sanguineous or mucous discharges. She supposed herself to be about six months pregnant. By the *touch*, found the vagina sensitive, very long, and not relaxed; the cervix soft and somewhat enlarged; os and cervical canal considerably dilated, and the seat of a soft coarsely granulous ulcer.

I directed the patient to eat ice, and to use opiate enemata at proper intervals.

11th.—Pains not so severe, but slept none last night. Upon examining with the speculum, to-day, the parts were red and tender, while in the upper part of the urethra, about an inch behind the urethral orifice, there was a large soft purple ecchymosed tumour, or excrescence, which fell into the end of the instrument and obstructed its passage. By persevering and careful manipulation, however, it was flattened out before the tube as it passed along to the cervix. This body was dark, red, soft, equally enlarged all round the rim. Immediately round the os, and penetrating a considerable distance into the canal, was an ulcer, which did not present the dark, fungous and luxu-

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riant appearance, met with in females advanced in pregnancy. Applied nitrate of silver energetically to the whole surface, and as far into the canal as it would penetrate.

12th.—No pain was experienced from the caustic; has a little sanguinolent discharge; uterine pains better; says they are frequently excited by the tumultuous movements of the child. By the 15th, the uterine pains were gone, and the patient seemed safe from all immediate danger of abortion, and the ulcer was rapidly improving. Applied nitrate of silver to the ulcer and the ecchymosed sides of the vaginal tumour.

20th.—Patient getting on well; ulcer nearly well; no vaginal discharges; tumour same, but patient says it sometimes swells so as to project from the external parts. All symptoms of inflammatory irritation of the cervix, soon after this, disappeared, and the patient went on uninterruptedly to full term. She was confined, under the management of a midwife, on the 23rd of September, who, perceiving that there was "something wrong" upon examination, and that although the pains were strong and frequent, the os did not dilate, or the labour otherwise advance, requested my assistance about 1 o'clock, A. M. Placing the patient in a favourable position, opposite a bright light, and introducing the speculum, the os was seen firmly closed by a dense thick tendinous membrane of a bluish appearance. On slitting this open, with a lancet blade attached to a handle of proper length, a considerable quantity of thick, dark, tenacious mucus was suddenly ejected into the tube. The os then rapidly dilating, the bag of membranes was as suddenly and rapidly projected, for a couple of inches, into the instrument, and after remaining in that state for a few moments, burst, with great force, scattering the amniotic fluid, mixed with meconium, upon all objects that happened to be stationed in front. The speculum then being withdrawn, and a per vaginam examination made, the os was discovered to be largely dilated and the labour proceeded naturally. She was accordingly left in charge of the midwife, who delivered her of a dead child (though full grown and apparently healthy) in about an hour after the operation. She had not perceived the foetal motions for several days, and this, with the presence of meconium in the amniotic discharge, rendered its death almost certain.

Remarks.—The ulceration in this case, healed with extraordinary

contents, the immediate and marked result was calming and sedative. This, however, should not be surprising, as it is a strictly analogous result to that obtained by the application of the same agent to granulous conjunctivæ, to ulcerated corneæ, &c., which have long been familiar to the profession.

No. 14.—Case of long continued inflammation and ulceration of the Cervix, finally cured in the pregnant state, with occluded Os.

A lady aged about 34, the mother of four children, who had been suffering for five or six years with the usual group of symptoms, that indicate inflammation and ulceration of the cervix, requested my attendance, January, —. Her sufferings from pain in the back, almost constant headache, and from pelvic bearing down, had, I learned, been unusually severe—so great, indeed, as to confine her to the house, a greater part of the time, for several years. Walking or standing in the erect position, for more than a few minutes at a time, produced such an aggravation of the lumbar pains and bearing down, that she avoided both, as much as possible. Singular to relate, her symptoms were not increased in severity during menstruation, which was regular (except when pregnant or nursing) and natural in time and quantity, and commonly followed by some feelings of relief. Her cervix was considerably enlarged and indurated; the os dilated, and surrounded by a good-sized ulcer, and this, by a large circle of inflammatory redness, streaked by a number of purple and congested ulcers. A good deal of thick, yellow discharge, was discovered in the upper part of the vaginal canal, by the instrumental examination. It is almost needless to mention that, under this condition of things, her general health was very bad, and her life one of suffering and discomfort. Her constitution was a nervous and irritable one, and would not admit of the use of opium and other remedies, that otherwise would have ameliorated her condition.

Treatment.—This lady was subjected, for several months, to the ordinary local treatment of cauterization, (two leechings,) and astringent and detergent lotions, with considerable, but by no means regular or completely beneficial effect. Several times the cervix was reduced nearly to a natural state, and the os to a natural size, there remaining only a point of inflammatory redness. But often, the latter would spread into a large red blush, accompanied by hypertrophy, granulation, and corresponding increase in the other symptoms. At no time, however, was she clear, for more than a few hours at a time, of pain in the lumbar or uterine regions. At the end of five months, not receiving the amount of relief expected, and perceiving that she was again preg-

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nant, she resolved to suspend treatment until after her confinement. Her condition was distressing during the whole of that period, having all the symptoms except the vaginal discharges, which ceased entirely, during the latter months. On being called to attend her accouchment, which occurred in the December following, I found the cervix soft and natural; quite free from all vestige of ulceration, but perfectly adherent and occluded. The adhesions were separated without much difficulty, by manipulating for a few minutes with the index and middle finger, and the labour proceeded afterwards naturally—though far more prolonged than was usual with her. She was delivered of a large, healthy infant. Her getting up was remarkably rapid and perfect, compared with her former accouchments, and she is now in the enjoyment of excellent general and local health.

No. 15.—Case of occluded Os in a non-pregnant female, who had been cured of ulcerated cervix.

A young married woman was treated by me and cured of ulcerated cervix, in the spring of 1852. Towards the end of the treatment, (which did not last longer than two months,) I noticed a tendency to closure of the canal some way up from the mouth, and twice introduced a sound to prevent it. I also advised its use every three or four days, for several weeks; but feeling herself in the enjoyment of very good health, and not suffering at all, it was found difficult to enforce the recommendation, and finally, it was abandoned. On the return of the menstrual secretion, eight months after confinement, and two months after the cure of the ulceration, she was seized with agonizing pains in the uterine, lumbar and femoral regions, with pelvic bearing down, &c., all the symptoms that mark prevented menstruation. Upon using the speculum, a band of adhesion was perceived firmly uniting the opposite sides of the canal, about one-third of an inch from the os. After penetrating this strong diaphragm with a triangular probe, and then dilating the canal with a wooden sound, a copious discharge of menstrual blood

learned that she had had two abortions previous to this pregnancy ; had suffered much with pain in the back, pelvic pressure, dysuria, leucorrhœa, &c. From there being no urgent necessity for interference, and from the adhesion not having occurred in connection with treatment, and from some other particulars, I did not make up my mind to rupture the adhesions, until the second night of labour pains. But on the first examination afterwards, I found that nature had already performed that office, and the head, covered by the membranes, was presenting. Labour went on naturally, and the patient was soon delivered.

The last five cases present a group, having one feature in common, that of occlusion of the mouth of the womb. When I first became acquainted with this result of ulcerative disease of the cervix, I supposed it might be attributed to a small loss of substance, produced by the application of caustics ; but, having noticed its occurrence in some cases in which the nitrate of silver alone was used, and in others in which there had been no treatment at all, I am now inclined to think that it may take place in any non-menstruating female, where the disease is cured, either spontaneously or by treatment (see Nos. 16 and 20). Where the menstrual flow occurs once a month, with its accompanying secretions, it, of course, prevents adhesion by the mechanical effect of the discharge itself, and by the vital dilatation of the canal, which happens at the same time. In regard to the effect of caustics, it is probable that the action of nitrate of silver, and of acid nitrate of mercury—as applied to the surface—is solely of an alterative, constringing and curative nature.

No. 17.—Case in which a small ulceration produced severe symptoms in a consumptive female. Great improvement in health, and pregnancy occurring after the cure of the disease.

In the spring of 1852, I was called to attend, professionally, a young married woman, who was a member of a family remarkably liable to consumption, and who had been in declining health for several years, from this disease. I found her labouring under unequivocal symptoms of tuberculosis, but more advanced in the left lung, in the upper lobe of which there was a considerable cavity. She also was very much emaciated, and had hectic and great debility. Since her marriage—some few months previous to this date—she had become subject to pain in the back, and both iliac regions, with pelvic bearing down. She has had occasional leucorrhœa for several years, which lately has increased, and after her monthly periods, is often mixed with a thick yellow discharge,

and streaked with blood. By the touch and speculum, the cervix was found very small and hard, particularly in the centre; the os small, and discharging a little sanguineous muco-pus. The whole uterine body was small and moveable.

Treatment.—Cauterized with nitrate of silver, and directed her to use alum vaginal injections, and to take cod-liver oil, regularly. By a continuance of this treatment, occasionally substituting the acid nitrate of mercury, for two months, a marked and beneficial change was produced in her health—a considerable increase of flesh and strength took place—to such an extent, indeed, that she was enabled to take out-door pedestrian exercise, and her friends anticipated a complete recovery; and the ulcer, with its attendant lumbar, iliac and hypogastric pains, was entirely removed.

Remarks.—No doubt that much of the general improvement, in this case, was produced by the alterative agency of that admirable medicine, cod-liver oil, but fully as much of it must have been owing to the removal of the uterine inflammation, with its reflex irritations and pains. Shortly after I discharged her from treatment she was delivered (in due time) of a small, but apparently healthy infant, without having experienced any further indications of uterine disease. So completely had the powers of life, however, been exhausted by the disease of the lungs, that she never rallied from the shock of parturition, though the struggle was protracted for three months. During the last two months of her pregnancy, she suffered a great deal of inconvenience from granulous or tuberculous inflammation of the pharynx, hanging palate, and uvula, which were all covered by large granulations, and constantly discharging purulent matter.

No. 18.—Case of recurring inflammation and ulceration of the Cervix, with tumour in the right side.

Phillis, a delicate negro woman, aged 38, had suffered for many years from general bad health, with most of the symptoms of inflammation and ulceration of the cervix uteri. She had also been troubled with a very perceptible, but deep-seated and ill-defined tumour, in the inguinal region. When examined, the vagina was remarkably narrow and contracted—the cervix was not ulcerated externally, but had a bright red inflamed appearance, with lobular enlargements all round the rim. On examination, a few weeks after, the end of the cervix was covered with a bright, red, granulous ulcer—all the attendant symptoms and irritations being, at the same time, very much aggravated. This patient continued irregularly under treatment, for several months, and during that time

had a number of these exacerbations and retrocessions of inflammatory action of the cervix. There was no regular periodicity observed in these attacks, nor any marked connection with the menstrual flux, or with the cauterizing applications; but they seemed to proceed solely from the pathological reaction of the part itself. In regard to the treatment, the improvement, though slow and irregular, was very satisfactory; and finally, resulted in a restoration to a very fair degree of health—far better than she had enjoyed for years.

No. 19.—Case of severe inflammation and ulceration of the Cervix Uteri, with Urethritis, in a pregnant female. Pain in the right iliac and lumbar regions, so great as to simulate the passage of a Nephritic Calculus.

In May, 1853, I was requested to see Mrs. ———, a lady aged 25, who generally enjoyed fine health, and was then nearly seven months advanced in her fourth pregnancy. She had been suffering for several days from severe pain in the right lumbar and iliac regions, extending down to the neck of the bladder, with a constant tendency to urinate, which was also attended by acute pain. The urinary secretion was heavily loaded with lateritious sediment and thick mucus. The lumbar and iliac pain was liable, every now and then, to sudden aggravation, very much resembling the paroxysms witnessed during the passage of a calculus through the ureter. The probability of the attack being of this nature, was strongly impressed upon the patient's mind, from the fact that several members of her family had been troubled with this complaint. I learned that the patient had had leucorrhœa during the whole of her pregnancy, and this had recently increased, becoming more thick and yellow, and, occasionally, mixed with blood; and that she had noticed a similar discharge, for the first time in her life, during the latter period of her previous pregnancy. The pains in her right loin and iliac region, were quite recent, but the urinary irritation had approached more gradually. For the purpose of relieving the pains in her side, she had been using mustard poultices applied to that region, and with some benefit. At first, declining a more active treatment, she was kept under the influence of laudanum enemata, for eight or ten days, and with marked relief; but as soon as the opiate effect wore off, the pains returned with the same severity, and her condition was rapidly getting worse. Becoming aware of this herself, she consented to the necessary instrumental investigations. On examining per vaginam, the cervix was found soft and enlarged, and the os and cervical canal, which was so much dilated as to admit the finger for a considerable distance, was evi-

dently ulcerated, while the urethra was swollen and tender to the touch. Much pain and difficulty was experienced in using the speculum, from the greatly enlarged, hardened and irritable state of the urethra, which projected forward like a solid body in the way of the instrument. By careful manipulation, however, it was made to pass on to the cervix, which was found to be, as expected, enlarged and purple; the os and canal were occupied by a peculiarly livid, coarsely granulous, and angry looking ulcer. I found that the canal, in this case, was opened and dilated by the ulcerative process, to a greater extent (at least an inch) than I had previously met with in a pregnant female, and it was altogether one of the worst looking and threatening cases that had come under my observation. The slightest increase or extension upwards, must have brought on premature labour with all its attendant dangers.

Treatment.—The opportunity presented by the first examination, was seized for making a thorough application of nitrate of silver to the whole ulcerated surface, as far as it extended up the canal. The patient was then directed to keep strictly in the recumbent posture, and to use, methodically, the alum water and cold water injections. The cauterizations (after one leeching) were continued regularly, and the result of the whole treatment was a rapid and steady improvement in the ulcer, with all its attendant irritations. In four or five weeks it was almost healed, and the cervical canal nearly reduced to its natural size. The inflammation, thickening and irritation about the urethra, however, gave way more slowly, the patient still being troubled with dysuria; but, at length, it was completely removed. From accidental circumstances, I was obliged to discontinue treatment before the cervical disease was entirely cured, but enough had been done to prevent any immediate danger of miscarriage, and to enable her to go on safely to full term, which she did. In this case, not the slightest pain was felt at any time from the application of caustics.

No. 20.—*Case of extensive inflammation and ulceration of the Cervix Uteri, with intense sensitiveness of the parts and great constitutional disturbance, cured with occlusion.*

A lady aged forty-five, the mother of a very large family, the youngest of whom is now about three years old, who had usually enjoyed excellent health, has, for the last four or five years, been an invalid from

she has grown worse; the headache is now constant and distressing, sometimes agonising—she compares it to the pressure of an iron band round the head. The nausea is almost as constant, and very distressing; vomiting being frequently produced by ordinary sights, sounds and smells, that have become disagreeable to her diseased perceptions, and the leucorrhœa has become a profuse, yellow and frequently offensive discharge. (It is proper to mention here, that she has had no regular return of the menstrual flow since her confinement, and supposes that it has ceased.) Her backache is worse, and she has a very deep seated and distressing pain beneath the lower part of the sternum, and she is excessively nervous and restless. Until within the last year or two, a person of fine contour, clear florid complexion, and vigorous expression of countenance: her emaciated form, sunken eyes, projecting cheek-bones, sallow skin, told expressively of the destructive inroads that disease was making upon her constitution.

Upon making the usual examinations, the cervix was found to be retroverted, and lying on the back part of the vagina, hypertrophied to a very great extent, and remarkably hard, with a number of tubercular projections round the os. The cervix was nearly covered by an intensely red and irritable looking ulceration, extending interiorly into the cervical canal, and there was a copious discharge of very offensive purulent matter in the vagina. From the remarkable degree to which the induration had gone from the tubercles round the os, (which were not to be confounded with projections left by ulcerated fissures,) and from the offensive odour of the discharge, I was disposed to apprehend malignant disease, but concluded to await the result of a few weeks' treatment before giving that diagnosis. Not an ordinary symptom in this case, was the intense sensitiveness and irritability of the ulcerated surface, so great that the caustic could only be borne upon a small portion of it at a time—the slightest touch producing a nervous quivering over her whole system. Nor could, at first, she bear at all, the use of the alum solution. As the treatment progressed, however, the irritability of the external ulceration began to subside, and finally was confined to the interior of the canal—the ulceration gradually healed and retreated to the same place, the hypertrophy and retroversion diminished, and the induration softened. Although many months elapsed before the patient was finally

the head and stomach fade away with such regularity and certainty, before an entirely local treatment, applied to the other extremity of the body. One of the first and most agreeable results, was the removal of the offensive character of the discharge, though this was probably due as much to the institution of an efficient system of vaginal injections as to the caustics. The profuse discharges which were formerly retained so long in the vagina by the enlarged and retroverted cervix as to become putrid, were now washed away with daily regularity and certainty, and of course with a beneficial effect upon the health and comfort of the patient.

Mrs. ——— finally recovered her health completely, and upon a recent examination, I found no remains of disease, except a closed and adherent os—the cervix being healed over perfectly smooth, and restored to its natural position. As she was passed the period of menstrual life, and seemed to suffer no inconvenience from the closure, I did not think proper to make any attempt to restore the passage.

No. 21.—Case of inflammation and ulceration of the Cervix, with Cardiac disturbance and Hæmoptysis.

April 1st, 1852.—A widow lady aged twenty-five, the mother of one child, has been troubled, more or less, ever since its birth, with pain in the back, of a distressing character, which is increased to an acute degree whenever she is in the erect posture. She also has pelvic weight, pain in the hypogastric and right ovarian region—a sense of sinking about the stomach, and occasionally great debility. In addition to these, she has had for a long time, profuse white and yellow vaginal discharges. Menstruation has been regular as to time and quantity. Her own attention has been more attracted by the cardiac symptoms in her case, than by any other (having had frequent attacks of palpitation, with “sinking and stopping” feelings about the heart). For the relief of these attacks, she has undergone a variety of treatment—has been ptyalised, and taken precipitated carb. of iron, with benefit. On June 4th, after the preceding date, I made the following note in her case: “Menstrual discharge came on last night, preceded by most distressing pains and fulness about the head and heart.” On the 7th, the following: had a smart hemorrhage from the lungs to-day, preceded by a “stoppage and bad feelings about the heart,” to use the patient’s own words. This was the second time she had had bleeding from the lungs. When the patient’s health was sufficiently recovered to undergo instrumental examination, I found the anterior lip of the cervix considerably

enlarged and indurated, with an ulceration penetrating into the canal; the vagina lax for an unmarried female, and containing a good deal of muco-purulent discharge.

This patient, under the usual local treatment, along with blisters and the use of cod-liver oil, was in a few months restored to better health than she had had for years—the uterine symptoms soon giving way, and there being no severe return of the cardiac disturbance. She now, January, 1852, appears to be in excellent health, except the occasional sudden rupture of a small abscess in the lung; but this is, however, accompanied by no other disturbance than a slight fulness in the left chest, followed by a purulent discharge for a day or two.

Remarks.—Although this lady suffered so much from cardiac disturbance, yet, on careful examination, there was no real evidence of organic disease; there may have been a certain amount of dilatation. It is probable that the process of tuberculisation had commenced in the left lung, though there was no well marked indication of it at that time.

No. 22.—Severe case of internal inflammation and ulceration of the Cervix, yielding rapidly to treatment.

July 26th, 1852.—Rainy, a negro woman, aged 48, has had twenty-one births, including some abortions and miscarriages; when not pregnant, which is very seldom, her menstrual function has been regular; her youngest child is only fourteen months old. One month ago she was attacked by profuse uterine hemorrhage, (perhaps an abortion,) and has had a discharge every day since, which is some times pale, and at other times black and fetid. In the intervals between these lapses she has no discharge of a leucorrhœal character, but, to use her own language, is “very dry.” She is very low, emaciated and feeble, and appears to be in the last stage of marasmus, and is confined to her bed all the time. This is owing, in some degree, to a well-marked chronic gastritis, indicated by continued pain and tenderness at the epigastrium, with anorexia, etc., etc.

Upon making a digital examination, the vagina was intensely sensitive and hot, and the cervix was so much retroverted as only to be reached with great difficulty—the fundus of the womb being so much turned forward as to constitute a decided anteversion. From this necessarily imperfect investigation, I judged the anterior lip to be enlarged and indurated. At the first instrumental examination, which was very difficult and painful, the cervix was so completely and strongly turned backwards that the os could not be displayed by the most careful manipulation—vaginal lining very red.

Treatment.—Ordered blister to the epigastrium, half a gr. morphine every night, and the methodical and efficient use of weak injections of alum water.

Note, August 3d. "Improved; vaginal membrane paler; after manipulation for some time, succeeded in bringing the cervix fairly into view; anterior lip resembles a large walnut in shape and size; the posterior spread out upon this like a membrane; soft, flexible, and not at all inflamed. By separating this thin posterior lip from the anterior with a proper instrument, perceived that the posterior surface of the latter was covered by an angry ulceration, which made its appearance at the anterior edge of the os. Made a vigorous application of acid nitrate of mercury."

Note, August 18th. "Wonderfully better in health, strength and flesh; sitting up sewing; good appetite; no pains, but a little back-ache; some muco-purulent flow, but not offensive; ulcer rapidly healing."

Remarks.—Considering the low state to which she had been reduced, this patient recovered her health in a surprisingly short space of time; an improved appearance, greater strength, and better nutrition, rapidly following the restorative changes in the local disease. I do not think more than six cauterizations were used in the case. She has continued well.

No. 23.—Case of inflammation and ulceration of the Cervix: very few marked symptoms.

July, 1852.—A lady, aged twenty-six, who commenced menstruating at thirteen, and always did so naturally afterwards, except having a rending backache for the first day; married at twenty, and has had four children, the youngest of whom is now eleven months old; has, for the last five or six months, noticed that she has been obliged to urinate with marked and increasing frequency, and that she has had a white leucorrhoeal discharge, which has lately changed into a thick, bright-yellow, muco-purulent matter, with occasional pains in the right ovarian region, some times slight, at others severe, but generally evanescent; has had no hypogastric pain, and but slight feelings of pelvic bearing down—the only lumbar pains experienced have been the next morning after intercourse, when pain and debility in this region was always felt. She has had only two menstrual periods since the birth of her last child, and these have been prolonged, the first to seven days, and the latter to nine; her natural time being only five. She also complains of a feeling of relaxation and debility in the vaginal canal.

Digital examination.—Cervix rather low, and somewhat tumefied; the anterior lip more so; the posterior divided into three lobes; the os dilated, with a distinct ulcerated feeling, principally on the anterior lip. These observations were verified by the speculum, which displayed a considerable ulceration round the os, greater on the anterior lip; the posterior being divided into lobes by three deep ulcerated sulci. These fissures or sulci were, to the eye, filled up to the level of the rim by soft granulations. The whole ulcer had a varicose appearance, and I judged it to extend about half way up the cervical canal.

The ulcer was entirely cured, and the lady recovered her usual vigorous health in five or six weeks, and afterwards became pregnant, and was delivered, healthily and naturally, at full term. She then fell into ill health, from other causes, and, in a subsequent pregnancy, was attacked with ulceration, and aborted at three months.

ART. II.—*The Cranial Sutures; their true physiological significance. An Inaugural Dissertation, which Preceived the remium of the Medical College, State of South-Carolina, March, 1853.* By J. L. MCMILLAN, M D., of Mississippi.

IN the development of the bony skeleton of vertebrated animals, it is observed, that the osteogenesis of some portions is established, while in others it is in progress, and in others still there is but the cartilaginous condition. Among the bones in which the process of hardening is retarded, those of the head are to be classified. As the several bones composing the cranium partake, to a greater or less degree, in this dilatoriness, they will be found proportionally separated, interstices, more or less perfect, intervening, permitting among the respective pieces a change of relative position. These interstices or sutures, unlike other local peculiarities, can be traced throughout the class of vertebrated animals.

While in minds of limited capacity, to discover the causes effecting these peculiarities of structure is impracticable, it is yet possible to trace, so to speak, the anticipatory influence and dependence of function upon structure—the true basis and significance of conformity. It is thus that a knowledge of the design in homologies is to be attained; and in this relation of function and of structure, alone, will be found the principle of design, and, if not the cause of the homology, still the high necessity for such conformity. It is such views as these that reveal to us occasional apprehension of the mind of Deity; they are the seal and

shadow of a Great First Cause, the characters in the great book of nature.

If, as I maintain, such a necessity can be demonstrated concerning the use of the sutures, what is its true physiological significance?

Considering the difficulties of parturition, and, noticing the overlapping of these bones incident thereto, together with the anæsthesia thereby produced, so favourable not only to the expedition of labour, but to the safety of the maternal soft parts, some have suggested these as requiring this physical condition. This was the theory of the older anatomists, and the one most likely to obtain while anatomy was yet so imperfect as to institute no comparison. But when, owing to the developments of scientific research, it was seen, that in oviparous as well as in viviparous animals this peculiarity existed, the insufficiency of this reason was made evident. For this cause, alone, this theory is not tenable; it is of too limited application.

It was supposed by others, that this arrangement was intended to permit a more uniform development to the tender brain, than could take place were the cranium originally a solid bony case. It is evident the sutures do not interfere with this symmetrical evolution of the brain, yet that the opposite condition would, is by no means obvious. It is inconceivable why it should obstruct the proper advancement of one portion, and not equally affect that of another. If the continuity of the vault would be at all injurious, the injury would be general.

Morton, noticing this question, says, "the true use of the sutures undoubtedly is to subserve the growth of bones, which is accomplished by osseous deposition at their margins; hence a suture between two cranial bones is equivalent to the surface that intervenes between the shaft and epiphysis of a long bone." He further states that, as the periosteum covers not only the two surfaces of the cranial bones, but dips between them, at their margins, we have here an example of intra-membranous ossification. This doctrine, expressed with so much confidence, is at first view very plausible, but when duly considered, it is seen to be purely a sophism of "*non causa pro causa*." It is asserting how, not why, they are produced. It explains the existence of the sutures, not the necessity for their existence. Why was not the cranium developed as a single piece? or what is the significance of the division? This is the question to be met, and this Morton has not considered.

and having, as is the case with the spinal vertebræ, their separate points of ossification. It is maintained that the sutures are an absolute contingency of this method; that without their existence, such a development could not result. This opinion is highly ingenious, and, as regards the mere existence of the sutures, perfectly satisfactory. So far as the explanation goes, it must be conclusive; but it does not go far enough. It is the truth, not the whole truth. Like the last opinion, it explains the existence of the sutures as the result of the manner of the growth of bones, but leaves unconsidered the necessity for their existence, arising from the condition of the inclosed brain. It may be said that the unity and simplicity of the operations of nature, require that the analogy of development should be carried out. Here, then, is a true necessity, and as such I admit its importance; yet, considering the cranium in its relations to the brain, and the pre-eminent importance of the latter organ, I maintain that there is a necessity of paramount significance, still unexplained, founded in the very character of the structure and functions of this organ.

With a preliminary view of the principles involved in this discussion, I shall endeavour to demonstrate the character of this necessity. I should state, however, *in limine*, that while I deny the sufficiency of any or of all of these explanations, still I cheerfully accord them the importance they deserve. They are evidently manifestations of the design of the existence of the sutures, yet subsidiary in their character, and, as such, beautiful illustrations of the economy of creation.

In the history of man, beginning with the fœtus in utero, there are observed two modes of life characterized accordingly. Organic development is the one, active animal existence the other. And though traces of the latter may be noticed in the condition of the former, yet the higher attributes are pre-eminently manifested after birth; and as previously the organs are immature, their active exercise may be considered as characteristic of the latter condition. Carpenter, considering this question, says, "for the life of the embryo is one rather of organic or vegetative, than of animal activity; the action of the heart, and the occasional reflex movements of the limbs, being its only manifestations of nervo-muscular power." (Prin. Hum. Phys. p. 149.) Nutrition and development characterize the one, maturity and activity the other.

In these conditions, there is required, respectively, the presence of appropriate innervation. Before birth, this innervation has reference specially to nutrition and development, while, in after life, this is also observed, thought and voluntary motion being superadded. According with these necessities, the division of the nervous system, as made by

Ehrenberg, (Bichett,) into that of animal and that of vegetative life, has been generally received ; the functions of the former being referred to the cerebro-spinal, those of the latter to the ganglionic centres. The brain is considered the seat of volition and of thought, while the ganglionic has nothing to do with sensation or muscular action, but presides over the functions of nutrition, secretion, &c., performed by the capillaries. (Müller.) This difference of function pre-supposes a difference of character. Accordingly, during the earlier periods of embryonic life, these systems can be distinctly traced, springing from different layers of the embryonic membrane ; that of animal life from the serous or upper layer, that of vegetative life, from the mucous or lower ; showing, thereby, an essential difference of structure ; and in this may consist the cause of the difference now to be noticed. Agreeing with the character of their functions, these systems are subject to different laws of action and repose ; if, indeed, the ganglionic is ever entirely at rest. It is obvious that the action of the one, regulating the functions of organic life, must co-exist with the exercise of the functions themselves, in one or other of its many departments ; though the condition of the system, at various times, may afford periods of partial repose. "The organic system," says Müller, "has its periods of remission and rest, but these are not coincident with the sleep of animal life, and are very different for different organs." While, however, this system is never wholly idle, there are times when the condition of the other imposes upon it the necessity to repose. And though much has been written to prove the mind incapable of perfect quiescence, there are seasons when there is probably an entire cessation of the exercise of its several faculties. This state of repose has been termed sleep, concerning the condition and cause of which many opinions have been held, the clearest and most comprehensive of which is the one given by Dr. Dickson in his late essays. It is of all the most consistent with observed facts, and will, I conceive, account satisfactorily for any of the different characters of sleep. And as, according to the Newtonian principle, more causes than are true or sufficient are not to be assigned, this theory should be received, at all events, until its insufficiency be noticed. As this condition of repose is involved in the discussion of this question, I will state briefly the theory, with the arguments establishing it.

that compression produced by congestion of the blood in the veins and sinuses of the head, is claimed only in healthy sleep; compression of any kind will produce a species of sleep. Haller believed that the production of sleep was due to the congestion of the blood or other fluids in the cranium compressing the brain. Others have also maintained this opinion, while there have been, at different times, advocates of relaxation of tissue. None, however, have combined the elements as is done by Dr. Dickson. These causes intimately involve each other; relaxation not only favouring, but demanding congestion, and congestion, probably, promoting relaxation.

There are many arguments, and much of careful observation, in favour of this theory. That the brain, independently of the movements caused by the circulation and respiration, is subject to a condition of alternate erethism and collapse, according with the states of action and repose, is a matter of repeated observation. This erethism and collapse is noticed to vary at all times in accordance with the degree of mental activity; and so uniform are these appearances, that they have been rightly assumed as the conditions of activity and repose. The shrinking of the brain upon itself, during inactivity, is held to be due, as before stated, to a true relaxation. There is but one other method of explanation, and this by no means plausible. If it be supposed that the collapse is due to the retiring of the blood from the cavity of the cranium, there will be the vacancy caused by the withdrawal to be occupied. This can only be done by one of two agents: the cerebro-spinal fluid, or by the blood itself. But, as the afflux of blood would abrogate the very terms of the hypothesis, it is of necessity excluded. If, then, the substitute is to be found in the cerebro-spinal fluid, there will be a necessity of time, in which the requisite quantity may be absorbed and deposited there; a like degree of time being necessary for its removal. We should, then, be incapable of being aroused fully and instantly from complete sleep; which, however, is not the case: waking is, moreover, unaccompanied by any sensation of cerebral compression, the invariable consequence of the presence of an unusual quantity of this fluid. It is apparent, from this consideration, that the shrinking of the cerebral mass can only be the result of a true relaxation.

It is to be remarked, that during relaxation of the brain, owing to the peculiar construction of the veins and sinuses within the cranium, the blood is more likely to become congested around the surface, than within the substance of the organ. A diminution of the mass, and not an expansion, will therefore result.

Consequent upon the relaxation, is the vacancy already noticed, and for which an equivalent must be had. It is seen that the cerebro-spinal fluid cannot, in this case, be made available, while the blood is not only so, but is not liable to the objection urged against the former agent with regard to the phenomena of waking. Compression by the blood is also observed to produce certain pathological conditions strikingly analogous to sleep, and which are sometimes relieved only by blood-letting. The position most favourable to sleep—the horizontal—is an argument in favour of the blood, as this necessary substitute and agent of compression; the law by which fluids seek their level, being thus somewhat subsidiary, congestion more readily ensues.

Stimulants given to a person fatigued, if they do not produce feverishness, cause a disposition to sleep. Here is noticed the relaxation due to fatigue, or more strictly, to the want of nervous energy; in this condition, the stimulant being given, the heart is excited, and the blood propelled more forcibly and plentifully upon the relaxing brain; a tendency to sleep, from a kind of compression, causing greater relaxation, is the consequence. Compression, however, is not the only result; but if stimulants exercise a relaxing influence upon the capillary system of vessels—which opinion alone explains their remedial action in certain diseases, such as delirium tremens—their administration will also illustrate the importance of the relaxation; since, being carried to the blood, they will cause there an increase of the relaxation already existing. (Billing. Prin. Med.) A sedative being given, this tendency to sleep will be destroyed; for, by counteracting the action of the stimulant, and by the peculiar influence of the sedative upon the heart, the force and quantity of the blood is reduced to a normal degree; the brain is thereby relieved, and approaches the waking state. If, then, the constringent influence upon the capillary vessels, by some attributed to these medicines, be estimated, the phenomena of their administration will be fully explained; as the effect upon the nervous tissue of such a constriction of the capillaries; will be a renewal of tone and secretion of nervous power. This therapeutic consideration not only illustrates, but confirms, the doctrine of the agency of the blood in the veins and sinuses of the head, and the compression of the nervous tissues caused by the congestion; this of itself pre-supposing the existence of relaxation and collapse, which is also manifest.

Here, then, is sufficiently evidenced the true character of collapse noticed during sleep; and not only the necessity of a substitute in the stead of the collapsed brain, but the character of this substitute—the blood congested in the veins and sinuses of the head, and compressing

the inactive organs. The importance of the compression is illustrated by those cases in which mechanical pressure has been noticed to produce sleep.

Of the degree of inactivity of the brain during sleep, I have not space to argue. The question has been fully and ably discussed. Magendie, whose capacity for careful observation and deduction will hardly be questioned, speaking of the sleeper, says, "at last he entirely ceases to be conscious of his existence; he is then asleep." (Phys. p. 526.) J. Johnson asserts that "the senses failing to convey impressions, the mind has no longer materials to act on, and immediately becomes unconscious of its own existence." (Hygiene—article, Sleep.) "But when sleep is complete," says Dunglison, "the whole encephalic organ appears to be at rest." (Phys. vol. 2, p. 407.) There are, doubtless, periods when the mind is perfectly at rest, and "unconscious of its own existence."

These views will now be considered with reference to certain conditions of intra-uterine life, as connected with and sustaining the theory about to be proposed.

"The condition of the foetus in utero," says Carpenter, "may be regarded as one of continual slumber; the apparatus of animal life being completely excluded from all stimuli which could arouse it into activity, whilst the energy of the organic functions is entirely directed to the building up of the fabric." (Phys. p. 824.) Marshall Hall is probably still more definite: "the foetus in utero seems restrained to a sort of ganglionic life. Everything consists in nutrition and growth or development." (Dis. and Der. Ner. Sys. sec. 598.) Here are two distinct propositions including the views I shall consider of life in utero: the activity of the system of vegetation, and the contrary condition of the organs of animal life. In support of these propositions, there is no lack of authority. Adelon and Bichat regard the life of the foetus as one of an entirely "vegetative character," and that the animal functions are not exercised at all. Concerning the first, there can be no dissidence, provided we accept the division of the functions of the nervous system as already presented, and now maintained by physiologists. The histories, however, of anencephalous and amyelencephalous foetuses must sufficiently evidence the character of innervation requisite to foetal development, and determine in referring the organic or vegetative functions to the ganglionic system of nerves. It is during the period of uterine life, that this system will be found most actively exercised. In obedience to the principle of life, it is then preparing and combining the organs to be afterwards appropriated. Hence the

great development of this system at the time of birth. This is familiar to all, and in no way can it be explained apart from its functions as the organ of nutrition and development.

Contrasted with this condition of the ganglionic, is the inactivity of the cerebral system; the evidence of which is of both positive and negative character. We can conceive of no necessity for the action of the brain during this period of life. "The most elevated of the functions of relation—the mental and moral faculties—would seem to be needless to the foetus, and, consequently, little, if at all exercised." (Dunglison, *Phys.* vol. 2, p. 364.) The foetus is then growing, is then preparing for active life, and has nothing to do with "sensation, perception, judgment, volition or voluntary motion"—the functions of the cerebral system, as defined by Marshall Hall. Moreover, being effectually secluded, on account of its disposition within the womb, from external impressions, functional exercise of the brain could only result when excited by the varying condition of the organism; and as this organ would be usually inactive and collapsed, it would require a greater degree of force than usual in these impressions to cause perception—this perception being followed by a renewal of collapse. And as we are informed of no conditions or changes, either physiological or mechanical, that would tend to produce impressions of any other than a very ordinary degree of intensity, we infer that the perceptions from this source, if at all possible, would be exceedingly vague and transitory. This opinion will be confirmed if we consider that the gravid uterus is in a continued state of resistance, endeavouring to expel the foetus, which is maintained in situ by the contractibility of the circular fibres of the neck. For the foetus, being surrounded by a watery fluid, will, in obedience to the principal of hydro-dynamics, receive the resisting force of the womb, communicated by the liquid, in an equal degree upon its entire surface. So that this force, acting upon the separated and yielding bones of the head, will cause the several pieces to maintain close contact with the relaxed tissues of the brain; while the tissues being thus relaxed, will not only suffer compression, but will yield before this pressure, until their resistance shall balance the compressing force of the womb. This compression, prohibiting any degree of crithism, will also lessen the possibility of perception. The brain will be in a state very analogous to that of sleep after birth, and mental activity will be no more probable in the one case, than in the other.

There is, likewise, no evidence of cerebral activity at this period; the phenomenon of intra-uterine motion, assumed by some, as indicative of volition, not requiring for its explanation the exercise of the cerebral

functions. I will trace some of the most prominent points of this question, and see how far physiology will sustain the placing of these motions outside the pale of voluntary actions.

Motion analyzed, so far as it concerns us in this discussion, presents a character, not a cause, either voluntary or involuntary. Volition is not a necessary condition or cause, but only a contingency of muscular motion. There is but one power capable of producing vital motion, or of affecting muscular susceptibility; this is the "*vis nervosa*" of Haller. Through this medium, the Will, together with every character of stimuli acting as agents, produce their effects upon muscular contractibility. "We have a right to affirm," says Carpenter, "that the Will cannot exert any direct or immediate power over the muscles, but that its determinations are carried into effect through an intermediate mechanism, which, without any further guidance on our part, selects and combines the particular muscles whose contractions are requisite to produce the desired movement." "And hence it follows, that all the movements which are performed by the instrumentality of the cerebro-spinal system of ganglia and nerves, are, in their essential nature, automatic, and that their characters as reflex, instinctive, emotional or voluntary, are entirely dependent on the nature and seat of the impulses which respectively originate them." (Phys. p. 525, 526.) In his faithful portraiture of the mechanism of vital motion, it is seen that the "Will really operates through the automatic apparatus, exciting particular groups of muscular actions, just as they would be called forth by sensations directly excited by external objects." In this view, the motions of the fetus are readily explained as physical nervous actions, which, in the language of Todd, "include all those actions which are caused by a physical change, frequently morbid, in a centre or in a row, and those which result from the excitation of the vesicular matter, in which a motor nerve is implanted by the stimulation of the peripheral fibres of a contiguous sensitive nerve. These latter," in which class, those of the fetus are to be included, "have been designated reflex actions, and, although most of them in health are attended with consciousness, still this mental state is in no way necessary to the perfection of the nervous act, and should be regarded rather as an incidental accompaniment to it, than as an essential part of it." (Todd's

the Will, may affect the neighbouring nerves of motion, and thus will be caused contraction of the muscles.

But to pursue this investigation still further, it has been asserted that the "*vis nervosa*" is the medium through which all characters of influences affect the contractibility of the muscles. It is through the action of the Will upon this power, that voluntary motions are effected. This is also the power operating in Marshall Hall's division of the "true spinal" system. Thus any external agent exciting this power into action may produce motion of which "we may be totally unconscious. The immediate cause and effect are connected necessarily. The act may be attended by sensation; it may be modified by volition; but these mental operations are not essential to the true spinal functions." (Dis. and Der. Ner. Sys. p. 4.) Considered thus, reflex actions may be produced by the instrumentality of a power, which, in so far as the mere action is concerned, is wholly independent of the brain, and answering to the stimulus of the Will, just as it does to that of any class of agents, either internal or external. Thus it is, that these motions do not require for their explanation the intervention of any of the cerebral functions, but can be reasonably referred to the class of reflex actions. There is, moreover, direct evidence in favour of this distribution. M. Magendie, speaking of these motions, says, "we cannot suppose that they depend upon the Will, no intelligence at the time existing; this is also evident from the fact that acephalous infants exhibit the motions the same as the most perfectly formed children." (Phys. p. 248.) Here is proof positive; nothing can countervail the force of real observation. In these cases, motion must have been caused by reflex action; and it is highly probable that in this manner are produced all the motions of uterine life. From this class of cases, it is very manifest that such a character of nervous action is all that is requisite to the production of these motions; and as more causes than are true or sufficient are not to be assigned, it is not only a rational, but

relaxation. The moment of the introduction into the world, is the time when the brain, excited powerfully by the various circumstances of birth, together with the sudden change of physical condition, begins the real exercise of its appropriate functions.

Assuming, then, that the walls of the cranium were continuous before birth, the brain lying inactive and collapsed within this bony vault, there would be required the presence of a sufficiency of blood or cerebro-spinal fluid to occupy whatever vacancy might exist. Were this vicarious element furnished by the blood, in the veins or sinuses of the head, there would result such a weakening of these tissues, from the continued distention necessary to the congestion, that these would probably never regain a healthy degree of tone; and even did they, there would be a period of considerable duration, in which the life of the being would be in continual jeopardy, as there would ensue a congenital tendency to affections of the brain.

But more serious evil would result. The brain, continuing to grow while in this collapsed state, would, gradually encroaching upon the space occupied by the blood, eventually fill completely the cavity of the cranium, as in the case of the acting brain after birth. Were the child born with the collapsed brain, thus filling a cavity bounded by a continuous wall, it is apparent that the appropriate and specific excitants could have no effect upon the dormant and relaxed mass of organs; as the continuity of the cranium would oppose effectual resistance to any degree of erethism—the condition of activity. The brain could not expand; its faculties, by a law of its own nature, would be irresistibly confined. The child would be born in a state virtually similar to that of the anencephalous; and viability would be no more probable in the one case, than the other. The conclusion is inevitable, that life, under such circumstances, would be physically impossible.

Against this, it cannot be objected, that such an arrangement would compromise the safety of the parturient state. Were this all that were to be considered, it would have been possible with Him, whose fiat is the cause of life, to have restricted the development of the foetal head within narrower limits, or to have permitted a still greater development of the female pelvis, or to have modified both in such a manner, that safe delivery could have still resulted. We know that children are born, and live with heads sufficiently small, while there are pelves sufficiently large.

But I will consider the opposite condition, that in which these bones are separated by the normal interstices. The bones being disconnected, there will be a natural tendency, occasioned by the nutrition and growth.

of the brain, to adapt themselves to the form and size of this organ. There will also be the contractile power of the womb to aid in keeping the bones of the skull and the mass of the brain in close contact. There will, in consequence, be no vacancy, and, therefore, no need of any extra amount of fluid in the head; a circumstance highly deleterious to the integrity of the tissues. Having reached the customary period, this disposition of the bones favouring safe and speedy delivery, the child is ushered into the world in a state of anæsthesia. Being powerfully excited, the brain expands; and expanding, carries before it the cranial pieces; and now begins the exercise of its peculiar functions. Action and healthy life, and intellection, ensue, and the cranial pieces are gradually and perfectly ossified. In this do we discover the true physiological significance of the sutures; not that through them parturition is not facilitated; not that various principles may not concur in their development—principles illustrative of the economy of creation; but that the sensorium expanding in obedience to the sudden stimulation, they permit the requisite yielding of the disconnected bones, which in this light become more especially true *tutamini cerebri*. Hence, I affirm their appropriate and true office to be that of guarding the life and intellection of vertebrate animals; since, without their existence, active and vigorous life in certain of this class of animals, would be not only impracticable, but physically impossible.

This condition is not only adapted to the contingencies of life in vertebrate animals, but absolutely required in some classes of this division. In the class of birds, the activity of the brain is indispensably necessary to the delivery from the confining shell; which activity will be permitted by this condition of the bones, while the contrary would effectually prohibit such a state. The roomy disposition of the young within the shell, will also favour activity of the brain, of which there is ample evidence.

Not aware of having in any manner violated the principles of a fair and legitimate deduction, and conscious only of a paramount regard

in right lines, yet there are mediums of sense and other circumstances to reflect and refract its brilliant corruscations. The beam may permeate and illumine its limpid medium, yet it tends not directly to its object. In its mission of life to man, even the pure air of heaven obstructs its pathway.

ART. III.—*Successful Operation for the relief of Artificial Anus.* By
T. T. ROBERTSON, M.D., of Winnsboro, S. C.

Messrs. Editors,—The following case, I think, will not be without interest to your surgical readers. On the 24th Feb., 1853, Peter, aged 55, the property of Reuben Motley, was brought to me with strangulated inguinal hernia. According to his own account, and that of his master, the hernia had been strangulated about 36 hours. On the morning of the 23d, the physician in the neighbourhood was called in, and attempts were made at reduction. In the afternoon of the same day, a second physician was called, in consultation, and further attempts at reduction were made. It was now proposed that I should be sent for, to operate, but it being night, and very cold, the owner of the negro concluded to wait till morning, and bring the case to me, a distance of fifteen or sixteen miles. He was hauled about four miles, and then put upon the cars. The following was his condition when he arrived: the extremities cold, the pulse small and quick, nausea and occasional vomiting; no pain, except when walking, which he did in a half-bent position. The scrotum and penis were œdematous, and gave no pain when handled. Ice was applied whilst preparations were being made for the operation. After the application of ice, the taxis was tried, but without beneficial result, as was anticipated. Assisted by Drs. Clarke and Aiken, I now proceeded to the operation, which was performed in the usual way, and almost without pain; owing, no doubt, to the anæsthetic effects of the ice. Upon opening the sac, it was found to contain about four or five inches of intestine, and a portion of omentum, about the size of a hen's egg, all of which seemed to be disorganized. This was the opinion of all present, and just what was looked for before the operation. The stricture was at the external ring, and upon being divided, it was found that the intestine was perforated at this point, in one or two places, so as to allow the contents to escape. The intestine and omentum were now both carefully and thoroughly examined, and according to all the rules laid down in the books, were condemned, and pronounced unfit to be returned into the cavity of the abdomen.

The only chance for our patient now, was to be left, if left at all, with an artificial anus—dreadful alternative; but little preferable to death itself. Our duty, however, was plain. To return the hernia into the cavity of the abdomen, in its disorganized condition, we regarded as equivalent to death—with an artificial anus, there was still a chance for life. The whole of the strangulated portion, including the omentum and several inches of intestine, were excised. Previous to excision, a double ligature was passed through the mass of omentum, and each lateral half ligated separately. Several stiches were taken in the scrotal part of the incision, and cold water dressing was applied.

The following notes of his condition after the operation, have been furnished me by Dr. Aiken :

Feb. 25th. Patient rested well last night; cal. 2 grs., op. 1 gr. were given him; his pulse this morning is 96, full, but soft and compressible; no pain, except in the incision; skin moist; has some appetite.

26th. Rested well through the night, after taking cal. and op.; pulse 80 this morning; skin moist and warm; slight thirst; no more soreness than yesterday; had an evacuation of soft fecal matter through the wound in the morning, about 10 o'clock; continued through the evening at short intervals; had no fecal smell.

27th. Slept well last night, after taking cal. and op.; perspired freely; discharges from the bowels copious and involuntary, of an ochre colour, and without fecal smell; pulse 78; tongue furred; no more pain, but felt a fulness about the region operated on. In the evening, the pulse 84; the soreness no more extended, but the patient did not seem so well; no appetite, increased thirst; depression of spirits, slight nausea, and troubled with hiccough.

28th. The patient slightly better than yesterday; he rested well; had taken cal. and op.; pulse and skin as the day before. In the evening, pulse 92, but no particular change during the day.

March 1st. No medicine last night, and patient does not feel so well this morning. The pulse indicated rather a change for the worse; no more frequent, but small and quick. The skin, particularly of the extremities, is cold; discharges frequent; no transpiration; pulse more frequent in the evening; cal. and op. again prescribed.

2d. In the morning, pulse 76 and small, extremities cold; smell from the wound stronger and more disagreeable. Evening—patient seems to be nourished but little by any thing taken, and, doubtless, absorption does not take place as it should, from the continued action of the bowels. He appears sinking from inanition; appetite is still pretty good, but he grows weaker; pulse 82.

3d. Pulse still weaker, but the patient has appetite; rested pretty well after taking 40 m. tinct. op. Wine given him in the morning.

4th. This morning, the patient seems as talkative and hopeful as ever. His appetite continues good, but he loses flesh daily, and becomes weaker. His pulse is very feeble, but not rapid. Evening—he is unusually lively; his wife had come to see him during the day—the first visit during his illness. He was washed off from his filth better than he had been since the operation, and was made more comfortable; wine taken regularly.

5th. There was no feature worth relating in the case; patient very weak.

I find that Dr. Aiken has taken no account of the wound. No part of it united by first intention. The sac which had been found to be adherent everywhere, was thrown off by suppuration, and the wound was now assuming a healthy appearance. On the 6th, his master came to see him, and it was agreed to let him return home, where he would be better nursed. He was such an object of disgust, that it was difficult to have him attended to.

I did not see the patient again, till the 1st of April, when I visited him. I found the wound had healed, all except the part occupied by the open orifices of the intestine. The patient was cheerful, and able to sit up and attend to himself, but was very much emaciated. He had a good appetite, but his food was not assimilated. The portion of intestine that had been strangulated and removed, I judge, was a part of the jejunum, and the absorbing surface of the bowel was too short. Besides the irritation of the ends of the bowel, at the fistulous opening, increased the peristaltic action, and caused the aliment to be carried off too rapidly. The orifice was occupied by a red nipple-shaped tumour, supposed to be a portion of omentum, not retracted within the canal. The feces were seen escaping at the base of this tumour. I cauterized it freely, and directed it to be repeated till it was brought down to a level with the surface of the skin. I gave directions about diet, compresses, and cleanliness, and consoled the old man as well as I could, but I really felt that it was no cause of congratulation to myself or patient, that his life had been prolonged.

On the 6th of May, I was again importuned to visit my patient. I found him still more emaciated. He suffered with colics. The discharges were acrid; the skin for some distance around the fistulous opening was excoriated. The tumour which I had directed to be destroyed with caustic, was larger than before, and was now discovered to be the lower portion of the intestine prolapsed, instead of omentum,

as was at first supposed. There was now another small fistulous opening about an inch from the first. The feces, or rather the aliment, was passing off almost continually. The patient said his food would pass from him in fifteen or twenty minutes after taking it, and looking just as when he swallowed it. His looks certainly proved that very little of it was assimilated. I now determined to try Dupuytren's operation for the cure of Artificial Anus. This operation consists in destroying the septum between the two ends of the intestine, and opening a communication from the upper to the lower portion of the canal. Various plans have been devised and practiced for the relief of this most deplorable and unfortunate of all accidents, but without satisfactory results. The idea of exciting adhesions between the upper and lower portions of the intestine, and then dividing the septum, originated with our countryman, Dr. Physick. His plan was to pass a suture through the septum as far up as could be done with a curved needle, and when adhesion was supposed to have taken place, the septum was divided with the scissors or bistoury. Dupuytren practiced the same operation about the same time or a little later, without a knowledge of Physick's operation. Priority of discovery is claimed for both these distinguished surgeons. The operation failed in the hands of Dupuytren, but succeeded with Physick.

To the French surgeon belongs the credit of devising the instrument called the Enterotome. "This instrument is a kind of forceps calculated to seize the parts, to hold them in opposition, and forcibly to compress them. The result of its application would be the destruction of the vitality of the inclosed part; the detachment of the dead portion by the sloughing process, and the union by adhesion of the adjacent living parts, held in opposition by the instrument."

I had never seen the instrument of Dupuytren, but with this knowledge of its *modus operandi*, I had one constructed by Mr. Heintz, an excellent instrument maker, in Columbia, which served the purpose very well. The whole instrument is five and a half inches in length; the blades about three inches, and fitting one in the other, like a tongue and groove. The branches are detached like delivering forceps, and made to approximate or separate by means of a screw-vice in the handles. The female blade of the forceps is not more than the eighth of an inch in width; the male blade not quite so much. Both blades, when closely approximated by means of the screw in the handle, are not more than one-fourth of an inch in their longest diameter.

On the 13th of May, my patient returned to town, to try the effects of the Enterotome. I at once proceeded to apply the instrument

without any preparation. There was no difficulty in finding the lower portion of the bowel, for it was protruding nearly an inch, and looked like a fungous growth from the artificial opening, concealing from view entirely the upper portion of the canal. After a few minutes' search, I succeeded in introducing the male blade of the instrument into the upper portion of the canal; the female blade was then, without any difficulty, passed into the lower portion, the instrument locked, and screwed up as tight as it could be made at once. Dupuytren's mode was to tighten the instrument gradually from day to day; but I think this was certainly an error.

The patient complained a little, at the introduction of each blade of the forceps, but none while compression was being made. The operation was performed about noon, and at 8 o'clock in the evening I visited him, and found him as comfortable as he had been at any time since the operation for the hernia—gave a dose of morphine to insure a good night's rest.

14th. Found the patient sitting up at breakfast; said he never spent a better night, "but that the scissors had not stopped the running." Gave nothing, but enjoined strict diet and recumbent position.

15th. Impatient to get out of the house; some tendency to further prolapse. Strict injunction to keep the recumbent position, and morph. administered.

16th and 17th. Nothing worth reporting.

18th. Enterotome came away early in the morning, bringing away a black slough, the full length of its jaws, showing how effectually it had done its work. Evening—gave enema of salt water, which brought away some mucous only; applied a Hall's truss, but found there was too much soreness to wear it.

20th and 21st. Continues to pass every thing through the artificial opening; conclude to wait till all the inflammation and tumefaction caused by the application of the instrument is reduced, before resorting to the truss again.

24th. Applied the truss again, and gave an enema of salt and water, which brought away a quantity of very hard scyballæ, that must

kept on all the time. Nothing passed through the fistulous opening to-day, but had two good evacuations per anum.

26th. Patient dressed up, looking neat and clean again. From this time the feces passed regularly by the natural anus, a little, however, of the fluid part escaping through artificial opening; patient regaining flesh and strength rapidly. Exercised the prolapsed portion of intestine, which showed no disposition to retract. The edges of the fistulous opening were touched every second or third day with nitrate of silver. Being anxious, however, to send my patient home perfectly cured, I set about trying to close the fistula by adhesions. All who have attempted to close fistulous openings by adhesion, will know how difficult it is to accomplish. I commenced by paring off the callous edges, and using the interrupted suture; this failing, I tried the figure of eight or hair-lip suture, with no better result. The quilled suture was also tried, and failed.

Seeing, now, that the fistula would have to be closed by the slow process of nature, the patient was discharged, and sent home to his master, with directions to wear the truss and a soft pad constantly, and to touch the orifice occasionally with nitrate of silver. He was sent home some time about the first of July, and soon resumed his ordinary occupations. I saw him again, about four months afterwards; the fistula was not entirely closed, but very nearly so; barely admitting the introduction of a small silver probe, such as is found in pocket cases. A heated probe was passed into the orifice, to the depth of about an inch, which produced an entire closure in a few days.

This is, probably, the second successful operation for the cure of Artificial Anus, upon the plan of Dupuytren, that has been performed in this country.

Dr. J. Mason Warren, of Boston, operated successfully in 1847, under much more unfavourable circumstances. He speaks of his case as being the only one, up to that time, according to his medical experience. I have seen no account of any since.

Dr. Warren's case is reported in the *American Journal of Medical Sciences*, July Number, 1848.

ART. IV.—*A report read before the City Council of Charleston, and ordered to be printed with the proceedings ; with an appendix, in reply to the report of WM. HUME, M.D. By THOS. Y. SIMONS, M.D., Port Physician, Medical Adviser, and formerly Chairman of Board of Health, ex-President of Med. Soc. of S. Carolina, and ex-Vice-President of the American Medical Association, &c., &c.*

To the Honourable the Mayor and Alderman of Council :

Gentlemen—I have read, with peculiar attention, the elaborate report of Alderman Hume, as regards the causes which produce epidemic Yellow Fever in our city, and his remarks as regards quarantine regulations. In connection with my other duties, my whole medical life has been devoted to medical police, external as well as internal, and my best efforts, according to my humble capacity, have been to recommend all practical means in my power, for the promotion of medical hygiene. Hence, I am always ready and willing to co-operate in any measures conducive to the preservation of the health of the city of Charleston. In addressing you, therefore, on the present occasion, it is not my intention to inquire into the accuracy of the facts stated in Alderman Hume's report, or the justness of his inferences and conclusions derivable therefrom. My only motive is to give to you, and through you to the public, the system of external medical police, as it has been conducted, and a cheerful acquiescence and co-operation in any more efficient measures which you in your wisdom may deem more practical and more effectual.

I have been long aware of the conflicting opinions here as well as in other countries, as regards a proper regulation of quarantine restrictions to prevent the introduction of disease ; and hence I, as a public officer, have endeavoured, to the best of my abilities, feeling the responsibilities of my station, under your authority, to do the best in my power. Alderman Hume's report, which I know he did not so intend, might lead many to believe that there were no quarantine regulations as regards the introduction of disease, while, in fact, as I understand him to say, he only regards the present system not sufficiently stringent for the purposes contemplated. If the city can be better protected by more stringent and more effectual quarantine regulations than those now existing, and the proper means and appliances for the accomplishment of such an end can be adopted, and these plans prove efficient, it will be a great blessing as regards the preservation of health. Permit me, then, to present your honourable body the views which I publicly expressed in my address to the State Medical Association, in 1851, as

their orator, my subject being an *Essay on Yellow Fever*, as it has occurred in Charleston, including its origin and progress up to the present time. In that portion of the address in referring to the probable causes of the existence of Yellow Fever, I gave the different opinions of our eminent medical gentlemen on the subject, and then made the following remarks as regards it being introduced by importation.

I have thus given you the opinions of our own writers and physicians. You are all, doubtless, well acquainted with the different views taken by medical authors in the United States as well as Europe, and I think, after a thorough investigation, you will readily perceive different conclusions drawn from the same facts, and, indeed, what is more unpleasant, often discrepancy of opinion, as regards the facts. Likewise, that bilious remittent fever has been not unfrequently confounded with yellow fever, and above all, in the ardour of discussion, philosophic research after truth has too often been lost in the maze of controversial sophistries. Yet, I think that the most general opinion is, that the yellow fever, when epidemic, arises from local causes, while its contagious, or rather infectious influence, under certain circumstances, may occur. All epidemics may become infectious, without being strictly contagious or capable of transmissibility or extension, beyond a certain limit; and hence, in former ages, they were all regarded as contagious, whereas, now it is more than questioned by many.

Let us now inquire into a very interesting point of medical philosophy, which may throw some light on this subject. In former times, the system of medical police and medical hygiene were miserably pursued. It was then in the treatment a rule to have all fresh air excluded from the sick, for fear of catching cold. Now, from this regime, we observe that not only there was an accumulation of fomites emanating from the body, but an exhaustion of vital air, oxygen gas, and the accumulation of carbonic acid gas, (non vital,) hence the aggravated type and infectious character of the diseases then existing; * and under such a system of regime, it is not at all surprising that those, as nurses or friends, who were exposed to such a vitiated air, should themselves feel the effects of the disease. Hence, the terrible accounts which were given of the infectious and pestilential nature of epidemics,

* I now mention here as evidence of the antiquity of a correct medical opinion

and the violence and mortality incident thereto. From chemical discoveries, and the judicious application of these discoveries to useful and practical purposes, a new system of medical police and medical hygiene became gradually to be adopted, the effects of which have been signally observed wherever they have been faithfully used. We now regard it as essentially important, to have a free ventilation to such an extent as to correct the vitiated air produced from the sources already stated, and we likewise use certain substances which are regarded, at least, as partially disinfecting agents, and that plan has had a manifest influence and effect in diminishing the aggravated types of epidemics. We hear now no longer of the typhus gravior, which was so strongly depicted heretofore, which, says Southward Smith, is not known in the hospitals; but we have had evidences of the same disease in cases of emigrants crowded on board of vessels ill-ventilated, and accumulation of filth generating a foul and pestilential atmosphere, and creating a fever very similar to the description given of the typhus gravior.

From the great improvement in medical police, the apprehension of the propagation of these fevers by infection has been much questioned by many, and the virulence and mortality evidently greatly diminished. Predicated upon these principles, that extraordinary genius, Bonaparte, performed an act which had a powerful influence as regards his undaunted courage among his soldiers, and of good sense by his medical staff, when he passed through undismayed the hospitals in Arabia, encouraging his men suffering with the plague, a disease which, like yellow fever, was regarded with as much horror as the leper in ancient times, and which made the boldest warriors shrink from it with terror. These hospitals were well ventilated, and the virulence and mortality was much diminished.

There can be no doubt, then, that an atmosphere can be so generated in a ship or residence, as to produce a similar disease to those in its immediate atmosphere; but whether a person or persons coming into a healthy city could propagate the disease, so as to vitiate the atmosphere of a whole city, and create an epidemic, is very questionable. I will here introduce two very interesting cases I published many years ago, illustrative of this subject. A Spanish vessel came into Charleston and anchored at quarantine, with all her officers sick with yellow fever. The mate of an American vessel, sailing from New Orleans to Philadelphia, was placed on board the Spanish vessel, while at sea, to navigate her to Charleston. The mate, when I visited her, was perfectly well; the next day, however, he was seized with yellow fever, and died on the fourth day with black vomit.

To satisfy my mind as far as practicable, I wrote to New Orleans to ascertain if yellow fever was prevailing there at that time, to which I received in answer it was not. I then wrote to the port physician of Philadelphia, to ascertain if any sickness existed on board of the vessel on its arrival at quarantine, and the reply was—"There was none."

Now, here seems to me a clear case of infectious influence.

Again, of another disease, viz: cholera—I will cite an instance which occurred on our seacoast, and which I published. The brig "Amelia" was stranded on Folly Island, about twenty miles or more from Charleston, with patients suffering from cholera. Several of those who went to the wreck and encampment near, thereby took the disease, and as soon as the vessel and all her materials was burnt, the disease became arrested. Two of the sailors absconded, and arrived in Charleston, sickened with cholera in Elliot-street, and died—but no other cases occurred in the city, nor was it propagated therefrom.

Dr. Dickson wrote a very graphic account of this circumstance, and it was regarded by him and the contagionists as a most powerful evidence of its infectious character. Now, I admit and concur in the fact of its infectious influence to the extent in which it was generated. But still it is questionable if it could have extended beyond a certain limit or, in other words, would have in a city produced a general epidemic. Be this as it may, the circumstances prove an important lesson in medical police, to prevent all cases of such kind being introduced into a city. From what I have presented to you, you will perceive I have endeavoured to do so in the most impartial manner. You will likewise perceive that I regard it as peculiarly essential to have a judicious regulation of external and internal medical police. The laws of epidemics are puzzling problems in medical research, and have not been satisfactorily explained, either by chemical analysis or meteorological observations. Yet the investigations in these respective departments of scientific research have led to much valuable and useful improvements in medical police, if not in preventing, at least greatly ameliorating, the mortality of epidemic disease.

In another part of my address, after giving a description of the condition of Charleston in former times, as taken from the valuable history of Carolina, by the late venerable and distinguished Dr. Ramsay, I remark :

Such, then, was the condition of Charleston when it was early settled. Gradually and progressively all of these ponds and creeks were filled up. Subterraneous drains in the different streets were made, and drains from the yards of respective premises were made to communicate with

the drains in the streets. But it was found that the drains having the grates in the centre of the street, and becoming thus filled up, caused after heavy rains, a vast quantity of water to be accumulated in the streets, making them, at times, almost impassable. The offals of the city were required regularly every day, except Sunday, to be removed. The first improvement was the paving of East Bay-street and Elliot-street, where the yellow fever was generally the most rife; a great improvement in the healthiness of these localities was found to be the result. The great and general improvement, however, commenced in the administration of General Hayne, in 1836, at which time I was a member of the Council. It was then determined to make a general system of internal medical police. The leading objects were to have :

1. Every lot drained thoroughly, so as not to allow water to remain thereon and become stagnant; and if they could not be drained, to have them filled up.

2. To have all the cellars existing, to be kept so constructed as not to allow accumulation of water in them; and if this could not be done, to have them filled up.

3. To forbid subterraneous cellars in all subsequent buildings.

4. A great and important object was adopted, to have the streets paved or macadamized convexly, and to have side drains in the streets communicating with the main drain in the streets, thus preventing the accumulation of water; and having, likewise, the drains thus cleansed, and thereby preventing the accumulation of filth, requiring them to be opened and cleansed—a great cause, in my opinion, of disease.

Now these plans have been progressively going on, and in proportion has the healthiness of our city been improved; and while the public authorities are gradually, nay, I may say actively, pursuing this plan, I firmly believe it will be the cause of making the city not only one of the healthiest among commercial cities, but may possibly make us in a great degree, if not entirely, exempt from yellow fever, and should it occur, lessen its virulence and mortality. From a long experience and observation, I regard it the solemn duty of the public authorities to go on with this plan, regardless of expense, not only for the preservation of health, but for the extension of commercial prosperity.

From the statements which I have brought to your view, you will readily perceive that while I regard it as proper to prevent a fe-

ourselves. The true, and, in my opinion, the wise plan, is to attend particularly to internal medical police, with a judicious care as regards introduction of it from other sources. I must, in candour, say that I do not believe the yellow fever was ever produced in Charleston from importation; and this is the result of an experience of thirty years as Port Physician, and twenty-four years a member of the Board of Health, many years of which I have been its Chairman.

I will now state what measures have been used as regards quarantine regulations.

1. In summer, all vessels coming from ports where yellow fever occurs, are brought to quarantine; if all are well, they are permitted to come up.

2. If any are sick, the vessel is retained at quarantine; the sick sent to the Lazaretto, and the cabin and fore-castle cleansed and purified as far as practicable. We have no facilities for doing any more.

3. In winter as well as summer, any vessel having sickness, is brought by the pilots to quarantine, and said vessels are inspected, and the same measures pursued as in summer quarantine.

In Alderman Hume's report, he affirms that the epidemic yellow fever which has existed here, has been the result of importation, which I think he has not proved. He has stated some accidental coincidences, but to my mind, from my observations, not at all satisfactory or convincing. Still, while I do not concur in his inferences and reasonings, I am perfectly willing to co-operate cheerfully in any improvement in regulating quarantine that might prevent the introduction of disease.

Alderman Hume, as I understand him, has come to the following conclusions:

1. That the crew and passengers of a vessel arriving at quarantine may be well, and after coming to the city, cases of fever may occur, and thus cause an epidemic.

2. Even if this do not occur, the cargo may be infected, and the labourers who discharge the vessel may take the fever and spread it over the city. The latter circumstance has not occurred in my experience of thirty years.

The necessity of unloading vessels may, however, occur, and it is proper to make arrangements for such.

Permit me, now, gentlemen, to say, whether you adopt all of Alder-

In several instances I have been placed in painful circumstances, for the want of accommodation, when vessels having emigrants have arrived at quarantine. In these cases, I felt it a duty to remove the sick and the well out of the vessel infected, to a pure air, which has not only diminished the virulence and consequent mortality among them, but arrested its progress among the well. The building at the Lazaretto were so inadequate to accommodate them, that the physician's house was opened to them, and I have been indebted to the gentlemen owning houses not far from the Lazaretto, for their use, or I know not what I could have done. And if there are some differences in Alderman Hume's views and my own, theoretically, *practically* I not only agree with him, but hope his efforts may be successful.

I have intruded too long on your patience; but the importance of the subject, my desire to show you what I have done with my limited means, and my desire to declare (as I think my past life has proved) my willingness to make any sacrifice for the promotion of the health of our city, must be my apology.

APPENDIX.

It will be perceived, that in this report I gave only my general views to Council, expressing a perfect willingness to co-operate in and to obey any regulations which might be deemed more efficient than those which had been already adopted, while I felt that it was my duty to present my own opinion as the result of long experience and observation.

My report will demonstrate that, in the hygienic measures for the preservation of the health of a city, two things are necessary:—a vigorous effort for the removal of all the causes of disease which may exist among ourselves, as well as the use of the most practicable and available means of preventing its introduction from abroad. On these points the report explains every thing that I know; and while I could not be so presumptuous as to resist any more stringent means of external police that might be deemed expedient, it will not be expected that I should pass unnoticed the extraordinary report of Dr. Hume when not addressed to the city authorities alone, but to the medical profession,

should have impartially investigated the subject; and if, for reasons sufficient to himself, he has become a convert to opinions antagonistic to his previous views, which were in accordance with those entertained by a large number of the medical faculty since the year 1800, he should at least have been more courteous and respectful, and less arbitrary and dogmatic, in the promulgation of his newly-adopted creed.*

Dr. Hume, while he has for many years abandoned the medical profession and engaged, from time to time, in various and other pursuits, has always been devoted to scientific research and investigation, and exhibited in these studies an earnestness and disinterested zeal deserving of all commendation. He is naturally of an enthusiastic temperament, and hence, in his hours of leisure from the discharge of his duties as Professor of Experimental Philosophy in the State Military Academy, he has occupied himself in attempts to ascertain the cause of the origin of yellow fever among us. In the year 1850 and '52, he published clever articles in the Charleston Medical Journal, the object of which was to show that the disease was the result of local causes. His investigations, however, did not lead to the discovery of such a fact, and he subsequently changed his opinions. It now appears that Dr. Hume, having failed in his desire of discovering the origin of yellow fever by meteorological means, took it for granted that we were going wrong, that all the improvements and researches of modern times were false, and that we must fall back upon the opinions which originated in the 17th century, that yellow fever is introduced by importation; and it is, therefore, now heresy with him for any one to say that it is derived from local causes among ourselves.

From meteorological observations, Dr. Hume positively and publicly

* I quote the following extract from a review of Dr. Strobel's Essay on Yellow Fever, as admirably adapted to Dr. Hume. Dr. S. is the author from whom he has borrowed all his opinions, reasonings and facts, and his peculiar style. The criticism is from the pen of Dr. Condie, of Philadelphia:

"The most interesting portion of Dr. S.'s work, is that which comprises the facts connected with the occurrence of yellow fever in the city of Charleston in the year 1839. Even these facts are defective in many important particulars, and by no means so conclusively establish the author's views as to the origin and spread of the disease in that city as he seems to imagine.

"We are indeed convinced that the publication of the volume before us will have but a slight influence in gaining converts to the doctrine of the transmissibility of yellow fever by infection, generated in the manner supposed by the writer, while it

declared, that yellow fever must prevail in 1853. This opinion, which created some sensation in the community and caused many timid persons to leave the city, was predicated upon the condition of the dew point. But in despite of the dew point, in despite of the prevalence of yellow fever in New Orleans, Mobile, Havana and many other places, in despite of four sporadic cases occurring on the wharf—with the same medical police, internal and quarantine regulations in force as heretofore, having all the phenomena of his meteorological indications presented in their fullest and strongest force, his new theory being the result of existing importations—the yellow fever *did not appear* among us. For this exemption we thank God, and rejoice that, exempt ourselves, money was collected in large amounts from a grateful and sympathizing people, and sent to our afflicted sister cities.

Let us see how Dr. Hume, after the statements he had made, accounts for our extraordinary exemption. I quote his own language entire :

"The exemption of Charleston from yellow fever, during the last summer, is a subject of astonishment to all observers. That the season was unpropitious to health, the ravages of the fever on the south-western waters and other regions, fully exhibit. That we shared the general diffusion of unpropitious atmosphere must be admitted, and our meteorological instruments testify to the same ; yet the devouring angel spared our city. Was it that he came and found not a congenial atmosphere and soil ? Or did he not visit our city to test our weakness ? We apprehend the latter, for we can find no traces of his footsteps within our limits. It is true that two attempts were made by the fever to invade our city, but it was rudely and unceremoniously repelled and expelled on both occasions. As action was prompt, the history is short. Captain P——, of the Ellen Goldsborough, arrived from Baltimore, on the 30th August, sickened on the 4th of September, was sent to the Marine Hospital on the 6th ; was declared to have yellow fever, and on 7th was sent to the Lazaretto, where he died on the 10th. On the 23d Sept. Capt. P——, of the Berkelew, was reported under similar circumstances, and was immediately sent to the Lazaretto, where he also died. Thus ended both attempts at invasion ; and we may here remark that it is the first instance of *Jacksonism*, in a matter of this kind, ever under-

of South-Carolina, which has been hitherto withheld, and they abjure the dangerous doctrine 'that the rigid enforcement of the quarantine laws was by no means necessary on account of yellow fever.' Dr. David Ramsay, in the year 1800, writes to Dr. Miller, of New-York, that 'the disputes about the origin of yellow fever, which have agitated the Northern States, have never existed in Charleston. There is but one opinion among the physicians and inhabitants, and that is, that the disease was neither imported nor contagious.' Now the fruits of those disputes in the Northern States have been, the proof that the fever was imported, and was contingently contagious, and on these creeds they have succeeded in excluding it from their cities for the last thirty years; while we, who believe otherwise, have been nearly destroyed by its ravages in the same period. Do not the results show which is the true creed, and can there be any difficulty in explaining the revolution which has taken place in the medical faculty of our city? They have collected facts and arrayed them against doctrines."

Again—

"The absence of the fever from our city last summer is still a matter of general astonishment to all observers. Why should New-Orleans be depopulated, and Charleston preserve its health? It is a point which has not escaped the examination of your Committee. All local and general causes remain the same, but the most minute inquiry cannot discover the introduction of a case from abroad. No drunken sailor brought the germ of disease to infect our atmosphere. No unclean vessel lay at our wharves to be visited by a foreign *segár* merchant, to inhale the disease and return to his shop to sicken and to die. No infected ship sent her crew to the Marine Hospital—in fact, excepting the cases already referred to as sent to the Lazaretto, the fever did not enter our city; and to this cause, probably, we are indebted for the preservation of the health of the city."

As to the Captain of the *Ellen Goldsborough*, Dr. Hume is correct, but he has omitted to state that two of the seamen were sent down from the same vessel, who recovered. Now, the captain was sick for some time in the Marine Hospital, had the black vomit, and although

Hospital, to have caused it to spread, according to Dr. Hume's notions, as will presently be seen, of the poisonous nature and contagious character of the disease, especially in connection with his meteorological indications. There was no case introduced, nor have drunken sailors, nor any others, sick with yellow fever, ever been permitted to come up to the city, and it is a calumny to say so.

But he has again committed a blunder. The vessels from Havana and New-Orleans, all their crews being well, came up to the city, lay at the wharves, discharged their cargoes, and "segar merchants" visited them, but did not sicken and die. Now, if this does not prove mis-statements and illogical inference from assumptions, I know not what does; and such is the case in almost every part of the report.

Dr. Hume, in the expression that the disease "was rudely and uncere-moniously repelled and expelled," seems to me to do injustice to the mayor, who, actuated by the best motives, performed his duty with kindness and humanity. As to the "Jacksonism" of the matter, with due deference to the mayor, I must be permitted to say that his predecessors, under similar contingencies, have acted in the same manner.

With these preliminary remarks, I shall proceed candidly to investigate each of Dr. Hume's statements as going to prove the production of yellow fever in this city by importation.

After mentioning the years in which the fever has prevailed in Charleston, he makes the following remarks: "Shall modern energy quail before this monster, or shall we resolve to rise above the contingencies that give it birth? Shall we receive and worship it as a monarch, or shall we expel it as a tyrant? Our safety demands the latter, and may success attend our laudable endeavours to accomplish our sacred purpose."

Now, this is certainly not the language of philosophy, nor is it either very Attic or classical in style. Words so emphatic, and I may say grandiloquent, would seem to imply that all investigators into hygiene and the progress and probable cause of epidemics, were asleep, or, if awake, at least indifferent to the means of preventing disease and saving life, and, as his report subsequently implies, that the quarantine regulations were loosely and carelessly carried out, from an apprehension of injuring our commerce and a fear of the displeasure of the mercantile part of the community.

it is well known that the law is continually evaded, passengers going over to the Jersey shore and thence to the city. If, however, the state or the city would incur the necessary expense, as has been done in New-York, I think it would be an important and useful measure; for in my report to Council, to which I refer, it will be seen that I regard it as proper to adopt active hygienic means for preventing infected vessels from coming into a city, without a proper removal of the sick, and ventilation and disinfection of the vessels.

But, while agreeing to all this, I cannot concur in the statement of Dr. Vaché (State Commissioner of Health to the Marine Hospital on Staten Island), as quoted by Dr. Hume, "that the malady has not appeared in this city [New-York] for nearly a quarter of a century, and not since the present health laws have been rigidly enforced, and never will as long as the statute remains as it is, and its provisions are observed to the letter."

I do not know Dr. Vaché, but I am compelled to pronounce his language too strong and latitudinarian to create in me much respect for his opinions. Will he inform me how often, in more than thirty years, has yellow fever occurred in Boston, Philadelphia or Baltimore, where no such quarantine regulations have been enforced, or even in New-York how many persons have, by rail road, visited the city? It is true that it was said that the fever was introduced into Philadelphia by the barque Mandarin, from Cienfuegos, which Dr. Hume boldly says is a fact too well established to be disputed. Now, without taking either side on this question—for I am clearly opposed to any vessel having sickness on board being allowed to come up to a city until disinfection has taken place—still I must be permitted to say that the fact of the introduction of yellow fever by the Mandarin is denied by highly intelligent and eminent physicians, acquainted with all the circumstances of the case. And if the Mandarin did introduce the fever, it is remarkable that the disease should have been so circumscribed in its ravages. The small pox, a confessedly contagious disease, would have exhibited a very different result.*

* Since this article has been in the hands of the printer, a report of the yellow fever which prevailed in 1853 in Philadelphia—read before the College of Phila-

With respect to the steamer *Elair*, Dr. Hume makes the following remarks—

“Its capability of introduction into other countries is admitted by all who have carefully examined its history with unbiassed minds, but still, commercial interests induce many to believe that the matter is not fully established. The yellow fever, as an epidemic, has never been introduced into England; quarantines there are useless; hence the quarantines of other countries are injurious to British commerce, and the British government denies the necessity of them, and strives to prove their inutility. The last and most memorable case of an attempt to suppress the truth, was the introduction of the yellow fever into the island of *Boa Vista*, one of the *Cape de Verde*, by the British steamer *Eclair*. The government sent Dr. McWilliam to investigate the matter; on his return, his report was unfavourable to the government views of the subject. Dr. King was then sent, who returned with opposite views. Although he could not disprove the facts of Dr. McW., he gave them a different construction, and thus the government was enabled to maintain the integrity of its original creed.”

As regards the imputation upon the motives of the hygienic officers of England, I have nothing to say; their character needs no vindication from me.

Now, I regard Dr. King as justified in rejecting the oral testimony of John Jamieson, an ignorant man, who could have known nothing of yellow fever. Such evidence is always unreliable.

Dr. Hume remarks that “the truth of a precept is rarely questioned; popular prejudices, errors and ignorance prevail in every department of

traced directly to the infected locality, or its immediate vicinity. Twenty-two are of doubtful or unknown origin; while in one instance, that of Matthias Pettigrew, the disease was contracted at the Lazaretto station, where he had been at work on board the ship *Caledonia Brander*, from New-Orleans. The *Caledonia Brander* had yellow fever on board when she arrived.

“One hundred and eighteen of all the cases reported were treated in private practice; twenty-four at the Pennsylvania Hospital; eighteen at the Blockley Hospital; seven at the Bush Hill, or City Hospital; and three at St. Joseph's, on Green Hill.

“In no instance can it be shown that the disease has spread from these labour-

knowledge, and truth is frequently suffocated in its birth, by the incumbent mass of error and superstition ;” and immediately afterwards he says, that “it has been maintained by the medical faculty of this city, since 1800, that yellow fever takes its origin in Charleston from causes existing within the same, under the influence of peculiar conditions of the atmosphere,” which opinions he himself entertained until 1853, when his prognostication by meteorological experiments failed, and when he got hold of the work of the late Dr. Strobel, many of whose statements have been contradicted and disproved. Now, does Dr. Hume mean to say, that in all that time the medical faculty of this city were under the influence of popular prejudices, errors and ignorance ? Such is surely the inference from his language. I had always supposed the reverse to be the case, and that physicians were antagonistic to such influences. I hardly dare quote Dr. Rush on this subject, for in another place Dr. Hume styles his writings “flimsy and imaginative arguments.” These are strong epithets, which should scarcely have been applied by any one to the writings of an authority so venerable in the profession as Dr. Rush. Of Dr. Moultrie, of whose thesis he speaks so lightly as the production of a young man, let me say, it was translated into several languages, and has been quoted and particularly noticed by very many able medical writers. The great and learned Dr. Ramsay, who says that “the disease was neither imported nor contagious,” comes under the same category. Will it be said that Bancroft and Chervin, and all the intellectual members of the profession who have expressed similar opinions, are “flimsy and imaginative ?” I trust that Dr. Hume will not coolly be prepared to subscribe to so sweeping a denunciation of the learning and intelligence of the profession.

But the Doctor seems to think that he has, as if by inspiration, cut the Gordian knot which the combined wisdom, research and minute investigation of medical gentlemen, with all the facilities afforded by their respective governments, were unable to untie in relation to the propagation of yellow fever and other epidemics—a pronouncement which

no embargo on vessels coming from Spain or France, from the Spanish and French West Indies, or from New-Orleans. And, besides, the war with Great Britain supervening, the embargo and restrictions on commerce occasioned by the war ceased in 1815, and not in 1816.

Dr. Hume has failed to discover the true source of the yellow fever in 1817, as well as in 1819, 1824, 1827, 1828, 1830 and 1835, but he gets over the difficulty by saying that no body took the trouble to enquire into the causes, and if they had, doubtless they would have discovered its contagious origin. This, however, is all assumption, and in an argument, just good—for nothing.

Dr. Hume furnishes us with the following reasons for his abandonment of his theory of meteorological and terrestrial causes—

“If we compare the meteorological details of individual years with each other, we will not find that all the healthy years are alike, nor are the unhealthy; nor are the healthy always unlike the unhealthy; 1845 and 1846 differ materially, yet they were both healthy; 1845 and 1852 differ but little, yet one was healthy and the other unhealthy; 1849 and 1853 are similar, except, meteorologically, 1853 should have been more productive of disease than 1849, yet 1849 was unhealthy, while 1853 was healthy. Such comparisons tend much to weaken our faith ‘in the influence of the peculiar (because unknown) condition of the atmosphere.’ It is evident that there must be some disturbing cause, occasionally introduced to produce the observed irregularity; and from the irregularity of that irregularity, the disturbing cause cannot be directed by a natural agent. When we compare 1849 with 1853, we see a strong resemblance; 1849 is alleged to have produced the fever, we presume, by its peculiar meteorological condition; if so, 1853 should have been equally, if not more, prolific in producing the same evil; but as it was not, the meteorological condition of 1849 must have been equally innocent, or the same conditions are capable of producing opposite results—a position not generally admitted by philosophers.”

Now, I ask, in all candour, if the same method of reasoning does not apply to the theory of importation? For how will the Doctor account for the exemption in some years, when every year commercial intercourse

But, says Dr. Hume, if the fever can be repelled by means of a quarantine such as has been pursued in Turkey, &c. &c., it should be done, and he contends that it were better that the whole commerce with Cuba and New-Orleans should be suspended for six months in the year, if our city could thus be saved from the irruption of yellow fever. But your *if*, says Shakspeare, is a great peacemaker. And it must be remembered, in choosing this alternative, that you cannot control Savannah, or Georgetown, or Wilmington, but might transfer all this trade to these respective ports; and, to carry out your views, you must necessarily intercept all intercourse with them by sea and by land.

On page 152 of the Journal, Dr. Hume remarks as follows:

"To assert the domestic origin of yellow fever in Charleston, requires as much positive information, as to assert its foreign origin; yet we have never been favoured with the exposition of the facts. We have had denials on the other side, that 'the transmission of the fever is neither tenable as a fact, nor in accordance with the opinions of a great majority of the medical profession in this country, and that it has its origin, not from contagion, derivable from cases imported, but from local and general causes.' From what observations, or from what facts, these results were drawn, we are yet to learn; and we rejoice to believe that we never will, for the Medical Society has abandoned the conclusions."

In relation to the last clause of this paragraph I will merely state, that I have examined the records of the Medical Society for the authority for such an assertion, and find the following to be the "plain, unvarnished" statement of the facts. At a meeting of that body, held on the 1st Dec., 1853—there being only fifteen present, out of upwards of sixty, and but few among them of the old and experienced members of the profession—certain questions in relation to the origin of yellow fever were proposed by Dr. Hume for the purpose of obtaining the individual answers of the members. The result was as follows: four members replied that they had reason to know or believe that the yellow fever of Charleston is always of domestic origin; ten, including Dr. Hume, replied that they had no reason for knowing or believing that fact. To the question "Have you reason to know or believe that it is ever of foreign importation?" six replied in the affirmative and

is ever the result of a foreign importation grafted on the impure air of the city.

I now leave it to the candid reader to determine, under such circumstances—the votes being the result, not of a deliberate investigation and discussion of the subject, but merely the expression of the amount of experience of each member on which he felt authorized to form an opinion; not whether the fact was so, but whether Drs. A and B had had an opportunity of knowing that it was so—whether Dr. Hume has been authorized to adopt so broad an assumption as that the Medical Society had abandoned the conclusions at which, after learned and minute investigation, it had formerly arrived.

In relation to the fever of 1838, Dr. Hume says that the Lord Glenelg arrived at Boyce's wharf, on the 4th July, with yellow fever on board; that the Medora lay at Magwood's wharf, one hundred and twenty yards from the Lord Glenelg, and that the first case of yellow fever was the wife of the steward of the Medora, who frequently visited her husband in that ship; but it is important to add that her case did not occur until one month after the arrival of the Lord Glenelg.

In the first place, we deny that the Lord Glenelg had any yellow fever on board, and denied it at the time. And next, we ask, is it reasonable or feasible, or in accordance with experience and observation, that the disease should have been taken by the wife of the steward of the Medora, one month after the arrival of the Lord Glenelg, she not visiting this vessel, the infection remaining dormant in her system, and attacking no one else, not even those who were loading and unloading the said-to-be infected vessel, but reserving all its virulence for this poor woman, whose nearest approach to it was at the distance of one hundred and twenty yards, and that only occasionally.

But in his anxiety to trace the disease to a foreign source, Dr. Hume forgets other more potent causes; for he says nothing of the extensive conflagration which occurred in Charleston in the spring of that year, destroying nearly one-third of the city, nor of the effects of the consequent exposure of a large mass of heterogeneous matter to the influence of the sun and rain, nor of the number of strangers who came to the city for the purpose of re-building the dwellings which had been consumed by the fire, crowded together in boarding houses deficient in

"Of the origin of yellow fever of 1839," says Dr. Hume, "we have more detailed accounts, for which we are indebted to the industry and zeal of the late Dr. B. B. Strobel, and is remarkable as the first instance of a rebellion against the established creed of our physicians, and a determination to investigate and publish the truth as it was seen."*

Of course Dr. Hume must have been aware of the existence of such a work, for he was one of the colleagues of Dr. Strobel in the Medical College of South-Carolina, and yet for fourteen years was this "truth" unnoticed by him, or, at least, treated with silence and indifference, until he had failed in his other investigations of the cause of yellow fever.

But I here enter my demurrer to this assertion, for it is well-known that many of the statements of Dr. Strobel were detected, as unfounded, not only here, but elsewhere; and surely Dr. Hume must be cognizant of the fact that a committee was appointed by the Medical Society of South-Carolina (and, I may add, at my request) to make enquiries whether the prevalence of the yellow fever in 1839 was to be attributed, as Dr. Strobel had contended, to the importation of cases by the ship *Burmah*. Neither can he be ignorant of my own statement on this subject, which, however, he has passed over in silence, because its views did not suit his purpose—something like the Virginia magistrate, who could not bear to hear both sides of a question, because it bothered him in making up his opinion.

From the report of the committee alluded to above, I make the following extract:

"By an examination instituted through reference to the captains and mates of the various vessels, whose information was given from the log books, your committee have ascertained that they had, at the time of their sailing from the different ports, viz: Liverpool, Boston and New-York, for this port, perfectly healthy crews, with sound cargoes, incapable, from their character, of generating foul air. That there was no malignant disease prevailing at the time of their departure; that their crews had not, while in this port, any communication, either direct or indirect, with the *Burmah* or her crew; and finally, that *that ship*, in all these instances, lay in the stream, from a quarter to half a mile from them, except for a short period, of which mention will hereafter be made."—(p. 5.)

Again: "Thus far your committee have satisfied themselves, that the transmission of the fever through the agency of the Burmah, is neither tenable as a fact, nor in accordance with the opinions of a great majority of the medical profession in this country."—(p. 8.)

And again: "Your committee, therefore, are of opinion, that the yellow fever which has prevailed, and still continues this season, has its origin, not from contagion derivable from those cases imported in the ship Burmah on the 6th of June last, but from local and general causes."—(p. 10.)

It may be remarked, that during this year many of the lots which had been exposed by the fire of 1838, remained unoccupied, and continued to present the same surface to the deleterious influences of the sun and rain, and many strangers also remained in the city, engaged in the occupation of re-building. The number of deaths this year were one hundred and fifty-three—all strangers, except eight native children.

The next period was in 1843, when the fever prevailed in a portion of the Neck, or upper wards, but did not extend to the city. The *Hayne* arrived at quarantine in June, and Dr. Hume says a man of the name of Hewes was sick on board of yellow fever. I remember boarding the *Hayne* and examining her, and was informed by Dr. Wurde-mann, a gentleman of high standing and character, that there was no yellow fever on board. Now, according to Dr. Wragg, an accurate and close observer and sound thinker, the yellow fever did not appear until the latter part of September, two months after the sickness of Hewes, and in all this interval, says Dr. Wragg, it was remarkably healthy. But let us see how Dr. Hume manages this case, which at the time was abandoned as a cause of fever.

"It may be urged," says he, "against the probability that Mr. Hewes introduced the fever into this region, that the length of time between the arrival of Mr. Hewes and the development of the epidemic was too long. But, according to Dr. Wragg, 'there were no fevers prevalent until the 10th to 13th of September.' There were literally no cases of bilious remittent and intermittent fevers previous to that time, but after the rains and hot sun, these fevers did prevail; and it is to be presumed that the yellow fever element, until then latent, gave its peculiar type to the fever. This is in strict accordance with the facts elsewhere ob-

the disease should commence in the opposite house. Such coincidences much resemble cause and effect, and are difficult to refer to general and local causes alone."

I have read many far-fetched notions among the ultra-contagionists, but nothing worse than this; and I may ask, in the name of common sense, how the contagious principle could have created, after a period of two months, a virulent epidemic, and yet that the disease should have been so circumscribed in its ravages, prevailing over only a small section of the Neck and not penetrating the city at all, notwithstanding the constant intercourse between the two places?

We now come to the year 1849. I will first quote the Doctor's statement and then add my own commentary—

"The fever of 1849 will next be considered, with reference to the facts of its origin. On the 18th July, 1849, the barque *Numa* arrived from Havana, and on the 6th August one of her crew (*Sauraboni*) was sent to the Marine Hospital, where he died that night of the black vomit. She lay at the Union wharves, foot of Pinckney-street. The *Isabel* and Merchant, both from Havana, arrived on the 25th July and 25th August, and lay also at Union wharves. The engineer of the *Isabel* was attacked on the 7th August, proving infection on board of these two ships, at least. On the 20th August the British barque *Queen Victoria* hauled into Patton's wharf, next above Union, from Hull, England, sound and pure, and the *Huron*, from a British port, lay in the dock below: thus the pure and the impure vessels were all in the vicinity of each other. On the 20th or the 21st of August, the Captain of the *Huron* was attacked with fever, but recovered. On the 28th, Capt. Laycock, of the *Queen Victoria*, was attacked with yellow fever, and died of black vomit on the 1st September; five of his crew were also attacked, but recovered. Thus we have another example of what occurred in the stream in 1839, and must be attributed to the extension of the infection from one vessel to another. If we proceed up the streets leading to these wharves, we find a case, on the 3rd of September, at the corner of East Bay and Hasell-street (*Betts*); another on the 7th, east side of East Bay, one door above Hasell street (*Carrol*); and on the 10th, one at *Bennett's* Mill, foot of Wentworth-street, two wharves above Patton's (*Drew*); one corner of Anson and Market streets, on 9th (*Behlin*); and one in *Linguard-street*, on 10th (*Collins*)."

Now this does indeed furnish the strongest appearance of the contagious propagation of the disease, of any which he has related. But he has omitted to state, that an extensive excavation for a drain, of the foulest materials, was opened on East Bay, from Hasell-street to Patton's wharf; that the same offensive materials were thrown up opposite to Union wharf, south, to form the foundation of the new custom house—two powerful sources of pestilential air, under the influence of which

the contractor for laying the foundation of the custom house was attacked with yellow fever. He has also omitted to state that the same offensive stuff was deposited in King-street, from Horlbeck's-alley to Hasell-street, and that in this portion of King-street there were many respectable persons who sickened and died; while there were none of that class affected in any other part of the street. He forgets, too, that the persons employed in these excavations were Irishmen, who are peculiarly susceptible to the disease, and were, in fact, its greatest victims. I will presently give statistics showing how few seamen, in proportion to other classes, died of this disease. The Doctor does not recollect, further, that so satisfied were the city authorities that this was the active cause of the disease, that all works requiring the excavation of earth in summer, such as laying gas pipes or opening and cleansing drains, were prohibited in 1853, and the period of elections changed—another exciting cause—during which year the yellow fever did not prevail.

I now quote the following extraordinary passage:

"That Charleston is beyond the northern limit of self-generating epidemic yellow fever, is exceedingly probable; for it is the property of epidemics never to die in the places of their nativity. Yellow fever does die in our climate, for it is not renewed each successive summer; while at the Havana it flourishes in perpetuity. If the fever is not a native of the West Indies, it is, at least, naturalized; but it cannot be said to be naturalized in Charleston. Our winters extinguish it effectually. Charleston does occasionally produce sporadic cases of fever of domestic origin—but they do not extend, nor infect localities. In 1847, a drummer at the Citadel Academy went on a drunken frolic to Sullivan's Island, returned to Elliott-street in the city, and when he became a little sober, reported at his quarters at the Citadel. In a few days he was taken with all the symptoms of yellow fever, even unto black vomit; he neither died, nor did he extend his disease to the numerous subjects in his immediate vicinity. Had he obtained his disease from an infected West Indian vessel, the result and consequences would have been very different. We apprehend that it is from such cases as this that the elder physicians founded their belief of the domestic origin of yellow fever, and inferred, if it was seen occasionally to be of domestic origin, it must necessarily always be so, without distinguishing the remarkable difference which exists between the sporadic and epidemic."

Now, his first assumption is gratuitous. Yellow fever is, in warm climates, endemic in the summer, as typhus fever, in cold climates, is in the winter; and the Dr. may as well say that bilious remittent fever is beyond the northern limit, as it prevails in the country in the summer and fall, but does not in winter. Thus, the effect of country malaria

is bilious remittent fever; the other, of city malaria—each requiring altitude of temperature for their production.

But again. Was it ever before heard that yellow fever could in one place be contagious and in another non-contagious, any more than small pox or measles? Or was there ever uttered a more singular and bold assumption, or more extraordinary inference, than that our medical predecessors were misled by a puerile mode of reasoning in relation to the domestic origin of yellow fever? Now, although the whole series of Dr. Hume's remarks are nothing more than bare assumptions and illogical conclusions, this portion is so peculiarly bold and original as to excite at least astonishment.

The Doctor, after giving the history of the periods in which he thought he could prove the importation of yellow fever, says: "Having assumed an hypothesis adequate to explain all the observed phenomena connected with the origin and propagation of the yellow fever in Charleston, we are prepared to apply it to the prevention of the evil."

Now it is easy to assume an hypothesis, and if granted, to prove any thing by it, (and unfortunately the Doctor has too often reversed the Baconian system of reasoning by induction), but I have always thought that such a method of reasoning has been deemed illogical. It seems, however, that whenever the Doctor wishes to establish a favourite scheme or notion, he lays aside all the ordinary rules which govern other men in similar investigations.

It is with great reluctance, and, I may add, with deep regret, that I have been forced to reply to Dr. Hume. But I have felt it to be my duty, as the principal officer to whom the external and internal medical police of the city has been entrusted,* to reply to his erroneous statements, and to expose the dogmatic, unfair and impartial manner in which he has investigated the subject, because against the administration of my office must many of his denunciatory charges be supposed to be directed. As to the plan he proposes, of more stringent quarantine regulations, I offer no comments further than this: that if such measures are to be adopted, the authorities must furnish a proper lazaretto for such a purpose, where passengers may comfortably remain and be well taken care of, with commodious hospitals, and extensive warehouses for the storage of cargoes. Without these arrangements, all his

* In 1821 I was appointed, by the Governor, Port Physician, and have held the office ever since; and have, since 18—, acted either as Chairman *pro. tem.*, Chairman or Medical Adviser of the Board of Health. I resigned as Chairman in 1850, finding that the duty, at my time of life, infringed too much upon my other duties; and my services, such as they were, were entirely gratuitous.

plans will be unavailing, inhumane and ruinous. If it is desired to imitate New-York in rigidity of quarantine, we must imitate her in providing the proper means of carrying out that quarantine, regardless of expense.

In conclusion, I will only add that I have always thought and so expressed myself, that due precautionary measures should be adopted, and have, to the best of my abilities, carried out these views. But I have never been ultra in my opinions, either in medicine or its kindred branches of science, nor in politics or religion. I have seen much sorrow and much evil result from ultraism, and I am convinced that it is only by a judicious and firm conservatism that the baneful effects of such ultraism, in every department of life, can be averted.

Hence, while I have not hesitated plainly to express my opinions as regards hygienic measures, I have always been willing to put into execution such regulations as the public authorities in their wisdom may think proper. I conclude with quoting from a criticism of Dr. Strobel's work, in the *American Journal of Medical Sciences*, written by Dr. Condie, remarkable for his accuracy as an observer and admirable in his treatises on some epidemics—

"If the doctrine of the transmissibility of yellow fever be well founded, the sooner the public are convinced of the fact, the better; but if it is false, every effort should be made to destroy the baneful influence which a belief in it has invariably exerted whenever and wherever the disease has appeared—closing the ears to the cries of distress—sealing the heart against the calls of humanity, and causing, too often, the abandonment of the sick and the dying to the care of strangers, or to suffer and expire in gloomy, cheerless solitude.

' Dependents, friends, relations, love itself,
Forget the tender tie——
The sweet endearments of the feeling heart.'

Thus adding additional horrors to those produced by the ravages of the pestilence, and increasing, to no trifling extent, the number of its victims."

tenate subjects of this disease have often exhausted their purses by buying nostrums, which were, and are still, published in the newspapers, offering them certain cures. But too often has it been the case that the remedies of both the regular physician, and the advertising quack, have failed to do any good, the suffering man remaining weaker in both purse and body.

Having recently had a case of this disease under my charge, I feel myself called upon to make known to the profession my convictions that a certain remedy has been discovered, and that the disease in question need not be longer regarded as a reproach to us. The remedy is a purely mechanical one, but most effective; and I predict that so soon as the efficacy of the remedy becomes generally known, the disorder will be regarded by all as a very trifling complaint, and that physicians will seldom, if ever, be called upon to prescribe for the complaint. I allude to the *Spermatorrhœa* Rings lately used by the Boston physicians.

CASE.—A young man of this district consulted me last fall, requesting a prescription for the relief of *Spermatorrhœa*. He represented the discharges of semen as occurring repeatedly every night; that he would feel much exhausted every morning; that his health, as was very evident, had become very much impaired, and that his disorder occupied his thoughts so much as to render him almost incompetent to attend to any kind of business. The complaint had been of more than a year's standing. He had become dyspeptic, and frequently found it necessary to take medicine to procure alvine evacuations. He was, evidently, in a bad way. I prescribed for him active tonics, with the use of the cold bath at night, just before bed time. This treatment did not relieve at all, and he urgently requested permission to try "*Morse's Invigorating Cordial*," a nostrum holding forth many inducements. I advised him to try the *Spermatorrhœa* Ring, and gave him to read such statements as had, up to that time, appeared in the Medical Journals. To this trial he was not inclined, having got his mind bent upon a trial of *Morse's* nostrum; but, consented, that if the "*cordial*" failed, he would try the ring.

The "*Morse's Invigorating Cordial*," after the use of half a dozen bottles, had failed to make any change in his condition, and he was then prepared to yield to any treatment that I would advise. I, accordingly, wrote to Mr. E. H. Perkins, of Baltimore, who was at that time advertising the rings, to forward me, by mail, one of the rings. He did so very promptly, and the trial of this new remedy was at once made by my patient with many doubts and misgivings as to success. In fact, he had completely despaired of ever getting relieved.

The first week of its use he could scarcely sleep at all, being awakened every few minutes by the teeth of the ring piercing his flesh; sometimes waking in a fright, dreaming that some serpent had seized him by the organ. The interruption to the seminal discharges, however, was almost complete the first week; and, from that time (Decem-

ber) till now, he has been constantly wearing the ring at nights, with a complete exemption from the disorder. His general health has improved every day, and he now considers himself cured.

I have advised him to wear the ring a while longer, in order that his health may be completely re-established.

This is the only case in which I have given the ring a trial. Its decided success induces me to look upon it as the only sure and unfailing remedy for the distressing complaint it was intended to relieve.

REVIEWS.

ART. VI.—*Hallucinations; or the rational history of apparitions, visions, dreams, ecstacy, magnetism, and somnambulism.* By A. BRIERRE DE BOISMONT, Docteur en médecine de la faculté de Paris, Directeur d'un établissement d'aliénés, &c., &c., &c. First American from the second enlarged and improved Paris edition. Philadelphia: Lindsay & Blakiston. Pp. 553.

THE variety of criticism which the first edition of M. de Boismont's work called forth—on the one hand, sharp denunciation, on the other, almost unlimited praise—is of itself sufficient evidence of the novelty of the theory he advanced. Without either accepting or rejecting his doctrines, which, it must be acknowledged, bear the semblance of truth, but which require for their complete establishment careful investigation, and the observation of new facts, we propose laying before our readers a *résumé* of the points which he believes that he has established.

In making this analysis, it will be impossible to do more than state the conclusions at which the author has arrived, as it would require far more space than is allotted to a review to examine, in detail, the numerous facts and cases which are brought forward to illustrate his views, and without which it would be impossible to state his argument fairly.

Hallucinations are of two kinds; the one compatible with reason, the other stamped with the character of alienation. To the former class belong the visions and revelations which appeared as realities, to men of otherwise strong sound minds and clear reasoning powers, but whose whole faculties were strongly concentrated upon a single subject; such men, for example, as Luther and Swedenborg; the previsions of seers, mesmerists, clairvoyants, &c.; and the mystic apparition of ecstasies.

Hallucination is defined to be "the perception of the sensible signs of an idea; and illusion the false appreciation of real sensations." The former are subjective, and have their origin in the mind of the individual, as is proved by hallucinations of sight in the blind, and of hearing in the deaf; the latter are objective, and are false appreciations of real objects.

The classification of hallucinations is beset with difficulties, from their frequent complications with mania, monomania, dementia and illusions. M. de Boismont divides them into ten classes, the distinction between which will appear as we proceed.

Hallucinations with reason.—Hallucinations of sight and hearing can be normally produced: the eye, for example, if directed to a bright light, will see the same object if suddenly turned to obscurity. Memory, aided by imagination, is capable of reproducing before the senses, scenes or sounds that have been vividly impressed. Reverie, by the intense concentration of the nervous system, is capable of displaying to the senses of the individual the object which has occupied his thoughts. When these two conditions concur in individuals whose faith in the supernatural is great, the transition to visions and apparitions is easy. But these differ from those of the insane in the fact that they do not influence the conduct of the individual. The hallucination may be regarded as a deception of the senses, or accepted as a reality; the actions of the individual do not, however, depart from the common track. These kinds of hallucinations may sometimes be produced at will; they sometimes result from derangement of one or more important functions. Upon this hypothesis may be explained the words and acts of celebrated men accused of insanity.

Hallucinations of insanity in its simple state.—These differ from the preceding, in the fact that, in the immense majority of cases, the actions of the madman are influenced by his morbid sensations, so convinced is he of their reality; whereas, in hallucinations with reason, the error, even if unacknowledged, exercises no influence on the conduct of the individual. The hallucination of the insane may be of one sense only, or of all. Hallucination of all the senses is rare; of one, two, or three, is most common. Hallucinations of hearing come first in frequency, next those of sight and touch. Hallucinations of taste and smell are least frequent of all; they are, moreover, scarcely ever isolated, and are usually associated with illusions. The hallucinations of insanity, either of a single sense or more, may explain many acts of lunatics, hitherto incomprehensible.

Hallucinations as connected with illusions.—The characteristic difference of illusions and hallucinations, is the presence of an exterior body in the former, its absence in the latter. The man who mistakes one object for another, or one individual for some other person or thing, is a visionary; he who hears voices in the profound silence of night, or sees objects in utter darkness, is hallucinated. In either case the brain is the organ at fault; but in the former the brain perceives falsely a real object, in the latter, on the contrary, the perception exists in the brain, of an object which has no real existence. In the sane, these illusions of the senses are corrected by indubitable experiences and observation, and have

thinks this proportion too large, but believes that it exceeds fifty per cent. Monomania presents this complication most frequently; next in order follow hypomania, demonomania, erotomania, nostalgia and calentura. Hallucinations of hearing and of sight are most common; then follow those of touch, and taste, and smell. In monomania, illusions frequently accompany hallucinations.

Hallucinations in stupor.—"There are some lunatics who appear to comprehend nothing that is passing around them; with fixed eyes and gaping mouths they might be taken for idiots." This form of mania has received the name of stupor. M. de Boismont has cited a variety of cases to prove that these unfortunates are the prey of most distressing hallucinations and illusions. They see sights and hear sounds which, invisible and inaudible to others, form for them an imaginary world, while all surrounding objects are transformed by the illusions of their senses into entire correspondence with their imaginary existence. The conduct, actions, and whimsicalities of many apparently insensible, immovable maniacs, may be by such hallucinations and illusions.

Hallucinations in mania.—M. de Boismont believes hallucinations and illusions to be more common in mania than in other forms of insanity. The cases cited in this chapter would appear to prove that hallucinations and illusions may be the causes of mania, which then takes their place. In maniacs, hallucinations and illusions account for many of their singular acts, and may lead to actions of a dangerous or injurious character.

Hallucinations in dementia.—In dementia, when the intellectual faculties have already begun to fail, and in many cases where their obliteration has been nearly complete, the unfortunate victims are still subject to occasional hallucinations, which may for a time arouse them from their state of imbecility, and produce paroxysms of maniacal fury. Even in imbeciles and idiots, where all the faculties are not obliterated, hallucinations are possible, and do sometimes occur.

Hallucinations in delirium tremens.—These are unfortunately so well known on this side of the Atlantic, that no effort is necessary to prove them.

Having established the compatibility of hallucinations with reason in many instances, and their frequent co-existence with all the various forms of insanity in which the intellectual faculties are not entirely lost, it remains to enquire how frequently hallucinations may complicate nervous, febrile and other diseases, and how far the existence of such complication may serve to explain many of the phenomena attendant upon these affections.

Hallucinations in nervous diseases.—In catalepsy, hallucinations are rare, on account of the suspension of the intellectual faculties; examples of dreams and visions in this state are, however, mentioned. In epilepsy, hallucinations are much more common, and especially where this disease is complicated with insanity. The hallucination of epileptics are of a sad or alarming character, and may become the causes of dangerous, even fatal accidents. In the hysteria and hypochondriasis, also, hallucinations frequently occur; in both these affections the hallucinations may occur in a state of sanity, or be combined with mania, monomania and dementia. "The combinations of catalepsy, epilepsy,

hysteria, and hypochondria with monomania and different forms of insanity, their transition from one form to the other, and their re-union, by establishing numerous connections between these various affections, give a reason why hallucinations are so often observable." In chorea, chlorosis, rabies, and several other nervous affections, rare examples of hallucinations have been met with.

Hallucinations in nightmare and dreams.—As nightmare presents some analogy to insanity, it is not surprising that it should be combined with hallucinations; these may occur at all ages, from childhood to advanced life, and are sometimes intermittent. As in madness, so in nightmare, the individual may be conscious that the phenomena are not real. The hallucinations of nightmare usually cease on awaking: but if continued during waking hours they are taken for realities. During sleep, the operations of the mind being no longer under the control of the will, all ideas then formed are received as realities. The hallucinations of dreams are only the reproductions of past reminiscences, or may be traced to association of ideas.

Hallucinations in ecstasy, magnetism, somnambulism.—Ecstasy, being the result of the highest degree of exaltation of mind, is eminently favourable to the production of hallucinations; it frequently manifests itself in contemplative minds, and is also observed in religious persons, of even ordinary intellect. Consequently, during periods of fanaticism or of great excitements, the development of ecstasy is very frequent. Ecstasy may be manifested without any loss of reason, or may be combined with a disordered mind; the line of demarcation is sometimes difficult to distinguish. Ecstasy has been seen in children, in cataleptic hysteria, in mania and monomania; it has also taken the form of an epidemic. The phenomena of prevision, clairvoyance, second sight, magnetism and somnambulism, can only be explained by their affinity to ecstasy, of which they are only varieties.

Hallucinations in febrile and other maladies.—Hallucinations may be combined with all diseases; they sometimes precede the disease, and may exist in three degrees; in the first degree reason remains intact; in the second it is overthrown; in the third totally destroyed. "The principal diseases in which hallucinations have been observed, are inflammatory fevers, congestions, inflammation and diseases of the brain and its membranes, inflammations of the lungs, lesions of the digestive organs, typhoid and intermittent fevers, gout, hectic diseases, &c."

cation, the prevailing ideas of the period, and the state of society, must hold an important place. The moral causes which have exercised the greatest influence on hallucinations, have been a belief in the power and co-operation of spirits and demons, sorcery, magic, lycanthropy; visions of souls of the departed in purgatory asking prayers for their extrication; visions of spirits who make revelations, who announce an approaching death, or return to fulfil a compact, made during life; vampirism, ecstasies, &c. Remorse, strong vivid passions, intense pre-occupation and fixedness of ideas, are also the frequent sources of hallucinations.

The physical causes of hallucinations may be divided into five classes. In the first are comprised climate and locality, sex, temperament, profession, season. To the second belong mechanical causes, as alcoholic and fermented drinks; certain poisons, as opium, stramonium, belladonna, Indian hemp, and certain gaseous and narcotic substances. The third class comprehend hallucinations coincident with insanity, whether preceding or succeeding it, or occurring during its course. To the fourth belong the hallucinations which occur in connection with catalepsy, hysteria, hypochondria, rabies, nightmare, sleep, and ecstasy. The two last of these causes belong to this category, because the hallucinations of sleep are often the commencement of disease, while those of ecstasy often result in pathological conditions, although in the beginning they may have been the result of moral causes. Under the fifth head may be ranked the hallucinations observable in acute, chronic, and other diseases; as, for example, those occurring in acute delirium, diseases of the brain, typhoid and intermittent fevers, gout, chlorosis, and the last stage of chronic diseases; in prolonged abstinence, syncope, and during convalescence.

Hallucinations considered in a physiological, historical and religious point of view.—This chapter is the most unsatisfactory of the work, and from the number and variety of facts and observations from which the deductions are drawn, it is impossible to make any analysis which would be comprehensible to our readers, within the limits which we have to spare. Not satisfied with the application of his views to acknowledged facts, the author seeks to explain the extraordinary acts and opinions of celebrated men, from the days of Socrates to the present time, and the apparitions, miracles, &c., of the Roman Church, by reference to his doctrine of hallucinations. In this we believe that he has done injustice to his own cause, and prejudiced the mind of the reflecting reader against it. Many of the facts, to which he refers, are far from authentic; while those which are, have not been described with the minuteness of detail, necessary to satisfy the philosophical enquirer as to the positive mental condition of the individuals concerned. It is impossible, from the time which has elapsed since the occurrence of almost all the facts referred to in the chapter, to determine how much of the extraordinary was due to insanity, how much to hallucination, compatible with reason. The whole chapter would do credit to a special pleader, but is out of place in a philosophical work.

Physiology and symptomatology of illusions.—Are hallucinations purely intellectual, or do they require the intervention of the senses? M. M. Baillarger, Burdack and Müller, have supported this view of

the question, while M. de Boismont maintains that the intervention of the senses is not essential, as is proven by the occurrence of hallucinations of hearing in the deaf, and of sight in the blind. He nevertheless retains the classification of hallucinations advocated by M. Baillarger, viz: into psychical, and psycho-sensorial, as useful in the classification of the phenomena—the degree of intensity being the distinction between them.

The intellectual phenomena of hallucinations of hearing, are different kinds of noises, the reproduction of the habitual pre-occupation of mind of the individual, or the memory of preceding conversation, reading, &c. The hallucinations of hearing are frequently ascribed to invisible beings, who speak in the second or third persons. They are not, in general, under the control of the will. The sensorial phenomena of hallucinations of hearing are characterized by sounds, more or less solemn, murmurings, whisperings and voices, which make revelations from the other world, &c., &c. Hallucinations of hearing are most frequent in the insane, while those of sight are most usual in persons of sound mind.

A remarkable feature attending the intellectual phenomena of hallucinations of sight, is the reproduction, during an indefinite period, of a single object always the same, which afterwards assumes a great variety of changes. Visions in the hallucinated, often bear an intimate relation to the actual pre-occupation of mind of the patient, or they are the reproduction of vivid anterior sensations. They generally partake of the nature of the habitual ideas and occupations of the individual. The image is generally entire, sometimes only partial, which difference depends probably on the intensity of the original idea; if the impression was strong, the whole image is reproduced; a part only, if it was weak.

The sensorial phenomena of hallucinations of sight are more vivid than the intellectual; the images are seen and described by the sufferers with great clearness and precision. Visual creations of the hallucinated undergo motion, and changes of form and dimension; they appear and disappear, enlarge enormously, or fade gradually away; they may appear suddenly, disappear as rapidly, or last an indefinite time. Night, darkness and silence, are favourable to their production; although certain individuals have them indifferently, by day or night. They sometimes disappear when the eyes are closed, or the reverse: in some patients, they are only seen during the day, and disappear at night.

Hallucinations of sight are not as frequent in the blind as are hallucinations of hearing in the deaf.

tion, his conviction is so profound that nothing can convince him that it is a sensorial error; and this explains the indomitable constancy of sorcerers in the midst of tortures. Hallucinations may occur in all; they have been noticed in the feeblest, as well as the strongest minds. The effect of hallucinations is to produce profound alterations of both the circulatory and digestive functions, to pervert the secretions, and destroy sleep. The transitions from sleep to waking, and from waking to sleep—evening and night—are the states most favourable to the production of hallucinations. In dreams, the train of thought may be carried out which was commenced during waking. In some insane, the conduct of the day is regulated by the hallucinations of the nightly dreams. The hallucinations of somnambulism resemble those of dreams; they may occasionally result in very dangerous acts. Illusions may occasion all the sensorial and intellectual phenomena of hallucinations; their essential character is, the transformation of external bodies into fanciful conceptions. The false sexual impressions of many of the insane, is to be ascribed to hallucinations of touch. The involuntary exercise of memory and imagination, the suspension of external impressions, and the internal excitation of the sensorial organs, are the conditions which appear to exert the greatest influence on the productions of hallucinations.

Pathological anatomy.—As in sanity, so in hallucinations, no positive lesions have as yet been discovered, which can be considered as peculiar.

Progress, duration, diagnosis, prognosis.—Hallucinations may be remittent, intermittent, irregular, periodical, diurnal or nocturnal. Many causes may diminish their intensity, or suspend them. Their progress varies according to the form and character of the insanity; which, likewise, influences their duration. This last is variable; some last only a few hours, others during life.

The diagnosis of hallucinations is easily made, when patients communicate freely their sensations; in doubtful cases, the conversations, actions and writings, must be observed and compared.

The prognosis of simple hallucinations is more favourable than that of general hallucinations. Hallucinations compatible with reason are easily cured. Hallucinations which appear suddenly—those of febrile delirium, of narcotic substances, and of delirium tremens—are of slight importance, and cease with their causes. Hallucinations in monomania of a lively character, and of recent occurrence, add but little to the intensity of the attack; it is far otherwise in insanity of melancholy or mournful character. Hallucinations of nervous diseases, as hypochon-

cases. Hallucinations are sometimes cured by violent physical or moral shocks, sometimes by the efforts of nature alone.

Hallucinations considered in relation to medical jurisprudence.—Hallucinations alone, or combined with illusions, may be the cause of many dangerous or criminal acts; as, for example, suicide, murder, theft, incendiarism, and violent assaults. Such acts frequently occur quite suddenly, and are occasioned by the sight of imaginary enemies, by insults, grimaces and injuries, which only exist in the imaginations of the hallucinated. In some cases, suicide and murder are commanded by inaudible voices. Night, darkness and solitude, the transition from sleeping to waking, and the reverse, the condition of sleep, appear to favour the disposition. In the greater number of cases, an attentive examination will leave no doubt of the reality of an hallucination which has suddenly appeared: in doubtful cases, prolonged confinement, with sedulous watching, must be resorted to. Illusions produce the same reprehensible actions as hallucinations; with this difference only, that in hallucinations the imaginary object has no real existence, while in illusions, familiar objects are converted by the imagination into subjects of hate or terror.

We have thus endeavoured, without comment or discussion, to lay before our readers, as clearly as the necessary condensation would permit, M. de Boismont's theory of hallucinations. It is possible, even probable, that we may have failed in conveying to others a very precise or definite idea of his views. If this be so, it must be attributed to the fact, that his deductions are drawn from a large number of cases and observations, without a knowledge of which it is extremely difficult to understand the foundations of his doctrines. Our space is too restricted to permit the extraction of a sufficient number of these cases to render his conclusions more intelligible. To those who are sufficiently interested by this article, to desire more information on this subject, and to all who have any dealings with the insane, we cordially recommend the work. If the theory is not true, it at least throws much light on many disputed points of intellectual psychology.

ART. VII.—1. *Homœopathy: Its Tenets and Tendencies, etc.* By JAMES Y. SIMPSON, M. D., F. R. S. E., Professor of Midwifery in the University of Edinburgh, &c. &c. First American, from the third Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8vo. Pp. 304.

2. *Homœopathy Fairly Represented: A Reply to Professor Simpson's "Homœopathy" Misrepresented.* By WILLIAM HENDERSON, M. D., Professor of General Pathology in the University of Edinburgh. First American, from the last Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8vo. Pp. 302.

THE doctrines of Homœopathy, although founded upon the grossest errors and superstitions, inconsistent with every law of the human understanding, and contrary to all experience, have, for a time at least,

found a foot-hold among the most enlightened nations of the earth. It has been erroneously supposed that this system was only a new method of curing diseases, affecting only practical medicine; but Hahnemann and his followers state that Homœopathy is not only independent of, and incompatible with, all other medical theories, but also declare that "all the learned lumber, which theorists have been accumulating for two thousand years, must fall to the ground;" and that it is not necessary to be acquainted with any thing but the symptoms of natural diseases, and those artificially produced in healthy persons by drugs, to perform the greatest, safest and most durable cures.

We will endeavour to prove that such doctrines, if adopted in their fullest extent, would shake the very foundation of the human intellect; and all experience in the whole range of natural philosophy, not excepting even that which admits of mathematical demonstration, would be swept away, or rendered utterly nugatory.

From the facility with which the requisites to practice this art might be acquired, it is not a matter of surprise that many, too ignorant to succeed in the practice of rational medicine, or too indolent to devote themselves to scientific studies, eagerly embrace it from motives of personal interest. And the public having, at best, a superstitious idea of medicine, are easily induced to adopt any novelty or absurdity; there is for them nothing too trifling, not even Homœopathy; nothing too violent, not even the feats of Priessnitz; nothing too absurd, from the royal touch or that of a hanged man's hand, down to Perkinism, Mesmerism or Spiritual Rappings.*

From the advantages which would have accrued from the general adoption of Homœopathy, it is but reasonable to suppose that the European Governments allowed it a fair trial. Could it have stood the test of experience, it would have been their policy to have promoted its extension by every means in their power. "Willing to gratify the public voice," says Dr. Leo Wolf, "whenever expressing itself in favour of objects unconnected with politics, Homœopathy would have offered to them the best means to check the progress of the human intellect. For what could be better calculated to counteract and eradicate all reasonable or extravagant excitements for unrestricted religious or political liberty, than credulity and superstition, particularly in regard to objects of such general and great importance as health and life?"

Another weighty consideration would have been the immense diminution of expenditure which would have resulted from the abolition of the ancient course of instruction in the medical schools. The great number of professors would no longer have been of any use, since the successful treatment of disease depended only on a knowledge of the properties of drugs and the symptoms of natural disease, which every one could study for himself. In time of war the whole class of military physicians could have been dispensed with, and their places supplied by a few homœopathic surgeons, with their retinue of symptom recorders, &c. And the whole pharmaceutical apparatus and assortment of drugs, could have been conveniently comprised in a portable box of homœopathic dimensions.

* Tenets, p. 49.

The Prussian Government ordered a public trial of Homœopathy, to be made at the Charité, the largest hospital of Berlin, by one of Hahnemann's disciples, particularly recommended by him for that purpose. From the many hundred cases of disease in that hospital, he selected twenty-five, which he considered most suitable for his experiments; but *not one* was cured. The experiments instituted in Vienna, were attended with no better results. Dr. Herrmann, one of the oldest of Hahnemann's disciples, who was sent to Russia to test the efficacy of the homœopathic treatment, was so unsuccessful that he was dismissed, and the homœopathic practice prohibited in all the Russian military hospitals. "For the sake of experiment, an equal number of patients, quite similar to those under the care of Dr. Herrmann, were, at the same time, treated by Dr. Giggler, surgeon general of the Russian army, by mere diet, without administering any medicine, and the result was the same as that obtained by the homœopathic treatment."*

The authors of the works at the head of this article, are at direct issue upon the results of the homœopathic treatment at Fleischman's hospital, in Vienna. Dr. Simpson, we think, clearly proves that the results of pneumonia were not more favourable, under the homœopathic treatment, than in Dietsch's hospital, where no medical treatment was instituted. Dr. Henderson, ignoring the facts presented by Dr. Simpson, boldly claims a greater success for Homœopathy in pneumonia, than for the Allopathic treatment, in the various hospitals in Great Britain, or on the Continent. Indeed, throughout his book, Dr. Henderson overlooks facts and arguments, and in a cloud of words would obscure the truth, and force his own opinions upon his readers.

We have never felt more difficulty in fixing our attention upon a book than this. The author's ill humour is throughout apparent; and he would have the reader sympathize with his excited feelings. He labours to justify himself, by attributing every species of unworthy motives to his opponents; at which he fails, as he never recovers his self-possession, or appears satisfied with his efforts of refutation. The most conclusive arguments and self-evident facts, presented by his opponents, are passed over in silence; while some unimportant statement or illustration is seized upon, to show some apparent irrelevancy or inaccuracy. In his preface, our countryman, Dr. Horace Green, is mercilessly ridiculed. His suggestive patronymic is tauntingly held forth, as a subject of derision; and even his professional title is denied him.

Slender must the resources of that system be, when its supporters meet facts with abuse, and arguments with paradox and angry declamation.

In striking contrast, Dr. Simpson coolly surveys his antagonists, while he administers retributive castigation. His manner, though severe, is never disrespectful or unnecessarily harsh. He has reviewed every important point of the homœopathic doctrine, exposed its absurd-

referring to the books at the head of this article, or any other which may furnish the materials we require.

For upwards of thirty years the author of Homœopathy wandered about Germany, in search of practice; failing of success, he professed to be dissatisfied with the uncertainty and general inefficiency of the ordinary mode of practicing medicine, and resorted to translating English and French works into German, for the booksellers, as a means of earning a livelihood.

His biographers represent him as a martyr for truth; and proclaim him the greatest philosopher, the first medical genius of every age, and almost a saint. They do not mention that, in the early part of his career, he was a seller of secret nostrums. Dr. Wm. Leo. Wolf, in his remarks on Homœopathy, tells us that "about the year 1800, Hahnemann advertised a new salt, which he claimed to have discovered, and which he offered for sale, under the name of *alkali pneum*, at the moderate price of one louis d'or (\$4.00) per ounce. The Society for the Promotion of Natural Sciences, in Berlin, desirous of becoming acquainted with this new substance, had it analysed by some of the most experienced chemists, who pronounced it to be nothing but *common borax*. He shortly afterwards advertised an 'infallible preventive of scarlet fever;' but being disappointed in its sale, he afterwards confessed it to consist of nothing but a few grains of the extract of belladonna dissolved in water." These transactions prove Hahnemann dishonest; and consequently we cannot confide in his veracity on questions of the greatest importance, unless supported by the most unquestionable evidence.

It was in the year 1790, whilst translating Cullen's *Materia Medica*, that his attention was attracted to the *modus operandi* of Cinchona bark. He dosed himself for several consecutive days with this substance, and "was attacked with shivering and other symptoms of *intermittent fever*. He discontinued the use of the drug, and the symptoms disappeared. He tried it many times, and invariably with the same or similar results. Now observe 'FACT THE FIRST.' Cinchona gives a healthy man an intermittent fever, and Cinchona cures intermittent fever in the sick."—*Mr. Everest's letter to Dr. Cormack*, 1851.

Dr. Simpson justly attributes the symptoms, experienced by Hahnemann, to the preconceived hypothesis he had formed; and adds, that the thousands of pounds of cinchona and its active principle, which are yearly administered in Great Britain, to persons (in comparative health), as a tonic or otherwise, fail to produce ague or any thing like it. The number of observations on the effects of cinchona, are amply sufficient to establish the general fact, that cinchona cures ague and intermittent

allopathic authority, that cinchona does produce ague. And adds, "an ague, when its complement of leading symptoms is complete, consists of *shiverings*, of *febrile heat*, of *sweatings*, all of which symptoms, and many more, that resemble those of *particular cases* of ague, can be produced by cinchona." The allopathic authorities, Pereira, Guersant and Christison, whom he quotes, certainly never intended to convey the meaning which Dr. Henderson has drawn from their remarks.

Pereira says, cinchona, in large doses, produces "a febrile state, manifested by the *excitement of the vascular system*, and dry tongue;" Guersant observes, that the sulphate of quinine "excites a true *febrile movement*, of more or less duration, accompanied with *shiverings*, faintness, *cold sweats* and agitation;" and Christison says, that cinchona "is apt to excite nausea, pain in the stomach, and *febrile symptoms*."

These physiological symptoms—selected by Dr. Henderson to show that these authorities admit that cinchona produces ague—(intermittent fever,) might be produced by many other articles of the *Materia Medica*. Any stimulant will produce *vascular excitement*; but will it also *cure fever*?

There is a certain order in the symptoms of ague, which Dr. Henderson has properly collocated above; but the authors quoted do not so arrange the physiological effects of cinchona; so, however loosely their expressions are, they cannot, we think, be made to represent the symptoms of ague or intermittent fever.

The *periodic* recurrence of the symptoms of intermittent fever, which constitutes its characteristic feature, has escaped the notice of the writers on both sides of the question. So, even could it be proved that the physiological effects of cinchona are similar to the symptoms of a *paroxysm* of intermittent fever, still, as *periodicity*, in their occurrence, has never been observed, cinchona fails to substantiate the homœopathic law, that "like cures like."

From this "FIRST FACT," which we have shown to be merely a supposed observation, Hahnemann inferred that other drugs might have a similar correspondence with other diseases. He experimented with mercury, belladonna, digitalis, &c.; and, being satisfied that they produced symptoms analogous to those of the diseases for which they were remedies, concluded that this was a property peculiar to medicines of all kinds, constituting a *universal therapeutic law*, to which the following maxims may give expression.

"That all simple drugs, when taken in individual or health produce in

indication whence he can derive any intuitive notion." The "totality of the symptoms," he adds, "represents, in its full extent, the disease itself; that is, constitutes the true and only form of it, which the mind is capable of receiving." (*Organon*, pp. 105, 107.)

After expatiating at length on his supposed law, and making endless repetitions and contradictory statements, he thus sums up:

"From all that has been here stated, the following truths must be admitted.

"1st. There is nothing for the physician to cure in disease but the sufferings of the patient, and the changes in his state of health which are perceptible to the senses; that is to say, the totality or mass of symptoms, by which the disease points out the remedy it stands in need of; every internal cause that could be attributed to it, every occult character that man might be tempted to bestow, are nothing more than so many idle dreams and vain imaginings.

"2d. That state of the organism which we call disease, cannot be converted into health, but by the aid of another affection of the organism, excited by means of medicines. The experiments made upon healthy individuals, are the best and purest means that could be adopted to discover this virtue.

"3d. According to every known fact, it is impossible to cure a natural disease by the aid of medicines, which have the faculty of producing a contrary artificial state or symptom in healthy persons. Therefore, the Allopathic method can never effect a real cure. Even nature never performs a cure, or annihilates one disease, by adding to it another that is dissimilar, be the intensity of the latter ever so great.

"4th. Every fact serves to prove, that a medicine, capable of exciting in healthy persons a morbid symptom dissimilar to the disease that is to be cured, never effects any other than the momentary relief in disease of long standing without curing it, and suffers it to reappear, after a certain interval, more aggravated than ever. The antipathic and purely palliative method is, therefore, wholly opposed to the object that is to be attained, where the disease is an important one and of long standing.

"5th. The third method, the only one to which we can still have recourse, (the homœopathic,) which employs against the totality of the symptoms of a natural disease, a medicine that is capable of exciting in healthy persons symptoms that closely resemble those of the disease itself, is the only one that is really salutary, and which always annihilates disease, or the purely dynamic aberrations of the vital powers in an easy, prompt and perfect manner. In this respect, nature herself furnishes the example, when, by adding to an existing disease a new one that resembles it, she cures it promptly and effectually."

remarks, certainly "produces sleep in the restless and wakeful, and in giving ease to the suffering, is an example of this method." *Second* :—The *Allopathic method*, "which," Hahnemann says, "without ever regarding that which is really diseased in the body, attacks those parts which are sound, in order to draw off the malady from another quarter, and direct it towards the latter." He adds, this method "cannot cure in any case; having no analogy or opposing force to the symptoms of the disease, it can never reach the parts affected." The exemplification of this method is found in the relief of internal inflammation or irritation by counter-irritation. *Third* :—The *Homœopathic method*—the system which we are exposing—which, he says, "is the only one which experience proves to be always salutary. The pure and specific effects of remedies employed, being perfectly analogous to the natural symptoms, *they go right to the parts affected* ; and as two similar diseases cannot exist at the same time in the same system, the natural symptoms give way, provided the artificial ones slightly *surpass them in intensity*."

When expressing the *first maxim*, deducible from his *universal therapeutic law* (!), we stated that Hahnemann ascribed to drugs an unconditional action—producing, *under all circumstances*, the same effects. This is contrary to daily experience, and his own statements on page 205 of his *Organon*, where he admits that "drugs do not effect every individual in the same manner, or with the same degree of energy ; on the contrary there exists, in this respect, the greatest diversity possible." But consistency is not to be found in his *Organon*—the text book of homœopathic medicine—as it cannot constitute an element of Homœopathy. On page 196 and 197, of the edition of the *Organon* before us, he explicitly states that even idiosyncrasies do not modify the action of medicines, and where there appears to be a difference in the action of drugs, it is only *imaginary*, the internal effect being always the same, which is proved by these agents curing, homœopathically, diseases in individuals subject to idiosyncrasies. On page 119 and 120, of the same work, after stating that the physical and moral powers do not possess, unconditionally, the power of deranging health or of causing disease in all men, or in the same men at all periods, he adds, that "it is quite otherwise with the artificial morbid powers, which we call medicines. A genuine medicine will, at *all times* and under *every circumstance*, work upon *every* living individual, and excite in him the symptoms peculiar to it, (perceptible if the dose is large enough,) to such a degree, that the whole system is always attacked, and in a manner infected, by the medicinal disease, which, as I have before said, is not at all the case in natural diseases."

for, he states that though the smell of roses will cause some persons to faint, the active property of this flower is efficacious in curing this condition in all persons alike. But he does not say, (as he must consistently admit,) that it *cures* fainting in the very cases in which it *causes* fainting. If his reasoning be consistent, the conclusion is manifestly absurd. On Hahnemann's principle, as the savour of a cat produces fainting in some idiosyncrasies, this domestic animal has an unknown tendency to produce fainting in all mankind; and hence its approach to fainting persons ought to cure them of their syncope.* Tycho Brache trembled at the sight of a hare; and Mr. Marc mentions two individuals, in whom fainting was always produced by the odour of that animal.† Petroz states that he knew a man incapable of fear, and who, in despite of all means of prevention, fainted always when he saw a boiled calf's head.‡ Would the sight of this dish, or that of a hare, recover all mankind from the syncope condition? Haller was always made ill by the smell of cheese; and Boerhaave relates a case of idiosyncrasy, in which the odour of this substance led to bleeding at the nose. According to Homœopathic doctrine, cheese should be a remedy for epistaxis (!) Bachner and Tissot mention epilepsy, as produced in a person every time he saw anything red. Do red colours cure epilepsy? We might enumerate instances of every variety of morbid phenomena being produced by the most trivial and common-place objects, and even by certain mental states; but the multiplication of examples cannot be deemed necessary to refute the silly and ridiculous assumption, that what *causes* a symptom, or set of symptoms, in one healthy individual, will *cure* these symptoms, when *already produced*, in all mankind.

Dr. Cowan, of Reading, when speaking of Homœopathy, some time since, remarked, that "it is not the practice of medicine only, that is at stake—it is the practice of *sound thinking*. When we supplant by mushroom theories, truths which have hitherto occupied the minds of men, and been tested and confirmed by the experience of ages—when we bring into existence new phantasies of the human mind, and give them the outward formality of facts—we surrender the stability of our judgment, and depart from all soundness of thinking. With us, as medical practitioners, the rejection of Homœopathy is a question of principle, not of doses—a question of morals, and not of etiquette."§

In instituting their "*provings*," Hahnemann and his disciples first used "the same dose of the drug that practitioners are in the habit of

forgotten that even the administration of moderate doses, of the so called heroic remedies, is sufficient to produce modifications in the health of the most robust individuals. Medicines that are more gentle in their nature ought to be given in large doses, if we would likewise prove their action." Without any explanation, in a note on page 204, he abruptly tells us that, "recently I have judged it more proper to administer only doses that are very weak and attenuated to a very high degree, *because it is in this form that the virtue of medicinal agents are most developed.*"

Must not a man of sane mind reject such contradictions as these—occurring, as they do, on the same page—as more than bombastic cant, and supercilious conceit? The man who could leave such statements for posterity, must have been more a dreamer than a thinker—more intent on dictating to mankind his visionary discoveries, than building up a consistent system of facts.

Occasionally, in the text of the concluding hundred pages, he alludes to his preference for infinitesimal doses of medicines, but in his notes he strenuously advocates the superior efficacy of the highest dilutions, and complains of "the blind prejudice which actuate those who avoid small doses, and thus deprive themselves of the great advantages which experience has a thousand times proved to result from them."—(p. 277). The "suitableness of a medicine," he subsequently states, "for any given case of disease, does not depend on its accurate homœopathic selection alone, but likewise on the proper size, or rather smallness, of the dose. If we give *too strong a dose* of a medicine, which may have been even quite homœopathically chosen for the morbid state before us, it must, notwithstanding the inherent beneficial character of its nature, prove injurious by its mere magnitude, and by the unnecessary, *excessive* impression it makes upon the vital force which it convulses, &c." (Organon, p. 290). For "a medicine," says he, "of a positive and curative kind, will, without any fault in itself, produce just the contrary of that which it ought to do, if given in excessive quantity, by producing a greater disease than that present."—(Medicine of Experience, 1805.)

"With very large ordinary doses patients may," says Dr. Black, "be treated homœopathically; but then we may frequently expect a *positive increase of the disease, or even death.* The experience of such painful and dangerous aggravations, in no case necessary to cure, led Hahnemann to employ minute doses."*

On the last few pages of his Organon, Hahnemann confidently states, the *minutest* doses are *always* able to overcome the disease; and, elsewhere, when insisting on the necessity of the very smallest doses of the selected remedy, he adds: "The very smallest, I repeat; for it holds good, and will continue to hold good, as a homœopathic therapeutic maxim, *not to be refuted by any experience in the world*, that the best dose of the properly selected remedy is always the *VERY SMALLEST* one, in one of the high dynamizations, (X or the thirtieth dilution,) as well for chronic as for acute diseases, &c."†

Besides the increased efficacy which medicines acquire at each division

* Treatise on Homœopathy, p. 81.

† Tenets, p. 51.

or dilution, he contends, that "a new degree of power is imparted by the rubbing or shaking they undergo, a means of developing the inherent virtue of medicines that was unknown before my time; and which is so energetic, that latterly, I have been forced, by experience, to reduce the number of shakes to two, of which I formerly prescribed ten to each dilution."—(*Organon*, p. 295.)

Contrary to the laws of chemical affinity, and the dynamo-chemical processes of matter, Hahnemann, we see, contends that the powers of drugs are not only increased by his singular manipulations, but that they are developed by them in the inverse ratio of their weight and bulk. We would invite the reader's attention particularly to this topic, not only because it forms one of the principal objections to Homœopathy, but also because homœopathsists may be anxious to prove to the public, that physicians have committed a gross error, or have wilfully distorted, by sophisms, Hahnemann's meanings and intentions.

In order to form some idea of the extreme smallness of homœopathic doses, it may not be unimportant to dwell for a moment on their mode of preparation. Homœopathsists exhibit their drugs in the form of solutions or tinctures, or in the form of globules or small pills; and sometimes also in the form of powders, attenuated with the sugar of milk. The degree of "attenuation," "dilution," "potency," or "dynamization," varies from the 1st, 2nd, 3rd, 10th and 20th, to the 30th, &c. The first degree is obtained by mixing one drop of the drug to be used with 99 drops of alcohol, if it is to be a tincture; after a certain number of shakes, &c., one drop of this mixture is to be mingled with 99 drops more of alcohol, and so on, until this is repeated thirty times. The thirtieth dilution, therefore, contains the following part of one drop, viz:

Dilution—1st. 2 3 4 5 6 7 8 9 10 11 12 13 14
 100, 00 00 00 00 00 00 00 00 00 00 00 00 00
 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30.
 00 00 00 00 00 00 00 00 00 00 00 00 00 00 00.

When *globules* are to be prepared, pellets, as large as poppy seeds, of the sugar of milk, or common sugar and starch, are moistened with the desired attenuation, by applying the moistened stopper of the phial containing the dilution itself. These are then put in stoppered phials and kept ready for future use.*

Increasing experience induced Hahnemann to caution his disciples from carrying medicines about their persons, as the *successions* incident upon locomotion would *potentize* the doses to a poisonous degree of intensity!

"Whilst Hahnemann warned against giving *Drosera*, (Sundew,) in whooping-cough, that had been prepared with 20 shakes, Jenichen's preparations of that drug in the 500th dilution was shaken 600 times—

there could be no doubt as to inutility of any number of shakes, or their harmlessness to those who swallowed them.

The thirtieth dilution, which Hahnemann considered the most appropriate dose both in acute and chronic diseases, contained a decillionth of a grain or drop of the drug employed. We have arithmetically expressed this quantity; but to convey to the mind more forcibly the infinitesimal exiguity of this dose, we will resort to illustrations of an adequate magnitude, drawn from external nature, which Dr. Simpson has furnished for the purpose.

“Soon after the promulgation of Hahnemann’s doctrines, it was suggested that, ‘If the decillionth part of a grain have any efficacy, an ounce of medicine (Epsom Salts) thrown into the Lake of Geneva would be sufficient to physic all the Calvinists of Switzerland.’

“But more careful systematic calculations have shown that this is stopping infinitely short of the truth; and that the 30th homœopathic dilution, recommended as we have seen by Hahnemann in all cases, is, in such a parallel, enormously under-stated, instead of over-stated. In fact, the 10th solution alone would, as Mr. Cap has shown, require for its proper solution a body of water 500 times greater than the bulk of the Lake of Geneva; or a sea somewhat larger than the Gulf of Venice. To make the 11th solution, a quantity of water greater than the Mediterranean Sea or the German Ocean would be necessary. The 12th solution could scarcely be accomplished in a sea extending over the whole surface of the earth, and 500 fathoms in depth. And if the whole Solar System were buried in an ocean extending in depth from the Sun to Neptune, it would not form a sufficient fluid medium for adequately dissolving to the 30th dilution a common dose of any of the common medicines of the homœopaths. Yet the homœopaths allege, a few drops or sips of the proper medicine, properly dissolved in such enormous medicated seas and oceans, infallibly acts and cures; and that each drop would be found of ‘terrific potency,’ if the drug were duly mixed.

“The earth is computed to contain, at the present time, some 900,000,000 human beings; and that if all these 900,000,000 had been called into existence when Adam was created,—some 6,000 years ago,—and lived up to the present time, and each of these 900,000,000 individuals had, when first called into existence, begun to swallow, and continued to swallow up to the present hour, without rest or cessation, night and day, a decillionth dose of a grain, (say a grain of sulphur or quinine, or other drug,) such as Hahnemann recommends to be used, these 900,000,000 beings would not yet, during these past 6,000 years,

and with these infinitesimal doses of the requisite medicine you may cure any acute or chronic curable disease to which mankind is liable. And that one of these decillionths is truly the proper dose of the medicine to be used, is a 'therapeutic maxim' (says Hahnemann) not to be refuted by any experience in the world.'

"To understand, if possible, the exiguity of 'the best' dose of a homœopathic drug as thus laid down by Hahnemann, let us turn to another means of stating the infinitesimal amount of the dose. For this purpose let us suppose, merely for the sake of illustration, that the drug employed was commixed with a mass of sea-sand, as its vehicle or medium, instead of a mass of sugar or starch, such as is employed in forming homœopathic globules and powders.

"Most rational men, whether acquainted with medicine or not, would surely and justly look with perfect incredulity upon any physician who should maintain that *one* single grain of sulphur (to take it as an example) properly commixed with a heap of sand made by reducing to powder the stones, &c., employed in building and rearing all the houses in Edinburgh, or in London, or in Great Britain, would impart to each of the particles of the heap a sufficient amount of sulphur to enable it, if swallowed, to be a proper and sufficient dose for curing a man of Itch. Or if some enthusiast were to spring up and aver that a single grain of phosphorus or of aconite, so infinitely divided, that an equal portion of one grain of these drugs distributed among *all* the particles of sand that lie on the sea-shores of Great Britain, from Caithness to Cornwall, could change and medicate each one of these grains of sand, so that any dozen or two of them picked up along the coast of England or Scotland would, if swallowed, be certain to influence and cure a case of inflammation of the lungs, or of inflammatory fever, if given to men affected with these diseases; would the world not deem the asserter of this fact to be stating something beyond the bounds of mere extravagance and eccentricity? And, if our supposed enthusiast were further coolly and deliberately to aver, that his single grain of phosphorus or aconite, if divided amongst all the grains of sand to be found along all the sea-shores of our world, would equally, if not more certainly, cure the same inflammation of fever, would he not be looked upon as a man outraging every principle of possible belief? But the homœopaths aver infinitely more than this. Hahnemann and his followers, in using the 30th dilution of aconite, phosphorus, &c., aver that a *single grain* of any of them, distributed equally through a mass of sand as large as

would require twenty years to pass from the earth to the sun, and about five millions of years ere it could reach the nearest fixed star. But the velocity of light is immensely greater. According to Struve's late investigations, it flies at the rate of 166,000 geographical miles, or nearly 200,000 English miles, in a second. Travelling at this enormous rate, a ray of light passes from the moon to the earth in $1\frac{1}{2}$ second; from the sun to the earth in 8 minutes; but would require about three years to travel from the fixed star *α Centauri* to the earth. Other stars, however, are vastly more distant. The elder Herschel calculated that light, travelling at the enormous velocity we have stated, would require almost two millions of years to pass to the earth from the remotest luminous vapour reached by his forty feet reflecting telescope. And Professor Nichol imagines, from later observations with stronger instruments, that possibly there may be stars 'situated so deep in space that the rays of light from them do not reach our earth until after travelling across intervening abysses during centuries whose number stuns the imagination.' Possibly there may be some, he imagines, from which light would require to travel THIRTY MILLIONS OF YEARS, in order to reach our planet.

"Truly, such astronomical calculations do, as Professor Nichol observes, 'stun the imagination' of man. They are as naught, however, in comparison with the ideas and computations of homœopathsists. For one single grain of any homœopathic medicine, as sulphur, quinine, silex, &c., &c., &c., divided into decillionth globules of the 30th dilution, and arranged as twenty globules to the inch, would form a continuous line or string of globules reaching far, far beyond this inconceivable profundity in space. In other words, this grain of sulphur, &c., &c., &c., would, when divided into a decillion of homœopathic globules, make a line of which light, travelling at the rate of 20,000 miles in a second, would require above 1,000,000,000,000,000,000,000,000 years to pass along. Yet, ever Hahnemann and his followers, any one of these decillion globules of a single grain of the proper drug, picked out of this inconceivable length of line, would be found duly medicated for the treatment of all diseases, acute and chronic, and would form the most appropriate dose for their treatment. Truly the human imagination sometimes passes from the sublime to the ridiculous."

Some homœopathsists use higher attenuations than the *thirtieth*. Some have gone up as far as the 8,000th. The universe itself can furnish no illustration, or give the human mind any adequate idea of the extreme minuteness of the medicinal particle which would be contained in a dose of the 500th, much less of the higher attenuations. Yet Dr. Gross, a celebrated German homœopathist, wrote in favour of these high dilutions or potencies, and exclaimed to his brother homœopathsists, that

the credulity of his followers when he asserted the superior efficacy of the thirtieth dilution, but he has ventured infinitely further, and gravely tells us that, as his experience accumulated upon him, he found that merely *smelling* a decillionth globule was all that was requisite to produce the desired result. Writing in 1833, he observes that he can scarcely name one in a hundred, out of the many patients who sought his advice during the year, whose chronic or acute diseases were not successfully treated, *solely by means of this olfaction*, and that he found the power of the medicines equally strong when administered in this way; and consequently, olfaction did not require a more frequent repetition than the other mode of administration.

Dr. Gross, using, as we have seen, medicines of the highest potency, "often contented himself with allowing the patients to *smell* the remedy, waiting patiently for *four weeks* or so, for the completion of the cure, not even permitting a second smell, so mild yet certain is the remedial action."^{*}

"Dr. Crosiero of Paris, in a communication published subsequently to Hahnemann's death, gives some more particulars respecting the practice of Hahnemann in the last years of his life, of which he assures us he was often a witness. 'Hahnemann,' he writes, 'always made use of the well-known small globules, which were generally impregnated with the 30th dilution, both for acute and chronic diseases. . . . He latterly employed olfaction very frequently. For this end, he put one or two globules in a small medicinal phial, containing two drachms of alcohol, mixed with an equal quantity of water, which he caused to be inhaled once or twice with each nostril—never oftener. My own wife (says Dr. Crosiero) was cured by him in this manner of a violent pleurisy in the course of five hours. In chronic diseases, happen what might, he never allowed this olfaction to be repeated oftener than once a week. And he gave besides, for internal use, nothing but plain milk-sugar. And in this manner he effected the most marvellous cures, even in cases in which the rest of us had been able to do nothing.'"[†]

Even odourless substances, according to Hahnemann, were capable of producing the most magical effects. "A single grain of the quintillionth attenuation of gold," he tells us, "put into a small clean phial, will restore a morbidly desponding individual, with a constant inclination to commit suicide, in less than an hour to a peaceful state of mind,

will certainly be removed; in two hours he is *quite* well?"—(*Tenets*, p. 77.)

"It is easy to estimate the medical value or valuelessness of such a dose, given either in the way of olfaction or otherwise. It may be more difficult to estimate the *pecuniary* value of such a prescription. But perhaps most sane minds will hold, that a due appreciation of the billion-worth of such a homœopathic dose by olfaction, was formed by a patient mentioned by the Editor of the Pharmaceutical Journal. 'We have heard (says he) of a lady who, having been subjected to this process (the administration of a homœopathic dose by olfaction) by her homœopathic doctor, passed the fee before *his* nose, and then—replaced it in her pocket.' "*

When noticing the homœopathic law of cure, (*similia similibus curanter*,) we adverted to the curative indications of disease, which Hahnemann lays down as an "indubitable truth," to consist in the *sum* of all the *symptoms* alone. "By the removal (he maintains) of the whole of the perceptible *signs* and *symptoms* of the disease, the *sum total* of the disease is at the same time removed."—(*Organon*, p. 119.)

When the "*sum total of the symptoms*" of a disease is ascertained, the homœopathist has to select from his list of remedies, one which has been *proved* capable of producing in the healthy organism a similar set of symptoms, in order to effect a cure. But, as Hahnemann found that the ordinary doses of the *appropriate* remedy *aggravated* so much the disease of the patients, that their maladies were rendered worse, he "judged it more proper to administer only doses that were *very weak* and attenuated to a high degree."

We have dwelt sufficiently, we think, on the extreme degree of height to which this attenuation has been carried. It remains now to notice the manner in which these minute doses cure diseases, and the means by which their curative properties were ascertained. Hahnemann tells us, that by reason of their minuteness, they go right to the part affected and establish a disease of their own, which, obliterating the natural one, is in turn subdued by the vital force—the *vis medicatrix naturæ*.—(*Organon*, p. 296.) This little infinitesimal, though "powerful" enough to dislodge the most violent disease, strangely becomes very "gentle" when its task is done, and submits to the dominion of the former occupant which was too weak to defend its charge without its interference.

Homœopathists would have the world believe that by the aid of their *potentized* infinitesimals, diseases can be arrested at once, and most effectually cured. Even the self-limiting diseases—the *Erythematæ*, etc.—they tell us, can be permanently inoculated. Dr. Hering in his

rely so implicitly on its potency, that they in no case think it necessary to administer more than one *single simple* medicinal substance at one time. The true physician, observes Hahnemann, "never thinks of giving as a remedy any but a *single simple* medical substance," for "it is yet impossible to foresee how two or more medicinal substances might, when compounded, obstruct and alter each other's actions on the human body."—(*Organon*, p. 320 ; *Tenets*, p. 58.)

Like all other homœopathic theories, the absurdity of this, or rather the impossibility to apply it in practice is soon rendered apparent when we reflect upon the compound nature of all vegetable substances, and products of chemical combination termed simple medicines. In few or no instances can homœopaths observe their own *dicta*. In the administration of what they call "single simple" medicinal substances, the most heterogeneous and complex compositions exist. If we take Opium as an example—"a medicament frequently indicated in various disorders"—we find no less than twenty-one different substances; yet it is a homœopathic remedy. Other substances violate the homœopathic rule still more; but our intelligence is insulted when the homœopathist, who dogmatically tell us that only one *single simple medicine* is to be exhibited at one time, prescribes alkaline salts and other still more composite drugs.

We forbear further comment, and pass on to a consideration of their *provings*—the means by which the curative powers of drugs, and their applicability to certain diseases, are ascertained. This is done by giving the remedy to a person in health, and noting carefully the symptoms which followed. The symptoms which a drug is capable of producing in a healthy person, is said to be similar to those which, when occurring in the sick, are speedily and permanently cured by an infinitesimal dose of the same. The results of these experiments or *provings* of drugs are received by homœopaths as matters of immense importance. They constitute the so-called data or facts, which are to direct the practitioner in his selection of remedies for particular diseases; therefore the artificial symptoms which drugs are capable of exciting, have been most elaborately and carefully recorded. Strange that so accurate and refined a science as homœopathic therapeutics is claimed to be, should have no formal rules to guide the experimenter or drug-prover in his observations or preliminary preparations. Even the *size* of the doses used in these *provings* is not mentioned. "The truth," remarks Dr. Hooker, "of the whole matter is, that it makes so little difference to Hahnemann or his followers whether the dose in the provings be infinitesimal or are in the 'coarse form,' used by *ordinary* physicians, that they do not in their records of these provings, indicate in any way the kind of doses with which they were made. Nothing definite is said in regard to this point in any of the books which I have consulted."

These "proving," then, or suppositious symptoms, obtained by these provings, are essential and necessary before the principle of Homœopathy could be applied in practice; and on the accuracy of them, the application of Homœopathy, as a system of medicine, altogether and entirely depends. We naturally enquire into the value of these provings, and inspect the symptoms which are said to *follow* the exhibition of the drug experimented with. Such strange, absurd and insignificant phenomena are noted down as the direct and invariable effects of certain medicines, that common sense revolts at crediting them as such, and we are forced to view them as the *mere imaginings* on the part of the individuals experimented upon.

Dr. Simpson has selected several pages of these *invaluable homœopathic data* from Jahr's Manual and Hahnemann's *Materia Medica Pura*, a few of which we will insert here, to convey an idea of the absurdity to which we allude.

"Common house or table salt, (*Natrum Muraticum*, as Jahr terms it) when proved upon a healthy person, can cause above *four hundred and fifty* symptoms, and, consequently, is capable, on the homœopathic principle, of curing many of these symptoms when they occur similarly in different states of disease. He gravely describes these symptoms, under the several heads of "General Symptoms," and symptoms observed in the "Skin," "Sleep," "Fever," "Head," "Eyes," "Ears," "Nose," etc., etc. The mere enumeration of the symptoms produced in these various parts by the house-salt, occupies about six pages in Jahr's book, the citation of all which would occupy more space than we can bestow on them. But the following may suffice as examples:

Symptoms produced by common House-Salt.—"Rigidity of all the joints, which crack when they are moved." "Paralysis." "Swelling of the glands." "Great drowsiness during the day." "Retarded sleep and sleeplessness at night, with ineffectual efforts to go to sleep." "Agitated sleep, full of vivid and lascivious dreams." "Frightful dreams of quarrels, murders, fires, thieves." "At night pains in the back, quivering, apparently of the nerves, frequent emission of urine, headache, colic, asthmatic sufferings, and great anguish of body." "Hatred to persons who have formerly given offence." "Awkwardness." "Sensation on moving the head as if the brain wavered." "Spasmodic closing of the eyelids, especially in the morning, in the evening (during the twilight) and at night." "Numbness and insensibility of one side of the nose." "Boring in the bones of the nose." "Drawing, like extraction in the teeth, extending into the ear and the throat after a meal, and at night, with swelling of the cheek." "Speech embarrassed in consequence of the heaviness of the tongue." "Prolonged sensation as of hair on the tongue." "Loss of appetite, especially for bread, and repugnance to tobacco smoke." "Protrusion of hernia." "Excoriation between the buttocks, especially when walking." "Excessive excitement of the amative feeling, or dulness." "Nocturnal pains in the back." "Numerous flaws in the nails." "Burning in the feet." "Redness of the great toe." "Corns on the feet, with shooting and boring pains," etc., etc., etc.—(*Jahr's Manual of Homœopathic Medicine*, vol. i. p. 386, *et seq.*)

Let us take another example. "CARBONATE OF LIME OR CHALK (*Calcareæ Carbonica*) exists in most vegetables, and is contained in greater or less quantity—but in doses larger than the Hahnemannic—in almost every water which man drinks. It is found (says Dr. Pereira) even in the purest waters, viz: in rain water. Carbonate of Lime is, according to Jahr, usually employed in the dose of a decillionth, or in the 80th dilution; and the duration of its effects is fifty days in chronic affections. According to the provings of the homœopaths, Carbonate of Lime is capable of producing above *one thousand* symptoms. Out of this immense number of effects, we select the following as specimens of the symptoms it may cause, and hence may cure."

Symptoms produced by Chalk or Carbonate of Lime.—"Shootings and drawing pains in the limbs, chiefly at night, or in summer, and in change of weather." "Great tendency to strain the back in lifting." "Strong desire to be magnetized." "Emaciation, without failure of appetite. Great plumpness, and excessive obesity." "On walking in the open air, sadness with tears." "Visible quivering of the skin from head to foot." "Quotidian fever towards two o'clock in the afternoon, with yawning and cough, a desire to lie down at least for three hours, after which the hands become cold." "Tertian fever in the evening." "Disposition to weep, even about trifles." "Vexation and lamentation on account of old offences." "Anxiety and anguish excited by fancies, or frightful stories." "Disposition to take alarm; Apprehensions." "Pains of hammering in the head." "Icy coldness in, and especially on the right side of, the head." "Movement of the brain in walking." "Immense size of the head." "Strong dispositions to take cold through the head." "Smarting on reading during the day, or by candle light." "Flow of blood from the eyes." "Sense of smell dull, or exceedingly sensitive." "Fœtid odour before the nose, as if from a dunghill, rotten eggs, or gunpowder." "Face pale and hollow, with eyes sunk and surrounded by a livid circle." "Fœtid odour of the teeth." "Frequent protrusion of hæmorrhoidal excrescences during the evacuations." "Burning eruption in the form of a cluster in the arms." "Wetting the bed." "Polypus of the bladder." "Prolapsus uteri." "Varices in the labia majora." "Pain, as of excoriation and ulceration, in the nipples." "Ulceration of the larynx." "Cough excited by playing on the piano." "Warts on the arms and on the hands." "Contraction of the fingers." "The legs go to sleep when one is seated," etc., etc., etc.—*Jahr's Manual of Homœopathic Medicine*, vol. i. p. 108, et seq."

"Perhaps it is proper to add, in order show the fantastical affectation

ache after”—(P. 217.) The symptoms produced by Colchicum “are singularly aggravated by a too brilliant light and by the smell of pork,” (p. 186;) and “the smell of fresh eggs, or fat meat,” is liable “to occasion loss of consciousness” in those using this drug.—(P. 188.) Borax, or Subborate of Soda, produces and removes “great anxiety when riding in a carriage,” (p. 92;) and Charcoal generates and relieves “colic produced by the motion of a carriage.”—(P. 137.) Doses of Sweet Violet (*Viola Odorata*) cause, hence cure, an “aversion to all kinds of music, and principally the violin,” (p. 608;) and common Subcarbonate of Soda, and consequently Soda Water, both causes and cures “trembling when playing on the piano.”—(P. 381.) “The majority of the symptoms (produced by Daphne) manifest themselves when the moon is waning,” (p. 217;) and the symptoms following the use of Silex or Flint are aggravated at the new or full moon.—(P. 531.) Many of them produce peculiar conditional effects on particular parts; for example, the feet and toes. Thus *Agnus Castus* produces a “tendency to twist the feet when walking on the pavement,” (p. 12;) Camphor causes “acute drawing in the extremities of the toes and under the nails when walking,” (p. 121;) Crude Antimony produces “callous excrescences at the tips of the toes and under the nail of the great toe,” (p. 42;) Epsom Salts cause “shootings in the toes,” and “tearings in the tips of the fingers at night,” (p. 351;) Ammoniacum excites “drawing in the middle toe,” (p. 23;) a preparation of Iron (*Ferrum Magneticum*) makes “the little toe painful, as if it were violently compressed,” and causes “ganglion of the foot,” (p. 244;) *Athamanta* causes “tearing and burning in the left little toe,” (p. 66;) Sulphur causes “easy dislocation of the foot when walking,” (p. 572;) Nux Vomica produces “facility of dislocation of the instep,” (p. 422;) Carbonate of Soda gives a “tendency to dislocate and sprain the joint of the foot,” (p. 385;) etc., etc., etc.”

To show that these *provings*, selected from Jahr's Manual, are not only received but adopted by the homœopathists generally, we have only to state, that lately, between twenty and thirty homœopathic physicians of Great Britain, with Dr. Henderson among the number, have instituted the “HAHNEMANN PUBLISHING SOCIETY, for the purpose of publishing scientific and practical works on homœopathy,” some of which contain “provings” equally absurd with any that we have quoted

Pura," covers almost fifty octavo pages. And as early as 1838, Hering stated the results of the provings had already filled more than fifteen octavo volumes.

Hahnemann asserted that by these provings "a true *Materia Medica*" might be acquired; and then, said he, "what cures shall we not be able to perform in the vast empire of disease, when numerous observers, upon whose accuracy and veracity we can rely (!) shall have contributed the result of their researches to enrich this '*Materia Medica*,' the only one founded on fact? The art of curing will then approach to the same degree of certainty as the science of mathematics!"

"We cannot," observes Dr. Simpson, "however, dismiss these proving of homœopathic drugs—and particularly, of such drugs as house-salt, carbonate of lime and other articles which man of necessity daily employs in his food and drink—without offering one remark. If this portion of the creed of the homœopathists were true, man would be one of the most unfortunate and miserable of created beings. If, for example, the salt which he must take with his food, and the carbonate of lime which he must take with his drink, were actually capable of producing in the human constitution the hundreds of separate—and in some of their details, severe—symptoms ascribed to them by Hahnemann and his disciples, would not the body of man have been necessarily rendered wretched, and (as they mostly produce various marked '*moral symptoms*') would not his mind have been rendered sinful, by the very substances which his Creator obliges him to use constantly in the course of the common and requisite nourishment of his frame? If Homœopathy were true, would this arrangement not form a strong and incontrovertible argument for skeptics to use, who wished to call in question the bountiful and beneficent arrangements of Providence?"—(*Tenets*.)

Granting that the symptoms attributed to the various drugs did follow their exhibition, every properly educated person, endowed with an ordinary share of common sense, will be at no loss to account for the similarity of many of the symptoms occurring in different individuals, provided they were equally credulous in expecting them. But the *provers* or experimenters, we are led to believe, were less credulous than the practitioners who credited them, and, many of the out-of-the-way, farcical and ridiculously absurd phenomena which are solemnly recorded by homœopathists, as the "invariable" (unconditional) effects of certain drugs, were mischievously related by some facetious youngster. Dr. Leo Wolf was told by a trustworthy young physician, then in this country,

BIBLIOGRAPHICAL NOTICES.

ART. VIII.—*A Treatise on Venereal Diseases.* By A. VIDAL, (*de Cassis*,) Surgeon of the Venereal Hospital of Paris; Author of the *Traité de Pathologie Externe et de Médecine Opératoire*, etc., etc. With coloured plates. Translated and edited by George C. Blackman, M. D. New-York: Samuel S. & William Wood. 1854. 8vo., pp. 499. (Through Messrs. McCarter & Co., Charleston.)

THE principles of Syphilis, as first propounded and classified by John Hunter, and modernized by Ricord, have been very generally adopted in Europe and this country, as offering the most satisfactory explanation of the nature and characteristics of venereal diseases. Hunter divided constitutional syphilis into two periods, and maintained the non-contagiousness of secondary symptoms. Ricord differed from Hunter on the subject of the identity or non-identity of Syphilis and Blennorrhoea. He lays down, as his first and fundamental principle, the non-identity of these two diseases; and his experiments at l'*Hôpital du Midi*, attested by thousands of competent observers, have satisfied all but a few restless and prejudiced minds of the correctness of his views. M.M. Velpeau, Cazenave, Gibert and some others, have always manifested the most inveterate hostility to the doctrines of Ricord; and, during the past two years, they have occupied almost continually the sessions of the *Académie de Médecine* with their angry denunciations and bitter invective.

M. Vidal, enjoying similar advantages for observation and experiment with Ricord, has come out as the champion of this party, and what they have advanced in the *Académie* he advocates in his book. His opposition to Ricord juts out on every page of his work; and nearly every question, supposed to have been definitely settled, is mooted, and thrown open again for discussion.

Two distinct schools are now established, having Ricord and Vidal as their respective champions. So we may expect to have the doctrines of the former thoroughly tested, and whatever errors they may contain cannot surely escape detection.

In reading the introductory pages of Vidal's work, which presents a synopsis of the two schools, with the subjects of contention their dis-

females, where every portion of the vagina can be seen by the use of the speculum, and where no ulcer, however minute, could escape observation. He lays down, as the most striking difference between simple and virulent gonorrhœa, the tendency of the latter to become chronic. Balanitis also comes under the head of primary syphilitic affections; he says that without excoriations or ulcerations, it is capable of producing chancre or contagion.

For the three varieties of Blennorrhagic Orchitis, viz: Epididymitis, Vaginalitis and Parenchymatous Orchitis, he recommends incisions, not punctures, as the most rational and best mode of treatment. "They afford immediate relief, by removing over-congestion and strangulation of the tissues. It is a sedative, antiphlogistic and resolvent, and was used on more than 400 cases without the least accident, and always with marked benefit." In advancing further into the work, we find him advocating the theory of metastasis for gonorrhœal ophthalmia; and laying down a number of reasons, which do not appear to us to have much weight, why this metastasis should occur.

The most striking differences, however, regard the effects of contagion. With him a chancre is not indispensable to the formation of Buboes, or of secondary and tertiary accidents. The syphilitic poison, he says, may be absorbed from a free mucous surface and become constitutional, without any local manifestation whatever. Or the poison might undergo a period of incubation—absorption in the meanwhile going on—so that Buboes *d' emblé* and cutaneous eruptions might precede the appearance of the chancre. He records cases of nodes and periostitis without chancres. A person after a suspected connection, he adds, can never be considered safe from infection. Such views of the syphilitic virus will modify considerably the local treatment. No good can be derived then from the nitrate of silver in the early stages of incubation, as taught by Ricord, before his period of absorption has arrived.

All chancres, he says, are more or less indurated; the induration not being the cause of constitutional disease, but an indication or effect of general contamination, and might, therefore, be considered a secondary accident.

Vidal, with a hospital full of materials at his command, has taken the lead in experimenting for the propagation of syphilis by secondary accidents. This subject, so important in a social as well as moral point of view, is creating considerable interest in Europe, particularly in France and Germany, and has many powerful adherents. It is a subject worthy of the many discussions which it is continually eliciting. Ricord's views of the non-inoculability of secondary accidents appear to be in a tottering condition, from the great array of apparently incontestable facts brought to bear against it by Wallace, Waller, Cazenave, Velpeau and others. It will require, however, further experimentation, and a greater accumulation of facts, to settle the question in a satisfactory manner. Ricord asserts that if the secondary accidents could once be inoculated they could be always inoculated, and that chancre alone is inoculable. Vidal states that success will depend upon the means employed. The manner of applying the poison may exercise a great influence on its effects. Even the pus of chancre, he contends, is not always contagious. The

pustular varieties of cutaneous syphilitic eruptions are those most readily transmitted. Mucous tubercles, which are sometimes primary, sometimes secondary, the former most frequently in women, can readily propagate the syphilitic affection. The same with tonsillar ulcers.

We find lastly a novel mode of treating syphilis, by what is called Syphilization, recently renewed by M. Auzias, to which we adverted in former numbers of this Journal. The object is to produce, by inoculating a certain number of chancres, either a prophylactic condition of exemption from both primary and secondary accidents, or obtain a therapeutic action for the destruction of the poison in the system. They expect it to perform the part of the vaccine pustule, and protect the system from contagion by saturation. Nations might possess this universal saturation by long familiarity with the disease. Vidal favours the views of Auzias, from a circumstance which he has often observed, that a part might become locally syphilized, as seen when a chancre exists on the end of the penis. Although the discharge of inoculable matter is constantly bathing the surrounding parts, we find no new chancre forming; but if such matter be carried to some distant surface we find an ulcer produced. Now by the multiplication of chancres a general syphilization is effected. From forty to one hundred inoculations are necessary; five or six daily. At first chancres are formed with all their characteristic symptoms, then only hybrid products, pustules which never degenerate; finally the soil becomes perfectly barren and refuses to yield even pustules; the system is saturated, all the material has been used up, and the fortunate individual possesses an immunity from this dreadful scourge. A case is mentioned of one, long diseased, who had taken in vain Mercury and Iodide of Potassium, improving in four or five days by inoculation.

This work, though the doctrines it advances are at variance with those hitherto considered orthodox, is, nevertheless, entitled to a most careful consideration, and should be read by the profession generally. It is ably translated, and admirably got up, with excellent coloured plates which give a very accurate idea of the various diseases represented.

ART. IX.—*A Text Book of Anatomy, and Guide in Dissections, for the use of Students of Medicine and Dental Surgery.* By WASHINGTON HANDY, M. D., Professor of Anatomy and Physiology in the Baltimore College of Dental Surgery, &c., &c. With two hundred and sixty-four illustrations. Philadelphia: Lindsay & Blakiston. 1854. 8vo. pp. 810. (Through Mr. John Russell, King street.)

THIS work, though designed for students, both of Medicine and Dental Surgery, is more particularly adapted to the latter. It directs "special attention to the mouth, showing step by step the important anatomical and physiological relations which it has with each and all the organs and functions of the general system."

The introduction gives an outline of the organization of animals gene-

rally. Then follows the subject proper, divided into four parts. The first treats of the elementary tissues of the body. The second part describes the organs of the head, in their functional order and dependency; combining thus their anatomy and physiology, and showing the relations of the mouth with the different parts of the head. The third part, embracing the trunk, treats in the same manner of the organs of this region, and their relations with the mouth. The fourth part is devoted to the extremities, which are demonstrated in the ordinary manner.

We deem the work a valuable addition to Dental literature; and hope it may accomplish the object of the author, in making the Dental student feel the necessity of a greater knowledge of anatomy than that of the mouth and its appendages.

ART. X.—*Elementary Chemistry, Theoretical and Practical*. By GEORGE FOWNES, F. R. S., late Professor of Chemistry in University College, London. Edited, with additions, by Robert Bridges, M. D., Professor of Chemistry in the Philadelphia College of Pharmacy, etc., etc. A new American, from the last revised London edition. Philadelphia: Blanchard & Lea. 1854. Small 8vo. pp. 554. (Through Mr. John Russell, King-street.)

SINCE the decease of its accomplished author, this deservedly popular work has gone through another edition, the revision of which has been thoroughly performed by Drs. H. Bence Jones and A. W. Hofmann. Upon the original basis, the revisers have made many additions, especially in Organic Chemistry, which has of late made much progress; and they have presented the work as they suppose the author would have desired, if his life had been spared to science.

The merits of this work are so well known and acknowledged in this country, that our commendation can add nothing to its reputation. The mere announcement of the appearance of a revised edition, including all the most recent acquisitions to chemistry, will be a sufficient inducement for those prosecuting this study to procure a copy.

ABSTRACTS

FROM FOREIGN AND DOMESTIC JOURNALS:

ANATOMY AND PHYSIOLOGY.

Entire want of the Lactal Secretion. (MORLAND'S Extracts from Soc. for Med. Improvement.)—Dr. Storer.—Six weeks since, a lady 21 years of age, whose health had been uniformly good for years, was confined with her first child. The *breasts secreted no milk*, although suction by the mouth had been continued three times daily by some member of her family until within a few days.

Dr. Abbot mentioned a case of non-secretion of milk for six weeks; it then appeared; the tardy action, Dr. A. thought, was owing to excessive prostration of the patient; the child was stillborn.

Dr. Minot referred to a case of want of secretion. No efforts were made to excite the glands to action, but rather the reverse, by reason of peculiar circumstances rendering their quiescence desirable. Sulphate of magnesia was administered.

Dr. Storer spoke of a case of weaning the child at nine months by reason of a freak of the mother; the milk disappeared, but returned on reapplication of the child to the breast.

Dr. Putnam referred to a case of non-secretion. He considered it constitutional.

Dr. Parkman said that the leaves of the castor-oil plant, made into a warm stupe, and applied to the breasts, had been vaunted lately in foreign journals as possessed of almost marvellous efficacy in the production of the lactal secretion.

Malformed Heart; Interventricular Opening. (MORLAND'S Extracts from Soc. for Med. Improvement.)—The patient was twenty years of age, and had been under the care of Dr. Cabot for the last three years. He was of a slender figure, though not particularly small or stunted, as is said to be often the case; very susceptible to cold, and had had marked cardiac symptoms from infancy. The lividity of the face and hands was always more or less noticeable, and, on any considerable exertion, it was deep, so as to attract attention in the street. Dyspnoea often occurred, so that he would be obliged, when walking to rest, and

more, and again on the 24th to the 25th; under this he sank, and died on the 29th. Under this attack of hemorrhage, his dyspnoea increased, and also a hacking cough, to attacks of which latter he had always been subject; there was also much fever. At the age of four or five years this patient had scarlet fever very severely, and at the age of ten, measles; he also had disease of the spine, producing a very marked backward curvature.

The heart, on examination, was found to be of about the usual size, and the opening between the two ventricles sufficiently large to allow the tips of two fingers to pass through. Left ventricle not at all thickened; but the right, as usual, very much so. The pulmonary artery has two well-developed valves; and the passage to it, from the right ventricle, to the extent of half an inch or more from the free edge of the valves, is so much contracted that the tip of the little finger would not pass. Some abnormal formation has been generally noticed at this part in cases of interventricular opening, and in all that have been observed here, it has been so without exception. There is a direct opening of the foramen ovale to the extent of about four lines, and a small band traverses it, as if to prevent its further enlargement. Almost the whole of the left lung was consolidated by a form of disease that seemed intermediate between pneumonia and a tubercular affection; being most advanced towards the base; in the upper right lobe there was tubercular disease and a small cavity.

On the Human Retina.—(Monthly Journal Med. Sci., from Amer. Jour. Med. Sci.)—In a memoir read before the Academy of Sciences, Sept. 26, 1854, by Kölliker and H. Müller, they announce some important investigations relative to the structure, the connection, and the probable function of the different parts of the retina. They describe the following layers: 1. Rods and cones; 2. Nucleiform bodies; 3. Gray substance; 4. Expansion of the optic nerve; 5. Limiting membrane. Passing over the last, it has been discovered by Kölliker that the expansion of the optic nerve is interrupted at the macula lutea, which exhibits no trace of nerve-fibres, while the nerve-cells form there a very thick layer of nine to twelve superimposed rows. In other parts, the termination of the nerve-fibres of the retina directly in the nerve-cells has been fully confirmed, the fibres becoming continuous with the processes, one to six in number, which these cells present, resembling entirely the prolongations of the ganglionic corpuscles of the brain and nervous ganglia. The nerve-fibres may, therefore, be said to originate from the nerve-cells. The cones (hitherto imperfectly described) are thicker and shorter than the rods, on the inner part of which they are placed; they present externally a prolongation resembling a short rod; they are pretty regularly set, and at the macula lutea, where the rods are entirely wanting, the cones are abundant, and form a continuous layer. From the internal part of each cone and rod there proceeds a fibre which passes through all the layers of the retina, and becomes lost on the inner surface of the limiting membrane. These fibres, first observed by H. Müller, in animals, are in relation with the nucleiform bodies; they have been named

"fibres radiæres," and are probably of great importance in regard to the functions of the retina.

From their observations, the authors conclude that the nerve-fibres of the retina do not serve for the objective perception of light, because they are deficient at the macula lutea, where vision is acute, and because the optic nerve itself is insensible of luminous impressions. It is improbable that the nerve-cells, or nucleiform bodies, which exist in several superimposed rows, can give rise to any very exact visual impression. The cones and rods remain, therefore, as the most likely parts to receive the impression of light, of which the Mosaic-like disposition would render the sensation as definite and exact as possible. The authors, however, have not completely demonstrated the connection by which such impressions could be transmitted to the fibres of the optic nerve; but they suppose that this communication takes place—1st, by means of the radiary fibres, which connect the cones with the nucleiform bodies; 2d, by means of the processes of the nucleiform bodies, which, becoming continuous with the external processes of the nerve-cells, would complete the communication from the nucleiform bodies to the nerve-fibres of the retina. However this may be, the authors conclude that the nerve-cells are the organs for the direct sensation of light, either immediately or by the intervention of the cones, rods, and radiary fibres; that these cells form a true ganglion or nervous centre, and that the optic nerve serves merely to transmit the sensations from the centre to the organ of intelligence and consciousness.

On certain Functions of the Spinal Cord.—(Proceedings of the Royal Society, 1853, from Amer. Jour. Med. Sci.)—T. Lockhart Clark draws the following conclusions from investigations which he has made on the ox, calf, cat, rat, mouse, and frog: 1. That the posterior roots of the spinal nerves consist of three kinds; two of them entering the posterior gray substance at right angles; the third kind, with different degrees of obliquity, tending upwards; a small portion only of the latter taking a longitudinal course, and becoming lost in the posterior white columns. 2. That in no instance were any fibres of the anterior roots seen to ascend with the anterior white columns, before they had entered the gray substance. 3. That besides the transverse bundles forming the anterior roots, a continuous system of exceedingly fine transverse fibres issue from the anterior gray substance, and become lost as they proceed towards the surface of the cord. 4. That from the preceding facts, it may be inferred that nearly all, if not the whole of the fibres composing the roots of the spinal nerves, proceed at once to the gray substance of the cord; and that, if any of them ascend directly to the brain, it must be *those only* of the *posterior* roots which run longitudinally in the posterior white columns. 5. That the communication between the sensorium and the spinal nerves is not established by the posterior white columns, but by the antero-lateral columns, especially the lateral. 6. That many of the fibres, belonging respectively to the anterior and posterior roots in different regions of the cord, terminate there by forming with each other a series of loops of various sizes and

lengths; and that it is not improbable that some of them may reach even as far as the brain. It is not perfectly denied by the author that a portion of the roots may be connected with the vesicles of the cords, but he considers the evidence of any such connection as very unsatisfactory. 7. The fine longitudinal fibres, described by Stilling, have not been found by the author. He is inclined to believe that the gray substance of the cord does not transmit impressions to and from the brain. 8. That there is great correspondence in the fibrous arrangement between the gray substance of the cord and the chiasma of the optic nerves. The author further remarks that the circumstances of the nerve-roots diverging upwards in the cord and intricately intermingling with each other, may explain why impressions made at one particular spot are communicated to distant parts of the cord, so as to excite simultaneous and sympathetic actions in classes of muscles which otherwise would appear unconnected.

SURGERY.

Wire Gauze for Bandages, Cradles, and Splints.—(Med. Times and Gaz. from Amer. Jour. Med. Sci.)—Specimens of this article, invented by Mr. Startin, were exhibited to the Medical Society of London, (Jan. 28, 1854.) The material employed is flattened copper or iron wire, and costs about 1s. 4d. per square foot; and, if the expense were not an object, the materials might be plated. The usual mode of application is, first to obtain a pattern for the splint by means of cartridge-paper, and then carefully to cut the sheet of gauze to the pattern. The splint further requires that the edges should be cut transversely at intervals, and the free edges covered with thin lead or adhesive plaster. Folds of linen, wet with water, are placed upon the limb underneath the splint, and the whole apparatus is kept in position by rollers or tapes. The merits of the invention were said to be those of lightness, cheapness, coolness, and affording the opportunity of readily applying lotions without disturbing the bandages. It was recommended in fractures, resections of the joints, and, indeed, in almost all instances in which cradles and splints are ordinarily employed.

A new Mode of Tying Vessels.—(Ranking's Abstract, from Rêvue Med. Chir.)—M. Mattei proposes a new kind of ligature, consisting of two loops, intercrossed, and with the ends in opposite directions. The end of the vessel is placed between these loops, and constricted by pulling at the ends; or if it is an entire and undetached trunk which has to be tied, one loop is first passed around the vessel in one direction, and then the other around it in a contrary direction, taking care to thread the ends in such a way around the silk forming the first loop, as to secure the necessary intercrossing. If the vessel is of large size, the ends of the loops are made to have an extra turn upon each other. After the vessel is tied, one end of each loop is cut away close to the vessel, and the other end is brought out of the wound, the two remain-

ing ends being disposed as nearly as possible in opposite directions. In this position they are allowed to remain until the vessel is obliterated, and then they are removed by gentle traction at each of the protruding ends, each loop slipping away from the other with perfect readiness, and without in any way disturbing the parts. In some cases, also, M. Mattei does not clip away any part of the loops, but attaching a thread to their bends, and bringing these threads out of the wound along with the ends of the loops, he is able to bring away the loops by pulling at the threads attached to them.

The grand advantage of this process is, that instead of having to remain until the included portion has been slowly eaten through by absorption, the ligature may be removed as soon as the vessel is obstructed; and thus one grand impediment to the process of healing by the first intention is removed. Another advantage is, that the doing away with the ulcerative process lessens the risk of secondary hemorrhage.

M. Mattei has tested the applicability of his plan in amputations of the breast, leg, and arm. He has removed the ligatures from the principal vessels on the fourth or fifth day, and from the smaller vessels in the course of the next day, and without any loss of blood; but he has not yet had an opportunity of experimenting upon the femoral or larger vessels.

Treatment of Nævus by Tartar Emetic Plaster.—(Lancet, in Amer. Jour. Med. Sci.)—Dr. Cumming read to the Westminster Medical and Surgical Society (Feb. 17, 1854) a paper on this subject. After alluding to the various methods that have been adopted in the treatment of these cases, and to the objections which apply to many of them, he stated that the most satisfactory cures had resulted from the inflammatory and ulcerative processes being spontaneously set up in the tumours, and producing the obliteration of the enlarged vessels by suppuration. He then mentioned Mr. Hodgson's plan of vaccinating the tumours, for the purpose of exciting the adhesive inflammation, but regarded it as a doubtful method of cure, since, though the tumour might be studded over with punctures, only two or three vesicles might arise, and only a partial obliteration ensue. He suggested, therefore, the more effectual plan of treatment by tartar emetic, the method of applying which will best be illustrated by the following case: A child, aged nine months, was brought to him on account of its having lost a large quantity of

peared, and, in order to secure as many as possible, the plaster had been pressed gently down over the pustules daily, until it was removed on the ninth day, at which time the eruption had completely involved the diseased structure. The pustules ran through their usual course, and in due time a slough was detached, and the ulcerated surface healed rapidly, the resulting cicatrix being scarcely noticed. Eight similar cases were then successively narrated, in all of which the same simple method of treatment was adopted with like success, the only exception being in one of them where the cure was only partial, in consequence of the tumour having been situated too near the angle of the orbit to permit the free use of the tartar emetic plaster. In all the other cases, the scars which resulted were comparatively slight. He then exhibited a patient on whom he had thus operated for two *nævi*, one of which being situated on either brow; the cicatrices which had followed, though large, were not unsightly.

Radical Cure of Hydrocele.—(N. Y. Jour. Med. in Amer. Jour. Med. Sci.)—Professor Willard Parker, of New-York, recommends for the radical cure of hydrocele, the local application of the solid nitrate of silver; and he states that this possesses the following advantages over other operations: 1. The ease and safety with which it may be performed. 2. The less liability to severe inflammation. 3. The certainty of success. The following case illustrates the mode of operating, and the results of treatment:—

“Mr. J., aged about 60, an Irishman, waiter by occupation, unmarried, had always enjoyed good health until April last, when he discovered an enlargement of the left scrotum. It had never previously been the seat of any difficulty. The tumour increased so rapidly that, within three weeks, it had become a great annoyance, and prevented him, simply from its size, from continuing at his business. At this time I first saw him, and such had been the rapidity of the growth of the tumour, that it had been mistaken for hernia, and he was wearing a truss. On examination, however, its true character, that of hydrocele, was made out without difficulty; a trocar and canula were accordingly introduced, and a large quantity of water withdrawn, and the patient dismissed. In about three weeks he again applied for relief, and I proceeded to operate for his radical cure in the following manner: After drawing off the fluid contents of the tumour in the ordinary way, I introduced through the canula a common probe the end of which was coated for

natural size. In this condition, it has since remained, with no symptoms of a return of the hydrocele, the cure having been complete."

Cases of Lymphorrhagia. By M. DEMARQUAY.—(Assoc. Med. Jour., in Ranking's Abstract.)—In the *Mémoires de la Société de Chirurgie de Paris*, tom. iii. fas. 2, M. Demarquay has published two of these very uncommon cases.

Case 1.—M. X., æt. 17, on the 9th of March, 1852, while playing with some relatives, accidentally found that his shirt and trousers were wet. On examination, he found that the fluid escaped from a part of the thigh; it was at first colourless, but soon become milky. A medical man was called, who arrested the flow by applying a compress dipped in cold water, with a slightly tightened bandage. The next day, M. Demarquay found, at the lower and inner part of the left thigh, numerous small and depressible elevations, which he at first took for hypertrophy of the follicles. The linen which had been worn on the previous day had become stiff, as if it had been dipped in an albuminous fluid. It was evident that much lymph had escaped, but the exact point of its exit could not be determined.

Some days afterwards, M. Demarquay had an opportunity of seeing some lymph escape from the lower and inner part of the right thigh of M. X. It was at first colourless, and of a slightly opaque tint, passing to yellowish-white on accumulation in a glass vessel. On examining with a lens, M. Demarquay ascertained that it escaped from a very limited spot; it was thrown out with some force, for it slightly raised the epidermis; indeed, it flowed in the same way as blood from a small arterial twig at the bottom of a wound. Eight *grammes* (a little more than two drachms) were collected in a wine glass; in a few minutes the fluid was entirely coagulated; it adhered to the vessel, and presented a bluish-white appearance, inclining to yellow.

Some days afterwards MM. Denonvilliers, Ricord, and Florens, examined the case with M. Demarquay. Independently of a series of granular elevations at the inner part of the right thigh, at the union of the lower with the middle third, there was a projection three or four *centimètres* (one inch and one-fifth to one inch and a half) in extent, reaching, in a slightly curved form, from the front to the inner part of the thigh. It yielded to pressure like a varicose vein; upon and around it were small granular elevations. The projection increased on motion, and diminished during rest; it was evidently a varicose dilatation of a lymphatic vessel.

From this time till September M. X. remained free from the lymphorrhagia. At the latter period, when in Montpellier, he was again attacked; the fluid was then examined by M. Bouisson and other physicians. A flow of lymph occurred for a short time on October 31st; and

formed in the centre, appearing as if composed of a series of reddish filaments in abundant serum. The varix had increased in size, and the granular elevations, observed in March, had become transparent and vesicular. On pricking one of them lymph escaped. The right thigh was at this point more than two *centimetres* (four-fifths of an inch) larger than the left.

Pressure above and below the point whence the lymph escaped gave rise to a jet as from a vein of moderate size. When pressure was made below the dilatation the flow was not arrested.

M. Demarquay observes, that there was here not only dilatation of a lymphatic vessel, but also of the superficial network at two points of the thigh, viz: anteriorly and interiorly at the junction of the lower with the middle third, and on the anterior part of the middle third. The escape of the lymph, during nine hours, did not produce inconvenience; and up to the date of the report the disease had continued, and the flow of lymph had gradually increased.

Case 2.—This case was observed by Dr. Fitzer. A young lady, æt. 16, who had not menstruated, and had had crural hernia from the age of eight years. In 1847, she found that she had on her abdomen a number of small granular elevations; she was examined by Dr. Fitzer, who found a brownish stripe, three finger-breadths in width, commencing an inch below the umbilicus, to the left of the linea alba. It extended to the left and upwards, passing between the false ribs and the ilium, and ended at the dorsal vertebræ, becoming smaller and more transparent at this part. The middle part of this streak was prominent, and was formed of about eighteen papillary enlargements; some of these resembled the mamillary papillæ in men, others those in women; they were not tender to the touch, and disappeared on pressure. In July, the patient felt some pain in the situation of the streak; and on the 31st of that month, after returning from a walk, she found that there was escaping a milky coagulable fluid. She estimated the quantity which escaped at a quarter of a pint. Dr. Fitzer found the appearance of the parts the same as before. The fluid continued to escape during three days. It was milky, of a saline taste, alkaline, and escaped from two of the largest of the elevations which have been described. When one orifice was compressed, more lymph flowed from the other. Dr. Fitzer cut one of the elevations with curved scissors; he was able to pass a probe for an inch right and left; a quantity of lymph escaped from this incision. Pressure, and the application of

vessels; cases of lymphorrhagia from wounds have been recorded by Nuck, Van Swieten, and Assalini.

Dilatation of the lymphatic vessels has been observed by Baillie, Mascagni, Amussat, Breschet, Sir A. Cooper, Bichat, Sommering, Morgagni, Beau, Ricord; also, by Bidloo, Meckel, Rokitsansky, Albers, Andral, Otto, and Hasse.* But the facts have not as yet been sufficient to enable pathologists to determine the causes, forms, varieties, diagnosis, prognosis, or treatment of the disease. As far as M. Demarquay knows, M. Beau is the only surgeon who has attended to the treatment. In cases of dilatation of the lymphatics of the prepuce, he introduces a small seton into the dilated vessel, and removes it at the end of three or four hours. Obliteration is thus produced.

A new form of Splint for Fractured Thigh. By THOS. DARBY, M. D.—(Dublin Med. Press, in Ranking's Abstract.)—In a recent meeting of the Surgical Society of Ireland, Dr. Darby described a splint which he had used with decided advantage on three occasions. It consists of a plane, in the form of a tray, capable of receiving the trunk of the body, perforated with a commode-hole for the buttocks, and furnished with two prolongations for the legs. The ends of these prolongations have foot-pieces, with an adjusting apparatus for the purpose of extension. Counter-extension is made either by a padded strap, or by a fixed upright pin corresponding to the fork. *The pelves* and both the inferior limbs are fixed in this apparatus. Dr. Darby mentions three cases in which this contrivance answered perfectly; one of them, after two or three different kinds of inclined plane and straight splint had been tried without any benefit. One great advantage is, that the apparatus may be got up at half an hour's notice by any carpenter.

Depression of the Skull Relieved by a Novel Mode of Cupping. By S. NICOLLS, M. D.—(Dublin Med. Press, in Ranking's Abstract.)—Some time ago a child, æt. two years, was brought to Dr. Nicolls, suffering from depression of the skull, consequent upon a fall from a table. The symptoms of the compression of the brain were urgent, and of some hours' continuance. On examining the head he found a deep narrow depression, about three inches in length, extending from the lateral part of the occipital along the parietal bone, but without any wound in the integuments. It occurred to him to try to elevate the depressed bone by the application of a cupping-glass, and, with this end in view, he tried several glasses of various sizes, but owing to the form of the parts he could not fix any of them. At last it occurred to him to surround the depressed parts with an embankment of common glazier's putty, and to apply the glass upon this, when he at once succeeded, both in fixing the glass and in raising the depressed bone. The symptoms of compression passed off shortly afterwards, and the child recovered, without any ill consequences from the accident.

Local Anæsthesia—Employment of Hardy's Instrument.—(Med.

* Hasse's Pathological Anatomy, "Sydenham Society's Translation," p. 9.

Times and Gazette, in Med. Examiner.)—The first essay with this instrument was made by M. Nelaton, at the Clinique, upon a girl who was suffering from abscess of the axilla, and from a small wound upon the back of the wrist, both extremely painful. The instrument was composed of a caoutchouc reservoir of air adapted to a copper pump, made to receive the sponge for the chloroform. A valve at one extremity permitted the air to enter the instrument, which terminated in a caoutchouc tube. The first application of the chloroform upon the tumour of the axilla produced an insensibility which lasted three hours, during which time the part could be handled and examined with impunity. In the second essay, M. Dubois plunged a knife into the abscess, which was ripe, after the employment of the chloroformic fumigation. The patient declared that she was not conscious of pain, and became aware of the fact that the abscess was opened only by touching the part with her hand. From this time she had no more pain. The little wound on the wrist, fumigated in the same manner, remained quite insensible.

Local Treatment of Carbuncle and Furuncle.—(New-York Med. Gazette.)—In the *Association Medical Journal* for July 15th, 1853, there is an article by Mr. Richard Flint on the "*Local Treatment of Carbuncle and Furuncle*," and as these pests have been unusually prevalent during the past two years, it may be a source of relief both to physician and patient to lay his plan of treatment before your readers.

He takes the common Litharge Plaster, spread upon white kid, and of sufficient size to extend a half an inch or more beyond the circumference of the inflammatory areola, and fitted firmly, so as to entirely exclude the atmosphere from the inflamed tissues. He prefers to disturb this plaster as little as possible; and for that purpose, after the abscess has been opened, he recommends a crucial incision through which the pus may pass, to be absorbed by lint, or a folded cloth external to the plaster. Mr. Flint says truly, "that the lancinating, throbbing, intense pain of a carbuncle or boil will frequently be assuaged within a short time after its application, and the final discharge of the dead matter, and healing of the wound greatly facilitated; and that this method is in every regard preferable to the application of fomentations, embrocations and poultices."

Another method which I have tried, and, if adopted in the early formative stage of these tumours, have found usually to cure by preventing suppuration—or by producing abortion of the forming furuncle or carbuncle—is as follows:

Apply freely to the seat of the inflammation Lugol's solution of Iodine, until the skin is stained quite dark, and then coat the surface completely with Collodion, and repeat the process once or twice daily until

The *modus operandi* of the two plans I conceive to be somewhat similar, except that the latter possesses some advantages over the former.

They both exclude the atmosphere, and we all know how necessary that is in cases of burns and scalds, or other accidents which have denuded the surface of the cuticle.

The diachylon plaster must also tend, by its pressure, to constrict the bloodvessels, and thus check the influx of blood; but mild as it is when compared with other emplastie preparations, by applying it to a healthy surface, we perceive it possesses *some* irritating or nerve-stimulating properties, which must be injurious to a structure suffering already from active inflammation.

On the contrary, the *solution* of Iodine is but slightly and momentarily stimulant when thus applied, but soon of itself produces a cooling, soothing sensation, and the collodion, while it mechanically constricts the surface, completely excludes the atmosphere from coming in contact with the inflamed, excited nerve-extremities which are congested, and *burn* the parts in contact with them, as does the point of the conductor from a galvanic apparatus.

It may be that we shall never know clearly what agencies are at work, chemically and vitally, in the formation of these tumours, or in local inflammations; or what is the source of the intense pain and inflammation of a part when no longer protected by the cuticular envelope; but it seems to me that we already *do* have a sufficient insight into the physiological and pathological workings of the human machinery, to justify us in *attempting* at least to discover the *principles* which should govern us in the application of therapeutical agents; and it is for the purpose of asking for the principle on which the diachylon plaster and the collodion act as curative agents in these cases, that I have extended these remarks.

Truly yours,

C. H. CLEVELAND.

Philadelphia, Feb., 1854.

N. B. Will you favour me by making a trial of the *Ferruginous Collodion*, of *Mr. Aran*, in some of these cases? He and others speak highly of its efficiency in erysipelatous inflammations; and it is said to be more astringent and more adhesive than the common collodion. The surgeons of this city are much pleased with it.

It is prepared by combining equal parts of collodion and the ethereal tincture of perchloride of iron.

Impalement upon a Pitchfork-handle. By Dr. SARGENT.—(Amer. Jour. Med. Sci.)—1. The following case is recorded by Dr. Sargent. A lady, aged 37, slipped down upon a pitchfork-handle, which passed into the vagina 22 inches, and was supposed by Dr. Sargent to have

2. A case somewhat similar is related by Dr. G. S. Bryant. It occurred in the person of a negress, aged 25, who jumped down on to some fodder, in which was a tobacco-stick four-and-a-half feet long, and one inch square. The stick passed up the vagina, and on the right side of the os uteri, entered the abdomen, and finally lodged against the twelfth and eleventh ribs. There was considerable hemorrhage from the vulva, and, subsequently, severe constitutional disturbance, with symptoms of peritonitis. On the fifth night after the injury, enormous hemorrhage occurred from the bowels; the patient slowly rallied, being convalescent at the end of a fortnight, and perfectly well in a few weeks after.

An Operation for Uniting the Small and Large Intestines in case of Obstruction.—(Boston Med. and Surg. Journal.)—One week in the month of January, M. Maisonneuve, at Hospital Cochin, performed twice an operation which, he says, has never before been done upon the human subject. Whether this is true or not, I am unable to say; but I think it has not been done in Europe. The operation, which is simple, consists in uniting the small and large intestines, in case of obstruction, so that a new route may be established for the passage of the fecal contents. This surgeon has reflected some time upon this method, and various others, to see if there was not some way whereby a certain class of patients might have the benefit of an operation in the hope that life might be prolonged. Among this class may be found persons who have been submitted to the operation for strangulated hernia, and where sphacelus has attacked the portion of intestine contained in the sac to such an extent that it is not prudent to return it till the gangrenous portions have sloughed away. Again, when hernia has been reduced by the taxis, but from some cause or other the bowels refuse to act, notwithstanding the intervention of purgatives or applications to allay any inflammation that may exist in the portion of intestine which has been forced out of the abdominal cavity. Finally, in any intestinal adhesion, or in any case where there is obstruction in the digestive tube. To surmount these difficulties, it is proposed, as before said, to establish a new communication.

M. Maisonneuve has performed this operation many times upon dogs, by strangulating a portion of the intestine and uniting a portion above and below the point of strangulation, with complete success. But it must be taken into account, that upon dogs it is almost impossible to provoke peritonitis, however much the peritoneum may be mutilated.

to the left side, and an incision was made in the abdominal parietes, from five to six inches in length, on a line with the body, just above the crest of the ileum. The cœcum was sought as a fixed rallying point of departure. Then a portion of the intestine above the stricture, which can be easily recognized as being distended with fecal matter, was attained; also a fold below. These were then brought together and placed parallel to each other. The superior portion was incised, with a pair of scissors, the length of six inches, and the distended parts above carefully emptied of their contents. The lower fold was incised in like manner, but the bowel contained nothing to be disgorged. These tubes lying side by side—their internal walls were united by a simple suture—the serous surfaces being in contact, something like the seam of a garment before it has been pressed. Then the internal walls of each tube were brought together by turning the incised edges inward—so that the serous surfaces, as before, should be in juxtaposition—and united by a suture armed with two needles. Thus the free margins and the serous surfaces were in proximity, so that agglutination might take place with facility. This being done, the intestines were returned, and the external incision dressed in the ordinary manner. The patient lived seven days. The fifth day he had free evacuations, whether from the new or old route I cannot say, but he finally sunk from peritonitis.

The second patient was a man who entered the Hospital with strangulated hernia, of eight days' duration. The abdomen around the hernial tumour presented a yellowish tint, showing that evidently there was already sphacelus in the constricted intestine. The operation for hernia was made, and the bowel was found too gangrenous to be returned into the abdomen, but was opened to give exit to a large amount of fecal matter, liquid, &c., and left in the incision. Two days after, the patient having had no discharge, and being almost in a state of collapse, the same operation as mentioned above was performed, but without any particular hope of success.

Notwithstanding these unfavourable results, I presume M. Maisonneuve will make other trials, when cases present themselves. The philosophy of this operation is simple, and it may lead to some beneficial results. Supposing the obstruction of the intestine should exist, and the operation should succeed, the patient's life might be prolonged by this new communication established in the intestinal tube. Or should the latter be successful, and the impediment be removed by the efforts of art or nature, the patient would be favoured with two routes for the circulation

PATHOLOGY AND PRACTICAL MEDICINE.

On some of the Signs of Incipient Pulmonary Phthisis.—By M. BOURDON.—(Virg. Med. & Surg. Journal, from Archives Gènèrales).—It is known with what difficulty the presence of pulmonary tubercles is diagnosticated when the disease is in its incipency, and how insufficient the stethoscopic signs are. The latter may be wanting when the lungs contain a large quantity of tubercles ; they may exist when the lungs contain none ; and they may be masked by different sounds. The general symptoms are not a sure means of detecting the disease ; a simple catarrh may be attended with very grave symptoms, while a true phthisis may affect the general system very slightly. It is at the commencement of the disease that the diagnosis is most important, because this is the only time there is a chance of treating it with any success. M. Bourdon has sought to elucidate this obscure point. Attracted for a long time by the frequency of certain symptoms in phthysical patients, he has studied them with care, in order to establish their value as signs. These symptoms are arranged under many heads, according to the organs in which they show themselves : 1. Gastric symptoms. 2. Hepatic symptoms. 3. Thoracic pain. The gastric symptoms are not to be confounded with the morbid phenomena observed in almost all diseases. Those which coincide with the incipency of phthisis are nausea, vomitings, epigastric pains, dyspepsia. You must not wait to find these symptoms in every case ; nevertheless, they are extremely frequent, since M. Bourdon found them in more than two-thirds of his subjects. Some phthysical patients suffer from one of these symptoms ; others, on the contrary, have them all combined. Nausea and vomiting are most common ; and it is generally after the cough that they exhibit themselves. It must be observed, however, that their frequency or intensity are not at all proportioned to the cough ; they occur when the stomach is empty as well as after a repast. Generally the vomitings consist of mucous matter, rarely of bile. Still, while they show themselves with a large number of the tubercles, they do so in a positive manner with very few. Another very common symptom is the epigastric pain. In the largest number of cases, it is accompanied with nausea and vomiting ; nevertheless it occurs alone sometimes. With the largest number of patients it is only manifested upon pressure, or is so slight as not to be noticed by the patient until their attention is attracted to it ; in a smaller number of patients it manifests itself spontaneously. Although

2. The liver sometimes presents remarkable peculiarities. In half the patients examined by M. Bourdon there was an increase of volume, frequently attended with pain. The increase of volume seemed to be particularly observable in the right lobe; the consistence of the organ seeming to undergo no change. The changes of sensibility are less frequent than the changes of volume. Spontaneous pains are rare; they only manifest themselves under pressure, or under some sudden and straining movement. In some rare cases this change of sensibility is observed without change of volume. As to the change the liver undergoes, the autopsies prove that in more than half of the cases it is due to fatty degeneration more or less advanced. The bile is generally of greater density, and more highly coloured.

3. The largest number of phthisical patients suffer from pain between the shoulders and in the sides of the chest; but, besides these spontaneous pains, there are others to which M. Bourdon calls the attention of physicians, and which are only recognized through the medium of percussion. The points where these pains are discovered are numerous, but they are most often seated below the clavicles, and only on the side affected with tubercles. If both sides are affected the pain is on the side most affected. As to the cause of this pain, M. Bourdon, like M. Beau, is inclined to ascribe it to intercostal neuralgia.

The conclusions drawn by M. Bourdon from his researches, are as follows:—When prolonged dyspepsia, nausea and vomitings are observed, without any assignable cause; when the liver is abnormally developed, without there being hepatitis or disease of the heart; when these phenomena are exhibited independent of any other affection, or in the course of chlorosis, or after measles or typhoid fever; the physician should turn his attention to tuberculosis, examine the patient with great care, and even when there is doubt of the reality of the pulmonary affection, he ought to act as if convinced, or at least he should take his precautions.

Medical Properties of Nitrate of Soda. By Dr. J. B. Brown.—(North-Western Med. & Surg. Journal.)—This article, which, in many works on *Materia Medica*, is not even mentioned, seems to be possessed of qualities too valuable to be entirely passed over, or, as is done in some books, merely quoted for the purpose of calling its name. As I have made use of this drug with much success in my own practice during the last two years in this country, I deem it a duty to communicate the results to our profession.

The first notice of the physiological action of this salt I found in a medical journal for 1843, in which Mr. Zimmermann published the results of his experiments. It is as follows:

“Nitrate of Soda dissolves the protein element of the blood much

this article to any very great extent as a therapeutical agent. As he attributes to it the most extraordinary properties in different diseases, claiming it as a universal remedy, and recommending it in the most heterogeneous affections without stating any particular indications for its use, I was induced, two years since, to make some experiments myself, for the purpose of ascertaining whether the medicine possessed the value which its friends had claimed for it. Rademacher thinks that it is more useful in gastric fevers than Nit. Potassæ, which as an active solvent of fibrine, causes a more rapid putrescence, and which in cases where the a priori zymotic tendency is evidently contra-indicated, whilst the Nit. Soda, although it restrains the pseudo-plastic process, does not produce any excessive evacuations.

When given as a gentle laxative this salt is easily borne, but when given in large quantities it produces fluid passages with tenesmus. Nevertheless, according to my experience with this remedy in the prevailing diseases of this country, such as irritation of the mucous membrane of the intestines, and especially in acute or chronic dysentery, its value can scarcely be surpassed by any other agent of our materia medica. No remedy has so rapidly succeeded with me in restoring natural passages, and relieving the intense suffering in the worst forms of this annual complaint, as Nitrate of Soda. Most of my cases I had to treat only four or five days, when all the symptoms under the influence of this specific, if I may use this term, disappeared entirely. Other cases, in which a cure was not so speedily brought about, were comparatively rare, and I found this fact generally owing to a fault of diet, or some other unfavourable circumstance of the patient. Nevertheless, I have been so fortunate, as never to treat any case of dysentery longer than at most for a fortnight, whilst out of six cases I generally restored five of them to health, within the above mentioned limited space of a few days. It is not my purpose to enter into a detail of the treatment of this disease, but I will confine myself here to a single remark, that of all the remedies in use against it, there is no one existing, to my knowledge, safer, quicker and more innocent in its agency, than the one under consideration.

The formula I usually employ is this :

R̄	Ol. Amygdal. dulc.	3 iv to vj
	Gum. Mimos.	3 ij to iij
	Aq. distillat.	3 jr to vj
	Fiat lege artis emulsio, cui adde	
	Sodæ Nitratis	} 2ss
	Aquæ lauro-Cerasi	} 3ss
	Syr. Simp.	3j

M. S.—A table-spoonful to be taken every one to two hours, after having been well shaken.

For children I prescribe the half or only third part of the quantity, and I found it an excellent means for overcoming in their different forms diarrhoea and dysentery, at least for the purpose of diminishing the irritability of the bowels, after which other remedies, according to the indications of the case, might be made use of.

After the evacuations have become normal, the medicine is to be con-

tinued for one or two days, on account of a great liability to relapse, which not unfrequently may very disagreeably surprise both the patient and physician. But this disposition is owing to the disease itself, as every practitioner will agree, and not to the specific effect of Nitrate of Soda. In some cases of the most severe characters I ordered the addition of a few grains of the Extract of Opium to the former mixture, in order to allay more rapidly the extremely painful tenesmus, and the nervous excitement in general. I prefer the extract to the opium in substance, because it has a milder effect, quieting the system more, and not producing that peculiar venous orgasmus, which opium itself does. But I must remark here that even this milder preparation of opium ought never to be used for several days in succession, for reasons I can not now explain, being beyond the limits of my present purpose. One of my patients, who suffered in a dreadful manner, after taking a mixture of brandy with pepper, in order to stop the whole matter at once, as he thought, had to take the above described portion four times one after the other; within seven days he was perfectly restored.

I have been now in more than a hundred cases of dysentery so successful, as to restore my patients within the period of from 4 to 14 days at most, under the principal influence of Nitrate of Soda, never using a single grain of calomel, which I avoid for fear of salivation and of its consequence, these being often as bad a complaint as the former, or even still worse. I would therefore recommend this salt as a very powerful and innocent substitute for calomel in all the different forms of dysenteric or congestive diarrhoea and dysentery.

I would mention, *en passant*, that tonics or astringents are to be employed, whenever, after the disappearance of the characteristic mucous discharges, a tendency to a diarrhoea from atony manifests itself, either spontaneously or in consequence of an improper diet.

Cases Illustrative of the Effects and Manner of Action of Particular Remedies in Diabetes.—By Dr. W. R. BASHAM. (Am. Jour. Medical Sciences, from London Lancet.)—Dr. W. R. Basham has published (*Lancet*, Nos. for Jan. 21 and 28, 1854), five cases of diabetes, to illustrate the effects of some remedies recently suggested for diabetes. The principle upon which the several remedies were employed rests on the opinion that diabetes is a disorder of the digestive and assimilative functions, in which the power of conversion and appropriation of the farinaceous and amylaceous elements of food is singularly perverted and disturbed. These alimentary principles are more rapidly converted into sugar or glucose than in healthy digestion; immediate absorption by the venous capillaries of the stomach follows, the further stages of oxidation are abruptly arrested, and the glucose, quickly passing into circulation, is, without further metamorphosis, excreted by the kidneys. I postpone, also, to another opportunity, any reference to the interesting question, whether all the sugar passed by diabetic patients is solely derived from vegetable food, or whether the tissues of the organism, as well as the nitrogenous elements of animal food, may not also contribute to the formation of the large amount of saccharine matter excreted in glucosuria. The regimen in the following cases consisted in diminish-

ing, as far as possible, the supply of vegetable material containing fecula or starch, the mass of nutriment being derived from the nitrogenous class of aliments. The medicinal remedies may be classed as follows, the more novel ones being placed first :

1. The permanganate of potass, hypothetically to supply the stomach with an increased amount of oxygen, by which the metamorphosis of the farinaceous material should be hastened forward into a higher state of oxidation than that of sugar, permanganate of potass, as is well known, out of the body, converting sugar into oxalic acid. Mr. Sampson, who first employed it, states (*Lancet*, Jan. 8, 1853), that his attention was drawn to this salt when seeking for some remedy which should give out oxygen when taken into the stomach, with the view of assisting the imperfect action of the digestive and assimilative functions. He records a case in which it appeared beneficial. We must not overlook the fact, however, that a prominent error in the digestive process of the diabetic patient is the premature and rapid conversion of the fecula of food into sugar ; it is hasty and imperfect—not tedious or protracted.

2. Agents that, hypothetically, should retard and delay the formation of glucose in the stomach. Certain substances possess the property of arresting the saccharine, vinous, and acetous fermentations. If glycerin be added to a half-fermented infusion of malt, the further formation of acid is checked ; and I have found that a mixture of potatoe starch and dilute nitric acid undergoes chemical conversion into dextrine and glucose more slowly, and is all but arrested, if glycerin be previously mixed with the starch. Glycerin, creasote, and sulphite of soda have been tried on this principle.

3. Opium and opiates, to act on the nervous system ; to diminish the excitement and irritability of the nervous centres.

4. Remedies which relieve thirst, and aid the digestive process by the supply of hydrochloric acid, and which tend indirectly to diminish the amount of fluid excreted by the kidneys, as recommended by Dr. Owen Rees. (*Medical Gazette*, vol. xl. p. 365.) Hydrochloric and some vegetable acids.

5. Diaphoretics, stimulating the palpably defective cutaneous secretion, and thus vicariously lessening the proportion of fluid to be excreted by the kidneys. Antimonials ; warm baths ; flannel clothing.

6. The use of alkalies, particularly ammonia ; on the hypothesis of Mialhe, that the starch of food is equally converted into sugar by healthy and diabetic persons ; but that in health it is metamorphosed and burnt off by the presence of alkalies, undergoing complete oxidation, and is ultimately discharged as carbonic acid from the lungs ; but that in diabetes it is not oxidized, owing to the deficiency of alkalies in the blood, the sugar, without further change, passing off by the kidneys.

These several remedies have never been administered in conjunction ; and if, in the same case, two or more have been tried, a day or two has been allowed to elapse, that the observation recorded might be fairly deduced from the remedy employed.

The results were as follows :

- 1st. The permanganate of potass was given in two cases ; during its administration, the amount of sugar excreted gradually increased, al-

though the fluid amount of urine became somewhat less, and the thirst appeared to be alleviated. No inconvenience attended its use; ten-grain doses were taken without any unpleasant effects on the digestive organs; indeed, it was thought that some benefit arose from it, as the fulness and eructations, in one case, seemed relieved by it; but, during its administration, the ratio of the sugar steadily increased. This occurred equally in both cases; the symptoms of each differed but little in intensity. There was but a slight discrepancy in their several ages, and in both the disease was unaccompanied by any pulmonary complication, so that there was scarcely room for a doubt that the increased amount of oxygen supplied to the food by the permanganate of potass facilitated the formation of sugar, and did not, as hypothetically inferred, advance the chemical conversion of the glucose into the stage of acid metamorphosis. Dr. Wood, of Philadelphia, has tried yeast in diabetes, on the principle here enunciated, that, as it converts sugar out of the body into acid products, acetic and carbonic acids, it might bring about analogous changes in the stomach. On a like principle, Dr. Gray, of Glasgow, has tried rennet, which converts sugar into lactic acid.

2dly. From the operation of the agents of the second class, administered on the hypothesis of their possibly retarding the conversion of the amylaceous elements of food into sugar, we can deduce only negative results. They were tried only in one case, and, during a period of 21 days, the amount of sugar was only faintly diminished, the specific gravity falling from 10.44 to 10.40, the average daily amount of urine remaining the same. The case in which these remedies were tried was one of great severity, and ultimately proved fatal; yet, notwithstanding other remedies succeeded in reducing the amount of sugar, though only temporarily. Although glycerin and sulphite of soda failed in producing any effects in this case, I am nevertheless desirous of again submitting these remedies to further trial, and testing, by the evidence of more extended observation, the fallacy or otherwise of their hypothetical agency.

3dly. *Opium and Opiates*.—These cases afford but a limited amount of evidence on the action of these agents. Opium certainly operated as a palliative; the thirst became much relieved, the amount of urine diminished, and the skin, by the presence of sudoresis, indicated a relief to its obstructed function; but the daily average amount of sugar excreted was not materially lessened, and the physical condition of the patient was not improved. Some constitutions will bear opium much better than others, and it must not be inferred, because these cases do not exhibit its agency in a more favourable light, that opium may not in other instances produce more remedial effects.

4thly. *Hydrochloric Acid*.—The action of this mineral acid appears in a favourable light in one case. It promoted the digestive function, relieved the flatulence, and probably furnished an important material to the solvent functions of the stomach. In other forms of dyspeptic derangements, its agency is familiar. It should always be taken some few minutes before food.

5thly. *Diaphoretics*.—These may be administered in conjunction with opium. The suppressed function of the skin is so very evident in

all cases of diabetes—becoming harsh, wrinkled, and furfuraceous, patients seldom perspiring, and relief being always apparent so soon as any moisture is obtained on the surface, that remedies which excite or assist in promoting cutaneous excretion are always more or less indicated. Opium itself tends to promote diaphoresis, even when given alone, and in its action in this respect may be much increased by combining it with antimonials. Flannel clothing should be strictly enjoined. Several of these cases illustrate the advantage of warm baths in conjunction with these agents.

8thly. *Ammonia and Alkalies.*—The testimony of almost all writers on this disease is in favour of the remedial power of alkalies, particularly of the carbonate of ammonia; and the cases just detailed corroborate the opinions of the most experienced physicians on their efficacy. The fifth case presents the most satisfactory proofs of this plan of treatment, as the patient left the hospital temporarily cured. Of the mode of action of alkalies in this disease, little is known beyond what is hypothetical. Mialhe states that the blood in diabetes is deficient in alkaline salts; and he affirms that the ultimate conversion of the sugar formed out of the food, into products capable of being eliminated by the respiratory function, is not effected in consequence of the deficiency. To supply this defect should be the leading principle in the treatment of glucosuria. Whether we adopt this theory or not, the fact remains indisputable, that a larger amount of relief is obtained by a steady and persevering use of ammonia and alkaline salts, than can be procured by any other class of remedies. However, to render them efficient, a well-regulated diet must be rigidly followed, and this should be limited as much as possible to animal or nitrogenous food. In the opinion of Bouchardet, clothing ranks next to diet. Moreover, the intelligent co-operation of the patient is absolutely necessary; for, unless, he can be made to understand and enter into the object for which so strict a diet is prescribed, the effects of the alkaline plan of treatment will prove uncertain and unsatisfactory. The progress of cases in private practice is, for the most part, always more satisfactory than among hospital patients, principally for this reason—that intelligence lends force to the efforts of self-denial, and develops a more powerful control over the appetites and habits. The less educated are but little inclined to abstain even from things which they know to be positively injurious, and they with difficulty can be brought to comprehend the necessity for refraining from bread and vegetables, which their necessities have always taught them to be the staple articles of their food.

I cannot take leave of this subject of diabetes, without referring to

Times and Gaz., Feb. 4, 1854) makes the following remarks on this subject: "M. Bouchardat has long recommended claret in this disease, and to the amount of two bottles even in the day. Dr. Prout used to permit his patients to take porter. Being desirous of determining which of these directions was to be preferred, I made some experiments regarding the amount of saccharine matter and acid in wine and beer. I have come to the conclusion, that claret is usually perfectly free from all saccharine matter. The same may be said of Burgundy, Rhine wine, and Moselle wine. Very rarely, indeed, a sherry may be met with which contains no sugar, but generally sherry, port, Maderia, Marsala, contains amounts of sugar varying from 2 grains to 34 grains to the ounce. The best porter gave me from 23 grains to 40 grains of saccharine matter to the ounce; the best stout, from 45 grains to 64 grains; the best bitter ale, from 14 grains to 130 grains to the ounce. Hence it appears that claret will not increase the sugar in the urine, while porter will do so considerably. Direct experiments with diabetic urine has also proved the same fact.

"Spirits, as brandy, whiskey, rum, usually contain no traces of sugar, and hence they may be prescribed for diabetic patients.

"M. Bouchardat, who has seen more of this disease than any one, says that milk is not allowable. The most careful experiments lead me to the opinion that milk scarcely increases the sugar in the urine, even when it is the sole article of diet."

It is most probable that milk-sugar is not easily converted into glucose in the human body. I have, therefore, constantly permitted it to be taken by diabetic patients. It is apt to give rise to constipation, and this has been best obviated by taking an equal quantity of fluid magnesia with the milk in the morning.

One of the terminations of diabetes is by phthisis, and on this account, as well for the emaciation, Dr. Jones was led to prescribe cod-liver oil. To a young woman in St. George's Hospital he gave an ounce daily for three months. In this time she gained eleven pounds in weight. Her thirst so diminished that she passed only two and a half pints of urine in twenty-four hours, instead of twelve pints.

"A man, 40 years old, came into the hospital, passing seven pints of urine in twenty-four hours. He was put on cod-liver oil, one and a half ounce in the day, with a drachm of liquor potassæ. In the first fortnight he lost four pounds and a half. He then took three ounces of oil in the day, with two drachms of liquor potassæ, and in a fortnight he had gained eight pounds. The quantity of urine was reduced to three pints. At the end of five weeks from his admission, he went out at his own request, saying that he was stronger than he had been for eight months, and that he was quite well. The urine still contained a small quantity of sugar."

An enormous amount of cod-liver oil may be taken in diabetes; thus, one of Dr. Jones' patients took, in five weeks, eight pints, fourteen and a half ounces.

On first entering the hospital, the patient lost eight pounds in weight; while taking the oil in large quantities, he regained this and one pound more. The urine, with the diet, decreased from five pints, specific

gravity 1040, to two and a half pints, specific gravity 1032. The sugar never disappeared from the urine.

Dr. Jones has tried various specifics that have been from time to time proposed. Permanganate of potassa, rennet, so called pepsin, arsenic, electricity, Vichy water, creasote, opium, bark, iron, mineral acids, alkalies, and many others. Hitherto, the search for a specific has only proved that none is yet known to exist.

"Still," he says, "when we look at the morbid anatomy, when minute microscopical examination can give no clue to the seat of the disease, when it pronounces that the stomach* and viscera are healthy, when it indicates that a functional error has ended life, we are led to hope that further investigations (some remarkable experiments have lately been published by Dr. Harley on the artificial production of diabetes in animals, by the action of stimulants injected into the vena portæ) may lead to the perfect theory of the disease, and by this to the radical cure, instead of the palliative treatment, of this most interesting disorder."

Dropsy in Relation to Treatment. By Dr. BARCLAY.—(Ibid.)—Dr. Barclay read a paper on this subject before the Western Medical and Surgical Society, January 20, 1854. He commenced by alluding to the high mortality of this disease, and stated that the only disease which at all approached it in this respect was phthisis. He drew his conclusions, and carefully illustrated his views, from the Medical Registries in St. George's Hospital during the years 1851-52. He then restricted the term dropsy to anasarca and ascites, considering that as hydrothorax, hydrocephalus, &c., when not presenting themselves as mere isolated portions of general dropsy, are practically found only as the result of inflammation, they should be classed accordingly. Although the distinction between anasarca and ascites is generally clear, yet in many cases both forms are present, but a little care will generally discern the primary form. The morbid states, giving rise to ascites, are much more fatal than those associated with anasarca; more than two-thirds of those admitted with the latter form being discharged from the hospital cured or relieved, while less than one-third of those affected with ascites reap the like benefits; but here we should bear in mind that many anasarcaous patients return again and again to the hospital to be relieved of the same set of symptoms. Though the actual frequency of, and, consequently, the actual number of deaths from, anasarca is far greater than from ascites, yet the ratio of mortality in the latter form is exactly double that in the former. With regard to anasarca, though by far the greater number of cases are associated with disease of either kidney or heart, yet certain cases will compel us to attribute the disease to some other cause. During the two years alluded to, nearly one-fourth of these cases could not be accounted for. The most frequent association of this disease is disease of the kidney, with or without disease of the

* My friend, Dr. Handfield Jones, has lately examined minutely for me the stomach of a patient of mine who died of diabetes, in St. George's Hospital. It appeared to be healthy.

heart; next comes disease of the heart itself. But though these lesions are apparently so evident a cause for serous effusion, yet, upon further analysis of these cases, there will be found by no means such a direct connection between the disease and symptom, except where both organs were implicated. This assertion is borne out by the fact that, in the two years before mentioned, but a few cases of diseased kidney, and still fewer of diseased heart, presented no other malady of sufficient importance to find a place in the register of disease; and to every one conversant with the disease, the pale face of albuminuria, and the dusky hue of impeded circulation, point out something beyond the mere change of structure—some change in the blood itself—to be one of the necessary associations of dropsy. We found also that 63 per cent. of the patients labouring under disease of the kidney, and 42 per cent. of those with disease of the heart, have at the same time been affected with anasarca, and that, in some cases, the only other complication found was bronchitis with emphysema. All which facts go to prove that disease of the heart and kidney may go on for years without causing dropsy, until the mucous membrane of the lungs becomes congested, and that serous accumulation follows. The explanation then is found in the obstruction that takes place in the circulation through the lungs, and secondarily in the effects of a chronic bronchitis on the constitution. Next to bronchitis, phthisis stands as a cause for dropsy, its degree probably being regulated by the amount of night-sweats, which draw off, to a certain extent, the serous accumulations. In other cases, anæmia goes to prove that some state of the blood is an essential element in the occurrence of dropsy.

In regard to cardiac disease, valvular lesion is the most common cause of dropsy, and hypertrophy the least so, which shows that the production of dropsy depends upon some failure of the *vis à tergo*—a view borne out by a careful analysis of the various cases. In mitral disease, the danger does not depend upon the loudness of the murmur, but upon the feebleness of the pulse, and in aortic disease dropsy seldom occurs until regurgitation is established. An increased circulation alone, too, is barely a cause of dropsy, unless some obstruction exists to the circulation of the blood, or there be an abnormal tendency to transudation of serum. Bronchitis, whether due to congestion of the lungs, from mitral regurgitation, or from exposure to cold, is one of the diseases which act in this manner. A similar (but a more fatal) result is

tous lungs, leading one to the supposition that either atrophy or dilatation of that organ existed.

3. That a mitral murmur may be covered by a turbulent action of the heart.

4. That cases occur in which regurgitation through the mitral valves depends upon hypertrophy, a condition leading to no error, as the results are the same whatever the cause of the regurgitation.

The relation of kidney disease to dropsy was then discussed, in which he stated that each stage of the disease was marked by a peculiar state of the urine. In the early stage, the urine is generally clear and free from albumen, though this condition of the secretion sometimes is found in advanced stages of the same disease, which must make us careful in our examinations of the urine, and in forming our suspicions as to the degree of renal change. In the congestive stage, albumen is present, and in acute cases of dropsy, as after scarlatina, it is found in small quantities only in connection with an abundant supply of lithates. In the hemorrhagic stage, the albumen is most abundant, but will be found to bear no relation to the blood passed. Diseased kidney acts in the production of dropsy in two ways—by suppression of urine, and by causing a drain of albumen and salts from the body, in consequence of which the blood becomes impoverished, and, when the disease of the heart is present, it is not difficult to see this morbid state act with increased energy in company with albuminuria. In ascites there is more obscurity, and though the dropsy may arise from obstructed circulation through the liver, it may also happen if the mutual balance between absorption and secretion of the peritoneal surface be destroyed. But the dropsy may be due to some general disease of the system, and be associated with anasarca, in which case some distinct cause will be found for the latter. A diseased state for the peritoneum, including inflammation and malignant conditions of that membrane, may also cause ascites, and, as either may be associated with renal or cardiac disease, it is clear that all such cases are connected with and dependent on some obstruction to the onward current of the blood. As dropsy seldom depends upon the morbid state of any organ alone, but generally in connection with some functional disturbance of it for the time being, these super-added causes are amenable to treatment, though the original one may not be. In simple, uncomplicated anasarca (as after scarlatina) we have simply to counteract the suppression of urine, but in other cases we must improve the tonicity of the blood and system generally. When the kidneys are congested, and effusion is an immediate result, we should use the intestines and the skin as our channels for the discharge of the serum, rather than the kidneys. In complicated cases, we often have irritation to soothe, and inflammation to subdue, before the dropsy be attacked. In chronic bronchitis, in connection with these cases, we

in cases of dropsy, which was strongly recommended by some members present.

On Insufficient Alimentation, and the Value of Phosphate of Lime in Nutrition. By M. MOURIÈS. (Virg. Med. & Surg. Journal, from Bull. de l'Académie, &c.)—In the *Bulletin de l'Académie Impériale de Médecine*, for January, 1854, we find a report by M. Bouchardat, on the researches of a young and learned chemist, M. Mouriès, in regard to the effects of phosphate of lime in the nutrition of animals, and the influence which the judicious employment of this salt is capable of exercising upon the mortality of children in large cities.

It has been a comparatively short period since physiologists began to appreciate properly the importance of inorganic principles in the phenomena of life. The farther we penetrate into this complex problem, the greater is the importance attributed to bodies, the presence of which in the human organism was regarded as quite accidental.

Very dissimilar organic compounds may be substituted for each other in our diet without any disorder in the general harmony, but the inorganic principles can only be replaced by substances very closely analogous to them. Albumen, fibrin, and casein, and other more complex aliments, though differing in origin and composition, may fulfil the same physiological end, but it is different with inorganic principles. Lecanu has shown that iron is indispensable for the proper constitution of blood-globules; chloride of sodium is of primary importance also as a constituent of the liquor sanguinis, and it is only as an exception that we find, in certain granivora, this salt partially replaced by the phosphate of soda or of potash. Liebig has shown that the chloride of potassium of the muscles cannot be replaced by chloride of sodium. Each inorganic constituent of the organism has, therefore, its definite and limited sphere of action, to which it is exclusively adapted.

Among the indispensable inorganic salts, the phosphate of lime holds an important rank. M. Mouriès has devoted himself to the elucidation of its peculiar action. He deduces from his experiments the following conclusions:

1. Phosphate of lime plays a more important part in nutrition than has heretofore been believed. Independently of its necessity as a constituent of bone, this salt maintains that irritability without which there is no assimilation, and consequently no nutrition. Its insufficiency, therefore, produces death with all the symptoms of inanition, while its insufficiency in a less degree, produces a series of lymphatic diseases.

2. The food consumed in cities is deficient in this respect. Nurses' milk has, consequently, the same defect. The infant as well as the foetus suffers from the deprivation of this element so indispensable to its development and life. Hence one of the causes of the increase in the number of still-born children, and of the mortality of infancy.

3. The addition of this salt, in combination with animal matter, to alimentary substances, obviates one cause of disease and death.

The following are the principal facts on which M. Mouriès relies to establish these conclusions:

The blood of animals contains a constant proportion of earthly phos-

phates, which is independent of their ingesta. The pigeon ingests phosphate of lime slightly in excess, in the grain and calcarious gravels which it picks up; the horse swallows an excess, in its fodder; the dog procures a still greater excess from the bones on which he is fed; and yet the blood of the pigeon contains in 1000 grammes 1.20 of phosphate of lime; the horse 0.5; the dog 0.4. This result is not accidental; all birds whose blood has been analysed have 1.5 to 1.2 of phosphate of lime, while the proportion in the blood of the carnivora and herbivora varies from 0.9 to 0.4. The proportion thus regulated by nature, is modified by age and sex. The bull, cow and calf have the same food, yet their blood contains respectively 0.5, 0.9, 0.8 of phosphate of lime.

The requisite proportion of alkaline phosphates varies, therefore, in different animals. A pigeon weighing one pound died at the end of ten months, during which period he was fed daily on one oz. of wheat, with common water for a drink, by which rather more than a grain of phosphate of lime was ingested daily: on the other hand, a woman weighing 100 pounds enjoyed perfect health upon a diet which furnished her daily with 90 grains of phosphate of lime. Thus health in the one case, and death, in the other, with relatively equal quantities of this salt.

We shall recur to this example to show how complex are the conditions of these experiments, and what reserve is necessary in drawing conclusions from them.

M. Mouriès asserts, and the fact has already been noted by Chossat, that if the proportion of alkaline phosphates of the food is deficient, there ensues atony of the digestive organs, imperfect assimilation, and death. To prove that pigeons die from want of phosphate of lime, we may observe that their death is hastened if they are allowed only distilled water, while their lives may be preserved by adding earthy phosphates to their food.

M. Bouchardat observed that the grain on which M. M. Mouriès and Chossat fed their pigeons contained only traces of common salt. The birds therefore should be expected to suffer from the deprivation of this principle. M. Bouchardat accordingly made this experiment; he confined two pigeons, and fed them on dried grain. In two months the health of the female became impaired; she suffered from thirst and diarrhoea and laid no more eggs. She was set at liberty. She flew immediately to a window-sill impregnated with alkaline chlorides, and began to peck eagerly; there was a larger quantity of salts on the interior of the window frame; the pigeon entered through the open window, and permitted herself to be re-captured, so imperious was her demand for these principles. Her health was re-established; in three days she laid another egg. It is wrong, therefore, to conclude with M. Mouriès, that a deficiency of phosphates is the only cause of the symptoms he observed; in this case, the absence of chlorides was the obvious cause.

M. Mourès has established, by interesting calculations, that grain furnishes a sufficient supply of phosphate of lime for the reparation of bone, but not for other essential functions of the economy. From the curious fact that there is a constant proportion between the temperature of animals, and the amount of phosphate of lime contained in their blood,

he deduces the principle that this salt keeps up animal irritability, without which nutrition is impossible. The following table must interest physiologists :

	PHOSPHATE OF LIME.		TEMPERATURE.
	Mouriès	Poggiale.	
Blood of the duck,	1.50		42°5 cent.
— the hen,	1.35	1.25	41°5 "
— the pigeon,	1.20	1.23	40° "
— man,	0.80	0.6	37°5 "
— the horse,	0.40	0.5	36°8 "
— frogs,	a trace.		9° "

If these results are confirmed, it will appear that the ingestion of phosphate of lime is not only indispensable for the reparation of bone, but that it is connected with the function of calorification.

In the second portion of his memoir, M. Mouriès, starting from the principle demonstrated by Chossat, verified by Boussingault, taught by Berard, and now admitted by all physiologists, that diet is defective which does not contain enough phosphate of lime to repair the waste which is continually going on in the economy, attempts to prove that the food commonly consumed in cities does not contain the quantity of this salt which is required by nurses and pregnant women.

He commences by calculating the quantity of phosphate of lime which ought to be ingested in the twenty-four hours, which he estimates from analysis of the excreta at 110 grains. He then attempts to show that this quantity is not contained in the food of nurses in cities. The urine of women in the country contains 90 grains of phosphate of lime in the 24 hours, while the amount of this salt in the urine of women in cities varies from 20 to 90 grains. M. Mouriès has sought to confirm his hypothesis by direct proofs; he has examined the food consumed in cities, and shown that it exhibits a deficiency of one half in alkaline phosphates. He has examined the milk of nurses, and shown that in 18 healthy country women the proportion of earthly phosphates in the milk varied from 1.2 to 2.4 *per cent.*, while in the milk of 10 Paris nurses the proportion varied from 0.5 to 0.9, and in seven others, there was only a trace of phosphate of lime.

In the third portion of his essay M. Mouriès adduces clinical facts in illustration of the advantage of supplying this deficiency of phosphate of lime in aliments. In 18 cases, in which the proportion of phosphate of lime averaged 0.7, 75 grains of this salt with twice that quantity of albumen was daily administered in soup; in a week the proportion of earthly phosphate in the milk rose to 2.1. In 5 cases pregnant women were subjected to the same treatment; the milk, after delivery, contain-

alarmed at the application of chemistry to medicines, especially when its results were as inoffensive as those he had discussed.

On the Prophylactic Influence of Quinine. By Dr. A. BRYSON. (Ibid, from Med. Times and Gaz.)—The benefit to be derived from the regular use of the vegetable tonics as preventives of fever in malarious climates, has long been known in our Southern States. In the eastern parts of Virginia, where, during the autumnal months, intermittent and remittent fevers are rife, it was the custom of the people to have prepared a daily drink, generally of a spirituous character, but containing either some of the varieties of mint, or some of the vegetable bitters, as gentian, chamomile or cinchona, of which the whole family partook, and to the use of which was attributed their exemption from the endemic fevers of that period of the year.

The introduction of quinine into general use, has, to a great degree, done away with this custom, for it was soon found that though the juleps and bitters of old times were much more palatable, yet that the quinine taken in the same way and at the same time, was a more certain prevention, and consequently it is generally the habit now to give once or twice a week that alkaloid in five or ten grain doses to every member of the family, and that the practice is efficacious in the highest degree, is undoubted.

We notice, in the "Medical Times and Gazette," for January, 1854, a report of Dr. Bryson, of the British Navy, on the prophylactic influence of quinine, when used by the seamen belonging to the men-of-war on the East Indian and African stations, where are met with those malignant fevers so fatal to the white man. He states that it had been a long standing rule in the Navy to give to the men sent ashore in these tropical climates, a drachm of powdered bark, either in wine or spirits, previously to leaving the ship in the morning. This practice, although acknowledged to be in some degree useful, had fallen into disrepute, partly because the preparation was nauseous, and more because the dose was too small to act completely as a preventive. Dr. Bryson, therefore, in the year 1847, in drawing up a report on the African station, advised that a spirituous solution of quinine, in the proportion of four grains of the salt to an ounce of wine, should be included among the medical stores of each ship, and that an ounce dose should be administered to each sailor whenever exposed to the exciting causes of fever. Experience soon taught him that it was necessary to keep up the prophylactic for some time after exposure, and he hence continued its use for at least fourteen days after their return on board, in order that the antagonistic influence of the medicine might be maintained, until the incubative period of the disease had expired.

The results on the whole were most satisfactory. Reports from the medical men attached to the various cruisers on the coast of Africa, speak with one voice on this subject. Says one, "Eighteen men were cruising off Barda Point and Mayemba bay for twenty-four days. They took daily one ounce of the wine containing four grains of quinine, and no sickness occurred. Again, "the boats were frequently cruising in the mouths of the rivers, or else blockading the coast between Delagoa

bay and Mozambique. I had frequent opportunities of observing the prophylactic effects of the quinine wine. In only one instance did fever follow its use, and then it was of a mild character. This contrasts strongly with the seizure of a whole boat's crew with fever, in March, 1851, when no wine was administered, as it was lost in crossing the bar of the river." The report of Mr. Sibbald says, "that while coaling at Sierra Leone, the weather was very wet, and both officers and men were unavoidably much exposed to the rain. An extra allowance of grog and quinine was given to each man, and continued for some days. Mr. —, however, placed no faith in its preventive influence, and would not take it. He, alone, suffered an attack of fever, which proved fatal."

Dr. I. Walsh, attached to the "Water Witch," says that "thirty-six men were employed at the attack on Lagos. They were in the river four or five days, and with the exception of three, all took the quinine wine whilst there, and fourteen days after they left. Five only were attacked with fever; namely, the three men who did not take the wine, and two others who imprudently exposed themselves to the sun, and bathed whilst much heated by violent exercise.

Dr. Bryson gives many other similar reports, and concludes by calling attention to the report of Doctor Burton, surgeon to the flag-ship of the African squadron. The remarkable fact appears that the number of deaths in the squadron has been yearly diminished, until it has come down to an equality with that observed on the more healthy stations. During the preceding year, the ratio of deaths, from disease, to the 1000 of mean force only amounted to 6.9, or less than 7-10ths of one per cent. He suggests the propriety of adopting some measures different from those now in existence, for the preservation of the health of the seamen employed in the merchant service on the coast of Africa. Vast numbers of these men in the prime of life, die every year, of fevers contracted on this coast, and Dr. B. advises that, instead of carrying a large supply of lemon-juice for the prevention of scurvy, which, in consequence of the abundance of fruits, is nearly useless, that they should be furnished with the quinine wine for the use of the crew, which should be administered in the same manner as in the men-of-war on the station.

Dine last Edition of Silasia and the Bahamas.—In the last volume of

vegetable wool. Both have been erected by M. de Pannewitz, inventor of a chemical process, by means of which it is possible to extract from the long and slender leaves of the Pine a very filaceous substance, which he has named wood (or Pine) wool. It can be curled, felted and woven.

All the acicular leaves of the Pines, and of the Coniferae in general, are composed of fibrillae, extremely fine and tough, surrounded and held together by a resinous substance, under the form of a thin pellicle. When, by decoction and the use of certain chemical agents, the resinous substance, is dissolved, it is easy to separate the fibres, to wash them and to free them from all foreign substances. According to the mode of preparation employed, the wooly (or fibrous) substances acquires a quality more or less fine, or remains in its coarse state; in the first instance it is used as wadding, in the second, to stuff mattresses. If this Pine has been preferred to other of our Pines, it is on account of the length of its leaves. It can be stripped of its foliage when quite young, without any injury; the operation takes place when it is still green. A man can gather 200lbs. of leaves a day.

"It was first advantageously substituted for cotton and wool in the manufacture of blankets. The hospital of Vienna bought 500, and after a trial of several years, has adopted them entirely. It has been remarked, among other advantages, that no kind of insects would lodge in the beds, and the aromatic odour was found agreeable and beneficial. These blankets have since been adopted by the Penitentiary of Vienna, the Charity Hospital, and the barracks of Breslau.

"Its cost is only one-third of that of horsehair; and the most experienced upholsterer, when the wool is employed in furniture, could not tell the one from the other. It can be spun and woven, and resembles the thread of Hemp for strength, and it may be made into rugs and horse-blankets.

"In the preparation of this wool, an etherial oil of a pleasant odour is produced. This oil is at first green; exposed to the rays of the sun, it assumes an orange-yellow tint; replaced in the shade, it resumes its former green colour; rectified, it becomes colourless. It differs from the essence of turpentine extracted from the same tree. It has been found efficient in rheumatism and gout, also as an anthelmintic in cutaneous diseases. Distilled, it is used in the preparation of lac of the finest kind. It burns in lamps like olive oil, and dissolves caoutchouc completely in a short time. Perfumers in Paris use it in large quantities.

"It is the liquid left by the decoction of the Pine-leaves that has been so beneficial in the form of bath. The bath establishment is a flourishing one.

"The membranous substance obtained by filtration at the time of the washing of the fibres, is pressed in bricks and dried; it is used as a combustible, and produces from the rosin it contains, a quantity of gas sufficient for the lighting of the factory. The production of a thousand quintals of wool, leaves a quantity of combustible matter equal in value to six cubic metres of Pine-wood."

Then follows a letter from C. R. Nesbitt, Esq., Governor of the Bahamas, to his Grace the Duke of Newcastle, Chief Secretary of State

for the Colonies, transmitting samples of the fibre for the Museum of the Royal Gardens.

GOVERNMENT-HOUSE, NASSAU, BAHAMAS, }
Dec. 10, 1853. }

My Lord Duke.:—I have the honour to enclose, for your Grace's information, the copy of a report made to me by the Surveyor-General of Lands, relative to the vast indigenous forests in the Colony, of that species of the *Abies* (*Pinus*?) commonly known as the "Pitch Pine Fir."

The Surveyor-General estimates these forests at not less than 200,000 acres in extent. They are, however, singularly limited to four islands in the Colony, viz: Abaco, Andros island, Grand Bahama, and New-Providence. This report was called for by me, in consequence of a specimen of the fibre obtained from the spines or leaves of this species of Fir-tree, deposited, during the present year, in the Nassau Museum; a larger portion of which fibre has been subsequently produced by the labour of prisoners, a specimen of which is herewith forwarded for your Grace's inspection.

I trust your Grace will kindly pardon any irregularity in thus bringing under your notice, a subject not of ordinary official routine. A severe hurricane that occurred about the 22d ult., has created much misery in the Colony, the lower orders, as is usually the case in such visitations, being the greatest sufferers. The sad event adds much to the previously entertained conviction, that the labouring class in this Colony suffer very much for the want of some additional staple, on which to employ their industry; and hoping that a new resource may be found in the staple obtainable in these indigenous forests, I trust that in thus communicating with your Grace, as the head of the Colonial Department, on the subject, some advantage may accrue to the inhabitants of this Colony.

I annex to this despatch a printed copy of a paper on the subject of the fibre of the Pine (given above) which appeared in the "*Nassau Guardian*," explanatory of its use at Vienna.

When the art of obtaining it with the greatest facility and in the manner best adapted for use, is fully known, and its value correctly ascertained, it may possibly eventually become an article of no inconsiderable export from the Colony, and if only as a matter of reference hereafter, your Grace may possibly not deem this despatch useless.

I have the honour to be, &c., &c.,

Signed

C. R. NESBITT, Lieut. Gov.

The Pine of the Bahamas is most probably the *Pinus palustris* Will'd, (*P. Australis*, Mx.) our long-leaved Pine, which extends through Florida to its most southern point, and which, therefore, cannot be very distant from those islands. (For the above selection and note, we are indebted to a distinguished Botanist of this State. *Eds. Charleston Journal.*)

Chorea.—By Dr. T. J. GARDEN. (*Virg. Med. & Surg. Journal*, from *Stethoscope*.)—Dr. T. J. Garden, of Charlotte county, Va., (*Stethoscope*, March, 1854,) substantiates his former assertions in regard to the efficacy

of the black cohosh or snakeroot (*cimicifuga racemosa*) in chorea, and adduces a letter from Dr. Wootton, of Lunenburg, in which several cases, illustrative of the advantages of this remedy, are detailed. Cimicifuga, in addition to its tonic property, is supposed to stimulate the secretions, especially the bronchial mucous membrane, and to exert a powerful influence, probably of a sedative character, upon the nervous system. Many years ago Dr. Garden recommended it in phthisis, and Dr. Jesse Young reported several cures of chorea effected by it. Prof. Wood declares that he has used it frequently in chorea with complete success, after the failure of purgatives and mineral tonics. The dose is one or two fluidrachms of the saturated tincture three or four times a day.

Delirium Tremens.—Dr. Grieve (*The Medical Chronicle*) relates a case in which the external applications of belladonna dispelled the distressing ocular delusions of a patient labouring under delirium tremens, as soon as the physiological effects of the remedy on the iris were manifested.

Gangrenum Oris.—Dr. Haybach, (*Organ, f. d. ges. Heilk.*, 2, 3, 1853,) boasts of the good effects he has derived from the topical application of creosote in gangrene of the mouth. The creosote succeeds better, according to Dr. Haybach, in those cases in which the economy has undergone a septic alteration than when the disease is purely local. We relate these results, like many others in this Record, with great reserve. We presume that Dr. Haybach has confounded *gangrenum oris* with ulcerative stomatitis. For the rest, Dr. Dunglison (*Pract. of Med.*, vol. i., p. 36,) advises in gangrene of the mouth creosote and alcohol, in equal parts, applied by a brush after incisions through the gangrenous parts.

Pneumonia.—Dr. Blakey, of Green, C. H., Va., (*Stethoscope*, February,) declares that he has never lost a patient with pneumonia in his professional life of seventeen years, and that he has never failed "to restore to perfect health the most violent cases, in from five to seven days!" The treatment which has been followed by these pleasing results, consists in the administration of tartar emetic, in one-fourth grain doses, every two hours, during the night as well as during the day, and the application of a "large vesicatory" over the seat of pain. The antimonial solution is combined with laudanum, unless opium is counter-indicated by the condition of the bowels, and the dose of tartar emetic is raised to one half a grain in obstinate cases. Dr. Blakey treats pleuritis and "inflammations of different portions of the air-tubes by the same method, which is applicable also to 'inflammatory rheumatism.'" "Ten days is the longest period in treating the most violent cases" of the last named disease!

EDITORIAL AND MISCELLANEOUS.

Editorial Correspondence of the Charleston Medical Journal and Review.

FLORENCE, ITALY, January, 1854.

Paris to Florence; Pass of the Mont Cenis; Genoa; Pisa; Hospitals; Museums, etc.; General and Medical advantages to the Physician and the Student, of a residence in Florence; Hospitals and Medical Men; Comparative advantages of Paris and Florence; Cheapness of Living; Price of Labour; Florence as a residence for Families.

D. J. CAIN, M.D.,

Dear Sir: The ability to avail oneself of the advantages of a residence in foreign countries, where languages to which we are more or less accustomed are spoken, increases, within certain limits, in a geometrical progression as the time advances. For the benefit, therefore, of a few additional months in Europe, and to study its language and literature I have selected Italy, and particularly this city, as my sojourn—wishing to remain still longer than during my first visit in so delicious a clime and in a country abounding in all that is noble in art and glorious in the recollections of its past. I trust, too, that the readers of our Journal will believe that no small portion of the inducement was the desire, certainly without pecuniary reward, to examine into the condition of Italian Medical Science, and, in some degree, to attempt to add to the interest of its pages. Now that the time of my stay here is almost elapsed, I look back with pleasure upon the period, and feel indebted to that pioneering spirit which led me to leave my companions in Paris for a journey, in winter, over the Alps. In addition, pleasant friends, warm southern hospitality and the generous impulses of Italians, also contribute no small portion to the gratification.

Permit me to say, as in the familiarity of a private letter, that, selecting the route by Lyons, I crossed the snow-covered pass of the Mont Cenis by night, in company with our fellow-citizen the Consul General for Egypt, to whose previous ill-health I was partly indebted for the pleasure of his company. Turin and Genoa lay on our route, and I left my friend at Spezzia. His invitation to accompany him to Egypt in the corvette Saint Louis, which, under the gallant Captain Ingraham, awaited him at that port, needed not the inducement of a view of the pyramids and a letter for the Journal, on *Egyptian Medicine*, to have prevailed upon me; but the short time that would be left to complete my allotted stay in Florence, and duties at home, bade me reluctantly to decline. I can assure you it is a very difficult thing to leave Europe, when you are once across the Atlantic, and were not the ties at home still stronger,

one might very pleasantly remain. I have on hand now an invitation to spend two or three months with a Spanish gentleman, on an olive plantation in Spain, which, like others, must go by the board.

In the communication for the January number, I referred to some of the medical institutions of Italy, but did not mention those of Genoa. In my second visit there, recently, I went over the largest of its hospitals, and was struck by its magnitude and style. Although perhaps not more imposing than that at Lyons, yet in its solid stone construction, its lofty ceilings, marble floors and decorated chapels—in its courts filled with statues and busts, and its walls covered with paintings—it is worthy both of this palatial city and of that age of merchant princes when “Genoa the Superb” poured into her own lap the wealth of the world, and when she rivalled Venice for the mastery of the Mediterranean and the Orient.

At this, my second visit to Pisa also, I had the opportunity of seeing the University, the Hospital and Anatomical School, though I did not remain long enough to deliver letters which I had from friends in Paris to the professors—among whom is Matteucci, the well-known author. The hospital is situated on the same famous square upon which, as early as the thirteenth century, Nicholas and John of Pisa raised their cathedral, two hundred years before Brunelleschi and Michael Angelo built theirs at Rome and Florence. Here, too, facing the hospital, stand the Leaning Tower, the Campo Santo and the Baptistry, which might well compose four of the wonders of the modern world. The hospital I did not think remarkable in any respect, nor particularly well kept. I was very politely conducted over it by a young medical gentleman, who acted as chief of clinique or *agrége*. In the anatomical museum are several very curious and even extraordinary specimens. The dissecting rooms were somewhat similar to those with us, only with a greater profusion of marble covered tables.

Since my arrival in Florence I have been indebted to persons in Paris and to Italian friends here, for letters and personal introductions to the most distinguished professors and practitioners among them. I may

to him the decoration of the Order of St. Joseph, an honour which he would not consent to share with the Austrian General, who, with his armed bands, guards Italy against itself. In the battles of '48, this surgeon shared the common dangers against the enemies of his country, and his experience in military surgery is quite extensive.

Regniol is now Professor of Surgery in the Hospital of "Santa Maria Nuova," and I attend his morning clinics from 7½ to 8½. He is generally followed by about fifty students and medical men, including internes. Before any surgical operation can be performed in the hospital there must be a consultation among the other surgeons attached to the institution, even for as simple a matter as hydrocele. Prof. Regniol tells me that he regards it as a useful precaution, as they do not interfere as to the mode, but simply as to the question of propriety. He says that the patients, both medical and surgical, have also the privilege of demanding consultations when they please. To me it is entirely a new feature. How Velpeau, Malgaigne and Jobert, as well as some that I know of nearer home, would kick at such a rule? This is the only place in which medical men are not quite as absolute as Nicholas!

Regniol and Ranzi are publishing a work on Surgery, which makes its appearance in fasciculi. I do not know what amount of original matter it contains. Ranzi is at present called to Egypt, on a professional visit to the Pacha's court.

I have had the opinion of medical gentlemen here whom I knew at Paris, and who, consequently, have enjoyed an opportunity for comparison, and they, as well as all who have spoken with me, agree in esteeming Bufalini as the most distinguished of the practitioners and medical writers of Italy—sharing this celebrity, however, with others at Pisa and elsewhere.

He has written extensively on Therapeutics, and also on subjects in relation with medical ethics, and I trust on a future occasion to be able to furnish our readers with an analysis of some of his, with that of other productions of living Italian writers.

Bufalini has two wards, male and female, of about a hundred beds, in the Hospital Santa Maria Nuova, where he delivers his clinique *at the bed-side*, three days out of the week, at 10½ A. M., and a lecture, *viva voce*, on Therapeutics and Materia Medica, at 1 o'clock, on the intervening days. His style of lecturing is fine, animated and dignified, and, being a Romagniot, his enunciation, though not Tuscan, is exceedingly distinct, clear and sonorous. His audience, and this when speak-

ing of those who, for the most part, are medical students, is always respectful and attentive. A roll is called before the lecture begins. The cliniques, his as well as the others, generally continue an hour and a half, and the professor in these, as well as in the Obstetrical wards, remains at the bed-side examining some student—either he who has had special charge of the case or another selected upon the spot. The latter is required to interrogate the patient, form a diagnosis and prescribe; at the conclusion, the professor sums up the result and expresses his own judgment.

This is similar to the plan pursued by Valleix, at La Pitié, and by a few others in Paris, and is of the greatest service. It awakens the attention of the student and audience, stimulates his zeal both in reading, observation and study, brings his attainments before his companions, and when persevered in, renders him an experienced physician.

The *Agrèges* occupy the intervening days and have the same privilege, the professor being then usually absent.

In large cities in America this subdivision in the distribution of honours, where there are many candidates for places in hospital, might be imitated with benefit to the sick and to the profession.

Pachini, who has published several papers well known in Europe, is the Professor of General Anatomy. There is, also, one of Pathology. He delivers his lectures in a small room, seated at a table placed in the middle; at his side is his prosector, and around him, either sitting or standing, generally without much order, though universally quiet and well behaved, are the students. Every thing has a very informal air.

In the dissecting rooms, which are well supplied with subjects, as well as is the dead-house, *post mortems* are made regularly, and thus there is no want of means for study.

There are also lectures and recitations, with the practice of the pharmaceutical details, in the pharmacy attached to the hospital and medical schools, all of which occupy one range of buildings.

This hospital contains a thousand beds, and is the largest in Florence, there being five others in the city, including those dedicated to diseases of the skin, to idiots, sick soldiers, etc. It occupies the buildings of an old religious establishment, and, with its long corridors and lofty roof, has quite a conventual air and appearance. There is a chapel attached to it, and its walls and courts are filled with scriptural paintings in fresco, dating back, probably, many centuries. Hospitals in Europe are great eleemosynary institutions, from whence aid of every kind is distributed, and they occupy quite an important portion of the public attention and enjoy much municipal care and consideration.

The professors are not paid as well as those in America. I am informed that, with only one or two exceptions, the salary seldom exceeds five hundred dollars a year (500 scudi). The lectures commence in December and continue during five or six months.

The students divide their time between the University of Pisa and the Medical School at Florence, spending five years at the former, where certain branches of study are pursued, and three here, making eight years in all required for a medical education. Their actual expenses are about \$300 for the whole period, of course exclusive of living.

I am not certain to what extent, but political reasons have led them to subdivide those in the Universities, so as to lessen the number in each. There was a school also at Sienna, but it is suppressed.

The following is a schedule of hours and distribution of subjects taught at the Medical School of Florence and at the Hospital Santa Maria Nuova, translated from a copy from the hospital furnished me by a friend:

Each Day.	{	From 7½ to 8½—Surgical Clinique.
		" 8½ " 9 —Surgical Consultation and Operations.
		" 9 " 10 —Medical and Surgical Attendance.
		" 9½ " 10½—Autopsies.
		" 10 " 10½—Medical Consultations.
		" 10½ " 12 —Medical Clinique.
		" 12 " 1 —Obstetrical Clinique.
Monday, Wednesday and Friday.	{	" 8½ " 2½—Practical Pharmacy.
		From 9 to 10 —Elements of Natural History.
		" 11 " 12 —Lesson on Botany.
		" 12 " 1 —Descriptive Anatomy.
		" 1 " 2 —Patholog. Anatomy.
		" 2 " 3 —Operative Surgery.
		" 2 " 3 —Pharmacology.
Tuesday, Thursday and Saturday.	{	" 3 " 4 —Organic Chemistry—Pathol.
		" 2½ " 3½—Clinique and Lesson on Mental Maladies.
		From 1 to 2—Repetition of Surgical Operations.
		" 1 " 2—Special Pathology.
	{	" 2 " 3—Anatomy of the Regions.
		" 3 " 4—Lesson on Obstetrics.

preparation was the entire head and bust of a young girl, with the skin rather whiter than natural, but its structure perfectly preserved.

I have, in the January number, already referred to the well-known Museum of Anatomical Preparations, which is in another part of the city, near to the Pitti Palace, and which belongs to the Grand Duke. Most of them were not, as I stated, made by Zummi, but only those representing the effects of the plague. The greater part are much more modern. This museum is an honour to Tuscany. The wax preparations of phaenogamous and cryptogamous plants are worthy of attention, and indicate great labour and skill. Professor Parlatore, a Sicilian by birth and the author of the best modern work on Italian botany since the time of Michele, delivers lectures three times a week on Botany and Vegetable Physiology. He is attended by citizens and students, though not numerously. Prof. P. has presented all of his collection to the museum of the Grand Duke, in whose service he is; he has travelled extensively in the North of Europe and elsewhere, and devotes his attention exclusively to this branch of science. There are botanical gardens attached to the Boboli gardens and also to the hospital. I visited the former in company with M. Caruel, a young botanist of distinction to whom I had letters from friends and correspondents in Paris, and I was delighted with the variety, order and handsome arrangement of its contents. I was pleased to see an old fellow-citizen, the *persimmon tree*—*Juniperus Virginiana*, L.—in full fruit.

The Marquis Ginori drove me, a few days since, to his country seat, near Florence, where I saw one of the finest private collection of hot-house plants I have yet met with in Europe, including several hundred species, some very rare. I could not but reflect how seldom, if ever, we see such, even in public institutions, in our country. I may add that this gentleman also owns the largest manufactory of porcelain in Tuscany, which I had also the pleasure of inspecting with him.

By invitation of my friend, the Cav. Frescobaldi, I visited the penitentiary and prison of the *Murate* during the last week, in company with himself, the Duke of W. and Lord H. In its construction, all the modern improvements recommended both in America and in Europe have been adopted, and in the wards for the sick, attached, there is every thing which can contribute to the comfort of the inmates.

Public charitable institutions abound, and they are distinguished for cleanliness and good order. There are always gardens to admit of exercise, and no pains or expense are spared both in the construction and outfit. It is in these that the United, particularly the Southern, States have much to imitate and to learn.

Having occasion recently to spend two or three mornings with Prof. Amici, of the Astronomical School, I have been induced to order from him a lens for my microscope. The latter was made in 1850, by Nachet, in Paris, and though not containing his strongest power, and, therefore, not permitting of a full comparison, yet I had ocular proof of the great superiority of Amici's. It may not be without interest to mention that this gentleman, who is an amateur in the manufacture of optical glasses, has invented a new principle for their construction, partly by means of a drop of water interposed between the object glass and the substance to be examined. The field of vision is larger, much more light is thrown upon the object, and it is rendered vastly more distinct and well defined. His price is about a hundred pauls (\$10) for a lens of about one thousand diameters, which can be adapted to instruments made by others.

Prof. Amici's microscopes are, however, not near as well mounted as those of Nachet and others. He praises the ingenuity and skill of Spencer, of New-York, but declares that his lenses have not the power and clearness of his own. I saw one of Spencer's in his room.

I will now go into some detail respecting the mode of life and expenses of living in Florence, in order that information may be furnished those who desire, for its medical and other advantages, to make it their abode during any length of time. Much of this information I have derived from my own experience, the rest I have been at some pains and expense to acquire. This I have been induced to do because I believe that this city may well be selected, as a substitute for Paris, by those who, desirous of visiting Europe for purposes of education, are deterred by the expenses attending a residence in the latter.

Now I am not prepared, at present, to pronounce on the exact condition of medical science here, nor am I able to declare that a student or medical man could pursue his studies here to equal advantage as in Paris, for I believe the opposite to be the truth. But there are hundreds at home whose means do not permit them to reside one or two years in London or Paris, who, as the sequel will prove, could, with great ease, have all those general advantages which a residence in Eu-

man with almost equal facility as at London, Paris or Berlin ; but a certain acquaintance with Italian is with more ease acquired than either of those just mentioned, and he would be able, in a comparatively short period, to master it sufficiently to derive instruction from the lectures and clinics. To one before unacquainted with French, even in Paris the first six months are worth almost nothing. To the lectures, as well as the advantages of seeing the sick in the hospitals, he is here, as well as elsewhere on the continent, freely admitted ; besides, he enjoys all the collateral ones of a residence in a European capital and in a country the most ancient, instructive and interesting of the old world. Many, for what they throw away or squander during six months or a year even in one of our Northern cities of the United States, might reside in Florence with ease and comfort.

No place, perhaps, surpasses Paris in the facility and advantage with which a stranger is permitted to select and pursue any branch of learning or any department of medicine, under the best masters and under the beneficial influences of all that zeal and variety which characterizes the French people and that renowned city ; but let our observations be applied to those who are compelled to forego these, and yet to select between remaining at home and living for any period of time in a foreign country.

I am now myself persuaded that after reading the following, and when the example shall become general, numbers who remain languidly and inactive for years in small towns in the United States, will learn to seek to enlarge their thoughts and improve their medical knowledge and experience, by attempting to share in those benefits which are thus so easily afforded in a city like Florence, situated in a smiling valley, whose rich and fertile soil throws profusely into her lap that which elsewhere must be paid for at great cost. Whether it is this alone, or its almost untrammelled trade with the free port of Leghorn, or both together, I know not, but the fact remains that, for cheapness of living, no spot on earth surpasses, if it compares with, this city.

I am informed by an intelligent English physician, established here, that some years since students lived just as well, and at no more cost, in a German University (Gottingen). Of this I have no personal experience, but am certain that it is impossible either at Vienna or Berlin.

I should, perhaps, advise our readers that, though theoretically an advocate of, I am quite a poor hand at, economy and retrenchment, and worse at calculation ; the simplest arithmetic perplexes me, and I am frightened at the shadow of a fraction ; but will endeavour, on the pres-

ent occasion, to overcome my repugnance to the latter and descend into the common-place of detail, however apparently minute and trifling. To those who really wish information, it is the only kind practically serviceable. I only regret not being able to be more explicit on other topics.

Medical men may frequently cross the Atlantic for their professional service during the passage. Many with whom I am acquainted return in sailing packets, and besides their free passage, receive an additional pay in money—often ranging from 70 to \$100. Without this, the price of a passage to Liverpool or Havre varies from 50 to \$130, according to the class of the ship and the accommodations. The expenses of reaching Paris from Liverpool is from 20 to \$50; from Havre or Bordeaux, 5 to \$10. From Paris to this city, by Lyons, over the pass of the Mont Cenis, through Turin, Genoa and Pisa, 250 francs (\$50). By way of Marseilles, with a steamer to Leghorn and by rail-cars, about the same, possibly a little more, though with the gain of some hours in time. By the inland route, by diligence, along the coast, through Canne and Nice, and by the celebrated Cornish road (which I have also travelled), it could not be done for less than 300 francs.

The price of rooms, per month, in this city, varies from 20 to 60 pauls (12 to 33 francs), including service. These rooms are better than those he would be likely to obtain in Paris for a higher price. For my own chambers here, situated on the Piazza Santa Trinita, the most select and desirable part of the city, in a palatial building, including a large saloon and *chambre à coucher*, I pay 60 pauls, every thing included. In Paris, for a small but pretty chamber on the garden of the Luxembourg, I paid 50 francs (95 pauls).

In the best café houses and restaurants, his breakfast will necessitate the expenditure of from a half to one paul. The Italians seldom pay more than the first mentioned sum, usually, however, taking a supplemental one at 11 A. M. Dinners *a la carte*, including soup, two dishes, dessert and ordinary wine, cost habitually from 2 to 3 pauls ($\frac{1}{2}$ to $1\frac{1}{2}$ l.) I speak of those in the best establishments, well served and leaving nothing to be desired. It is possible, certainly not advisable, in Paris, to dine for less than 2 to 3 francs, as all who are experienced will confirm. This applies, too, to the *Quartier Latin*, where most students

the same costs the enormous sum of 2 crazies, which is equivalent to the $\frac{1}{2}$ of a paul! A crazie is an adulterated silver and copper coin, mixed with some baser metal, and much smaller and thinner, and having a more trifling look, than our English four-pence. It is a fraction more than a French sous, say about 7 centimes. It is divided into a certain number of entities, called *quatrini*, which *exist* and can be exchanged for commodities. In proof, for two of them a "lean and hungry" Tuscan buys bread made of pounded chesnuts, rendered at the same time palatable and nutritious by a little olive oil and pine seeds; this comes hot from the maker and vender, and his shop, situated in almost every street, is thronged with applicants. Or he may have a half-pint of the fruit itself, raw, roasted or boiled. The chesnut crop in Italy forms a material element of the popular provisionment, and its failure or abundance is a question which takes rank with others—with the vine in France and the potato in Ireland.

Clothing and dress for the outer man share with the more necessary aliments of the inner in the unambitiousness of their price. A full suit of the best black broadcloth, made to order, in the best tailoring establishment—not the *very most fashionable* in the city, for these, like the first rate hotels, have the same price the world over—involves the outlay of \$20. Boots and shoes are comparatively depressed in value. Things adapted to the sustenance of the intellectual faculties, such as books, for example, form an exception, and, in exchange for these, one is obliged to give roundly; yet you can buy choice old volumes of the sixteenth and seventeenth centuries for a mere song. I have, among others, a set of the Latin Medical Classics, of the sixteenth century, which would make a grocer bibliomaniac.

Fire wood costs 2 pauls a *box full*, and candles to read the books by 20 cr. the pound, of six or five, as pleases the purchaser. Washing, 2 to 3 pauls the dozen; 2 cr. for the larger unmentionables, and 1 cr. for the smaller—say 2 pauls a week for a *gentleman*. Segars (not an unimportant item for the student) can be got from a fourth of a crazie for the very ordinary made of native tobacco to 3 crazie for the best, which are called Havanas. The segars at 1 and 2 cr. are much better than could be obtained in America for the same sum, though no weed of European growth equals the genuine Cuban.

belonging to Cav. A.: it is elegantly furnished and handsomely situated one mile from Florence; I have visited it. In the words of the bill, it has ten chambers, not counting the *cuisine* where there is a supply of water. A carriage house and stable are attached, also an English wood, a lake with bark, a flower garden, bathing house and *chapel*. Every thing is furnished except silver and linen. The number of chambers can be augmented.

<i>Terms</i> —For one year, the whole,	-	-	-	1,350 francs.
“ six month, “ “	-	-	-	800 “
The first floor for one year,	-	-	-	850 “
“ “ “ “ six months,	-	-	-	500 “
The ground floor for one year,	-	-	-	800 “
“ “ “ “ six months,	-	-	-	300 “

Families can obtain others at a much lower price than this, which I give merely as a specimen, being acquainted with the locality.

A labourer in the country gains from $1\frac{1}{2}$ to 2 pauls (1 franc to 5 sous) a day. This year, on account of bad seasons, provisions, wine, and, in fact, every thing, is higher than they have been for several preceding ones; but yet the labourer, for 1 crazie or 8 quatrini ($1\frac{1}{2}$ c.), buys a pound of bread. The ordinary wine is generally factitious, on account of an almost total failure of the crop, and costs 6 crazie the bottle. In the restaurants, the better qualities of the wine of the country varies from 10 to 34 cr.; Montepulchiano, by the flask, and the wine of Chiana, costs more. Bordeaux, Champagne and other foreign wines, as high as 75 cr.; Marsalla and Madeira, 4 cr. for the *bichiere* or glass.

Beef, is 4 cr. ($\frac{1}{2}$ paul); choux fleurs, cabbage, etc., 1 cr.; eggs, 1 paul the dozen; fruit, according to the season, 1 cr. a pound, and often less; sugar, 5 cr. per pound; coffee, 2 pauls the pound, less in the country, where the duty is avoided. The price of butter ranges from 1 to 2 pauls a pound; during the summer, generally 1. Milk, 4 cr. the quart. Oil, formerly 6 cr. the pound, now dearer. For light, 6 cr.

From a schedule of prices for the month of January, stamped by the government, which I have purchased, I translate the following additional particulars:

Tuscan corn varies from 27 to 23 liras per sack. (A lira is $1\frac{1}{2}$ pauls or 12 crazie.) That from the valley of the Chiana and from Romagna is the best. Corn not from Tuscany, as, for example, from the Roman

pound (equivalent to 1 to 2 cr.), according to quality. *Potatoes*, 6l. (75c.) the hundred pounds. Old red *wine* is 35 to 45l. the barrel; new red, 35 to 30l.; white, 40 to 35l. for first and second qualities. Old *oil* from 50 to 64l. the barrel of 90 pounds—there being four qualities; new oil is 59 to 60l.; there is an inferior article sold at from 25 to 50l. The price of *hay* the hundred pounds is 3l. for the second quality; what is known as *fieno grosso* can be purchased at from 1 to 2 liras. *Straw*, 1l.

To furnish, then, a *résumé* of almost the extremes of the student's actual expenses per month, exclusive of dress, I present the following table, by which he will be assisted in making an estimate for himself, according to his tastes and habits. Of course he can swell it to any amount—

Chamber, one month, varying from,	12f.	to	30f.
Meals, $1\frac{1}{2}$ to 3f. per day. - - -	45f.	"	90f.
Wood and candles, 2 to 3f. per week,	8f.	"	12f.
Washing, per month, - - -	3f.	"	6f.

Total, - - - - 68f.—\$13 138f.—\$26

In the *Quartier Latin*, at Paris, the same expenses, exclusive of fire wood and lights, would not easily fall short of 90 to 200f. as the extremes. In a private family in which I resided for some time at Paris, although I am unable to state them with accuracy, my actual cost of living lay between 150 to 200f.—for example: breakfast, 15 sous; *dejeuner a la fourchette* with wine, 25 sous; dinner, 55 sous per day—total, 4f. and 15 sous, which, per month, gives 102f.; room, 50f.—total, 150f. But there are very many who live for less. Those who reside on the other side of the Seine and on the Boulevards, who are generally *not students*, are obliged to pay, for the locality and for every thing else, at a higher rate. The principal and much the largest item of my own expenses, during the sixteen months that I remained there, consisted in what was paid to Internes and Masters for private instruction on the various branches of medicine and for professional privileges of various kind.

The above details, which have so very little of sentiment about them, I have entered into with some repugnance, but with the sole motive of furnishing accurate and reliable information. I am in possession of a great deal more still, which might interest and which I would gladly add, were it better adapted to the object of this paper. A thousand peculiarities, trifling in themselves, strike an observant stranger, which, if collected together, would constitute quite a useful body of facts, and throw more light on the actual condition of things than the more laboured descriptions of tourists and travellers, who generally speak of the fine arts, pictures and palaces that, to be appreciated, must be seen.

I shall send you the *Gazetta Medica Italiana Toscana* in exchange with ours.

Hoping that I will be in time to correct the proof of this—a privilege I have not enjoyed, and which I trust our readers will remember in my favour—I am,

Very truly yours,

F. PEYRE PORCHER, M.D.

Return of Dr. F. Peyre Porcher.

WE have had the satisfaction of shaking by the hand our friend and associate, Dr. F. Peyre Porcher, who has just returned from Europe. His sojourn in Europe, though of a duration a little short of two years, was nevertheless profitable not only to himself, but likewise to the readers of the Journal, who may remember his valuable contributions, consisting of reports of lectures, general correspondence, etc. We are pleased to inform our readers that, before leaving Paris, he succeeded in securing the co-operation of several distinguished physicians and of internes of the hospitals of that capital, who will contribute to its pages papers of interest on subjects in the different departments of the science. He has, moreover, been enabled to effect an exchange of our Journal with all published in Paris, and two or three in Italy; thus will our readers be placed in possession, in each issue, of the latest medical news. C.

Dr. C. Hapboldt.

eighteen months. It is but an act of justice thus publicly to record his high appreciation of Dr. Happoldt's signal services in the conduct of the Journal, of which he has had, during a great portion of the time mentioned, the almost exclusive control. We have the assurance, however, that he will continue to lend the aid of his pen to the promotion of the interests of the Journal.

South-Carolina Medical Association.

THE fifth annual meeting of this Association was held in this city on Monday, the 30th January last.

Owing to causes, with which we are not acquainted, the attendance of members from the country was smaller than usual. The orator, Dr. W. T. Wragg, delivered a very able and instructive essay on the importance of sanatory measures, or internal medical police, in connection with the observance of quarantine regulations, in the prevention of importable or communicable diseases. We had hoped to present our readers this essay in full; but inasmuch as all papers read before the Association become its property, and their publication among its transactions has been ordered, we can do no more than advise those of our readers who are members of the Association to peruse it with attention, feeling assured that they will do so with profit to themselves.

Papers on various subjects were read by Drs. Gaillard, De Saussure, Robertson, and others, and will appear among the proceedings.

The officers of last year were re-elected for the current year, and Dr. W. W. Mobley, of Lancaster, was elected Orator for 1855.

Medical College of the State of South-Carolina.

THE class in attendance on the Session of this flourishing Institution, during the past winter, numbered 234; the largest that has ever assembled within its walls. In the early part of the Session there were about 270; but in consequence of the appearance of varioloid in the city, the number was reduced to 234. Of this number, 135 were from South-Carolina; 43 from Alabama; 25 from Georgia; 14 from North-Carolina; 7 from Florida; 7 from Mississippi; 2 from Virginia; 1 from Louisiana; and 1 from China.

During the last session of the Legislature of this State, the Faculty of the College petitioned for, and received, an appropriation of

TWENTY THOUSAND DOLLARS, which will be applied to a rearrangement of the interior of the building, by which the commodiousness of the lecture rooms will be increased. The size of the present rooms having been found inadequate to the accommodation of the students during the past season, the design is to construct rooms of such size as to comfortably seat four hundred.

With a portion of the sum appropriated by the State for the improvement of the Institution, the Pathological Museum will receive large additions by the purchase of illustrations of disease from celebrated manufacturers of Paris ; in short, the appliances for thorough instruction in all the departments of Medical Science, will not be surpassed by those of any other Institution in the United States.

The return of Professor Agassiz to his Lectureship of Comparative Anatomy, next winter, in addition to what we have stated, will leave nothing to be desired in the opportunities afforded for acquiring a complete medical education.

Savannah Medical College.

ON reference to a catalogue of the students who attended the first session of this Institution, we notice that they amounted to thirty-seven ; of which number, six received the degree of M.D.

This is as large a number as the majority of the Colleges in the United States have had at their first session, and augurs favourably for the future.

To Physicians of the South and South-West.

THE undersigned has in preparation a work on the Fevers which prevail in the South and South-West, together with the diseases of the Digestive and the Respiratory systems—acute and chronic—with a view, in part, to examining their connections with each other, in aetiology and pathology, and the influences which they may exercise in modifying the characters of one another, &c. &c.

exerted an influence upon its character—what form or type of disease usually prevails in the locality of its occurrence—what were the peculiar or special symptoms, or morbid manifestations, which gave it identity of character and distinguished it from the ordinary endemic, or inflammatory Dysentery—and, in short, any thing of interest concerning it.

Physicians who have had opportunities of observing the disease and studying its nature, will confer a favour by communicating to me the results of their observations, as early as their convenience will permit. My address, until the 15th of September, will be "Red Sulphur Springs, Hardin county, Tennessee." After that date, "Lowndesboro, Ala."

H. V. WOOTEN, M.D.

New Medical Journal.

THE NEW-ORLEANS MEDICAL NEWS AND HOSPITAL GAZETTE, recently established in the great emporium of the South-West, is a candidate for Southern patronage; and from the few numbers which have reached us, we predict for it the most triumphant success.

It will consist of records of the most interesting cases occurring in the hospitals of New-Orleans; transactions of Medical Societies; excerpts from home and foreign medical journals; communications from correspondents in Europe; reports of important cases in private practice, and of all subjects of general interest in Medical Science.

It will be issued on the *first* and *fifteenth* of every month, printed on fine paper, with handsome type, each number containing twenty-four octavo pages, at THREE DOLLARS *per annum*. Its editors are:

S. CHOPPEN, M.D., House Surgeon, Charity Hospital.

C. BEARD, M.D., Demonstrator of Anatomy, University of Louisiana.

R. SCHLATER, M.D., Visiting Physician, Charity Hospital.

P. C. BOYER, M.D., House Surgeon, Maison Santé.

Editorial Changes.

DR. B. DOWLER has undertaken the editorial management of the New-Orleans Medical and Surgical Journal, made vacant by the lamented death of the late Dr. Hester.

Dr. JOHN DAWSON has succeeded to the editorship of the Ohio Medical and Surgical Journal, in place of Dr. R. L. Howard, deceased.

Obituary.

It is our painful duty to record the death of another prominent medical editor, Dr. R. L. HOWARD, Professor of Surgery in the Sterling Medical College. The deceased conducted the Ohio Medical and Surgical Journal for many years, with ability and success. He departed this life on the 16th of January last, after a lingering illness.

Errata.

IN the paper of Dr. Pendleton, on Epidemic Dysentery, in the last number, our readers will make the following corrections :

Page 171, fifth line from top, for "*relaxation* of bowels," read "*ulceration* of bowels."

Page 172, ninth line from bottom, for "*injustice*," read "*justice*."

Page 176, eleventh line from bottom, for "*persimmon*," read "*persimmon bark*."

TO READERS AND PUBLISHERS.

THE following Books and Pamphlets have been received: Homœopathy Fairly Represented, not Misrepresented, Henderson; West on Diseases of Children; La Roche on Pneumonia and Malaria, Fuller on Rheumatism; Draper's Chemistry; Meigs on Diseases of the Uterus; Report of the Standing Committee on Surgery of the Kentucky State Medical Society, by Joshua B. Flint, M.D.; Report on the Health and Mortality of the City of Memphis, for 1853, by C. T. Quintard, M.D.; Report to the Indiana State Medical Society, on Asiatic Cholera, by Geo. Sutton, M.D.; Yellow or Malignant Bilious Fever, as it occurred in Philadelphia, by Wilson Jewell, M.D.; Mémoire sur les Accidents Morbides produits par l'usage des Cosmétiques à Base de Plomb, par M. le Docteur Fulgence Fiévée de Jeumont; Nouvelle Etude Pratique sur le Traitement du Cholera, par M. le Docteur Fulgence Fiévée de Jeumont; Transactions of the Medical Society of North-Carolina; Proceedings of the Medical Association of Texas; Address of Dr. R. J. Breckinridge to the Graduating Class of the Kentucky School of Medicine; Lecture by Dr. Jonathan Knight, Introductory to the Course of Lectures in the Medical Institution of Yale College, Sept. 1853; Report of Dr. Dowler on Yellow Fever; Report of Dr. Fenner on the same.

CHARLESTON

MEDICAL JOURNAL AND REVIEW.

Vol. IX.]

CHARLESTON, S. C., JULY, 1854.

[No. 4.

ART. I.—*M. Cl. Bernard on the Physiology of Absorption, and on the Abdomino-Renal Circulation.* Report of Lectures, with Vivisections, delivered in Paris, 1853, prepared by F. PLYER PORCHER, M. D. Paris, 1853-4.

WE must remember that absorption diminishes in the stomach during digestion, both on account of the afflux of blood and the pouring out of the gastric juice. In inflamed tissues, also engorged with blood, absorption is much less active.

There are two varieties of absorption—the one by the chyliferous vessels, and the other by the vena porta, each of which selects peculiar substances; the election depending, in some measure, upon the necessity or not for certain of these to pass through the liver, or to proceed immediately to be eliminated by the kidneys.

Starch and sugar, we now know, are absorbed into the vena porta.

Cane sugar introduced into the stomach as cane sugar, undergoes no change in the intestines into any other substance; it is absorbed as cane sugar, though a small portion of it, by acid reaction, is changed into grape sugar. In dogs and horses a very large proportion enters the vena porta in its original condition.

Milk sugar only differs from the others in submitting, with difficulty, to alcoholic fermentation; though the pancreatic juice renders it fermentable. You do not, therefore, find much of this in the vena porta.

Raisin or grape sugar is absorbed as grape sugar in the vena porta.

Cane sugar cannot serve as a nutritive substance as cane sugar; for, if you inject it into a vein, it is found eliminated as sugar, and not absorbed. But, by passing through the liver, it undergoes a transformation which renders it subservient to nutrition, by the process which converts it into grape sugar.

Now, if you inject grape sugar into the veins of the neck, you

will find a part of it only in the blood, there being a certain amount of it which is really destroyed in passing. This species of sugar is also modified by the liver, which gives it a different physiological character.

Diabetic sugar, corresponding chemically with that of the grape, differs from it physiologically, as it is more easily absorbed and destroyed when injected into a vein.

The results of these observations teach us, that it is essential for all sugar to pass through the liver to undergo the fermentation and destruction, which its simple passage through the intestines is insufficient to complete.

Albuminous substances, also, M. Bernard has ascertained, must pass through the liver to receive the modifications essential to its complete absorption into the system. This is found in the lymphatics and in the vena porta, which are so difficult to experiment upon. He injected the albumen of eggs into the jugular vein, and it made its appearance in the urine; hence, of course, it does not remain as a nutritive product in the blood, though portions are also found in that fluid—(another variety of albumen, when injected in like manner, was eliminated by the kidneys)—hence both require the liver for their complete transformation. The same albumen, injected into the vena porta, passes directly to the liver, and you find none of it in the urine.

Fatty matter does not enter the liver, as is well known, but it first passes to the lungs, when it is thrown directly into the general current of the circulation. There is a slight absorption of a small portion of it in the vena porta, but this is absorbed naturally and through no chemical change. It is not emulsionized as it is by the pancreas. This small portion, taken up by the vena porta, goes to the liver as fat, where it disappears. M. Bernard could discover no trace of its presence in the blood which had left the liver. Some physiologists sustain the opinion, that in this organ it enters into the formation of sugar; but this is not verified. It certainly disappears there, and does not pass out of the liver as fat; it may help to form bile.

So, that the liver requires three kinds of substances—sugar, starch, and the small quota of fat just referred to.

Cyliferous vessels.—These are especially subservient to the absorption of fat. They are not found in fishes or birds: M. Bernard has fed pigeons on fat, and, upon examination, lymph was found in the vessels, but no fatty matter in suspension. In birds the latter is probably absorbed by the vena porta. In these animals the vena porta communicates largely with other veins, and there is, therefore, no absolute necessity for their contents to pass through the liver.

The liver thus appears to be interposed between the intestines and the grand circulatory system, to modify certain substances preparatory to their entrance, and prepare certain products for their elimination.

The liver has another duty to perform, viz : to prepare an equilibrium between certain substances ; as, for instance, when the proportions between flesh, viands and ordinary food, varies ; whether one is introduced in too great quantity, or the reverse, it is the liver which makes up for the deficiency, or converts what is superabundant into another product more congenial to the requirements of the system.

An animal, nourished exclusively on viands, has these azotized substances converted by the liver into sugar ; and the exclusive diet produces no effect upon the blood proportionate to the quantity introduced by means of its reduction during its passage through this organ, which exists, as it were, as a balance to preserve from the injurious influences of excess. Its anatomical position, also, with relation to the other organs, supports this idea founded on its physiological action.

There are three maladies affecting the human economy, which depend upon different modifications of the liver and its secretion, viz : diabetes, albumenuria, and fatty or chylous urine. But these need only be referred to at present.

The whole of the ingesta is not always absorbed—a large part is carried off with the secretions which might still have served the purposes of nutrition.

The residue is composed of—1st. What is refractory to the digestive process, as corneous substances—certain epidermic coverings, insoluble phosphates, etc. 2ndly. Substances which are expelled because not digested ; as when an animal is fed on *secula* in excess, that only is absorbed which is converted into sugar, whilst the remainder is voided unchanged. The same is true of fatty matter in excess, which also escapes in a condition very similar to that in which it entered.

What, then, is the reaction of the coecum, that some talk so much of ? The coecum, M. Bernard stated—and we saw experiments on a dog which confirmed it—has an acid reaction, but no new digestive powers. Some describe it as a second digestive stomach or cavity. The reaction of the coecum is sometimes alkaline ; it is depending upon the excess of certain products of digestion, or certain kinds of food introduced into the stomach. A superabundance of starch and sugar consumed by an animal, is changed to acetic acid, and, when it reaches the coecum, of course presents an acid reaction. Exactly the reverse occurs if the animal had been fed on azotized substances. The coeca of car-

nivorous animals, consequently, present an alkaline reaction which can be reversed by altering the character of the diet.

The coecum is not, then, an organ of secondary digestion. The well known phenomena of spontaneous decomposition which occurs there, depend upon extrinsic causes. M. Bernard does not account for the odour which characterizes this portion of the intestinal tube. The great variety in the size, and the absence even of the coecum in some animals, is another strong argument to prove its comparative unimportance. In the horse it is very large, and in the dog quite small.

All substances absorbed, which pass through the liver, do not go directly to the general circulation. They seem to be arrested by this organ.

Through the kidneys, also, many substances enter without making the circuit of the general circulation. There appears to be a peculiar abdomino-renal circulation, by which the quality of urine is altered independently of any influences derived from the circulation through the heart. In many animals a great deal of fluid substances go directly to the renal vein without reaching the liver. In almost all animals, with the exception of the mammiferous, there is a considerable amount of substances ingested, which circulate through the kidneys without even coming in contact with the lung.

In some animals the kidney is really an organ with functions, varying considerably from that of others. A carnivorous animal, during digestion, has the character of its urine materially altered in the proportion of its constituent. There being two species of normal urine—that secreted by the veins, and that eliminated from the arteries—they must, of course, vary with the ingesta.

M. Bernard, to test some of the changes which food of various kinds undergoes during digestion, and to obtain some of the chyle of the thoracic canal, placed a dog upon the table which had been fed on mixed food, composed of starch, sugar, viands, fat, etc. It was then killed by passing a sharp pointed bistoury into the medulla oblongata, and, upon examination, the intestinal canal presented the following pe-

vena porta (abdomino-renal circulation) and the general circulation, which varies under certain circumstances—as during the absence and presence of food in the stomach—and which it is important to observe.

An animal which has, for a long time, been abstinent, receives a large quantity of food in its stomach; a draught is suddenly made upon the *vena porta*, which differs from other veins in its not only carrying all the blood of the mesenteric veins, but it is also, at the same time, charged with the carrying of almost all the substances absorbed from the intestines; in this respect resembling the pulmonary artery.

Now in some animals, as in horses for example, there are veins passing directly from the *vena porta* to the *vena cava inferior*; and, when the former is much swollen, a portion of its contents pass to the latter without traversing the liver. So that just below the diaphragm is often very much distended, on account of the accumulation. A very small portion of its contents pass to the heart; the valves existing there (preparations of which were exhibited) prevent it from descending, and it is forced to reflow through the renal veins to the kidneys. So that, indeed, the kidney is between two pressures; and the secretion of urine must depend somewhat upon whether the animal is or is not in full digestion. The acid of the urine becomes alkaline during digestion, and its secretion is much increased in quantity.

During this pressure upon the valves of the *vena cava*, of which we have just spoken, the blood of the lower limbs goes by the *vena azygos*. The latter in these animals (the horse, for example) is very large, to compensate for the varying demands made upon it. After the hurry of digestion is over, the vessels cease in a measure to expand, and the blood then reflows along the *vena cava*, and finally gains the heart as ordinarily. But we see from the above, that a certain quantity of alimentary substances goes directly to the kidney, without reaching the heart. And the same is true of man, in whom, though with shorter intestines, it is less distinctly observed. M. Bernard was enabled to study the process with more advantage in animals with a larger and longer intestinal tube. By this view we see in what way the kidneys are enabled to maintain a balance in the circulation.

If prussiate of potash is given to an animal during full digestion, it is absorbed and passes through the hepatic and renal circulation, by which it is eliminated; a very small portion, if any, reaching the heart. A reverse condition takes place if another, which has fasted for some time, is fed on the same substance; the greater portion of it is received into the torrent of the circulation, by means of which it is conveyed to distant parts of the system. This explains the variety in the action of

the same or a smaller dose of a poison, when received into the system at different times; in the one case quite a large quantity may be eliminated with the urine, and rendered innocuous, when a much smaller amount, swallowed on an empty stomach, would have been attended with dangerous or fatal consequences.

Now, the physiological fact, that at certain digestive periods a larger portion of the absorbed aliment passes to the kidney without reaching the heart, receives anatomical development in some animals in whom the vessels, which contribute to this, are in more constant service, and become proportionally enlarged. The renal veins have a very important rôle to play, and one can witness, by making a small aperture into the abdomen, the reflux through them. In the English race-horse they are very much increased in size, from the constant rapidity with which the circulation is often kept up. The same excessive development is observed at the *abattoirs*, among animals which have been worried or run down; in which case there is considerable hepatic engorgement, with increase in the demands made upon the portal veins, together with those contributing to the functions of the kidney. [We know, also, that if the vena porta is injected after death, it produces considerable expansion on the liver; being invested by the capsule of glisson, its walls are not adherent to the liver like the hepatic vein, and thus admit of an increase in size.]

ART. II.—*Thoughts on Malaria, and the causes generally of Fever.*
(Continued from the Charleston Medical Journal, Vol. 7, No. 5.) By
J. D. RUMPH, M. D., Orangeburg, S. C.

THE editors of the Charleston Medical Journal, commenting on the papers of Dr. LaRoche, on Malaria, as causative of our endemic fevers, &c., remark: "That it appears at a juncture, when on the one hand, by some writers in the West, almost all our diseases are referred to malaria as a cause, and on the other, its existence is denied by others, because it cannot be demonstrated chemically, microscopically, &c.—the writers of the latter category seeming to forget that the existence and operation of a cause can, in some instances, be proved by a process of reasoning, the proof being as conclusive as if it resulted from a sight of it." "Upon both classes," says the journalist, "Dr. LaRoche inflicts heavy blows, whilst he himself is cased in a coat of mail." With the utmost deference and respect for the opinions of others, I shall not attempt

to establish or disprove the title, whether we are hereafter to regard Dr. LaRoche as the modern Goliath, and whether there might not yet be found a David who is able to discover a vulnerable point, by which this great giant could be slain.

In my feeble judgment, however, I have not seen anything in the Doctor's pieces to entitle him to anything beyond the consideration of a mere man, though one calculated to enhance the dignity and honor of our profession, and elevate its standard. His blows come fast and heavy, but nothing less than was expected, upon shoulders made broad to receive them. I endeavoured to excite inquiry on the subject, and have obtained it; but proof, such as I desire, has not been given, and that Dr. L., and others who have given to malaria such unlimited and illimitable scope, as the proximate cause of fever, have not afforded it incontestably to my mind, is now my purpose to show.

In 1852, when the yellow fever raged in Charleston very extensively, and in Savannah pretty actively, the season offered no clue to its approachment, taking the data of others as a guide. April of this year was cool and rainy. There were seven good rains, on the 4th, 5th, 11th, 14th, 17th, 20th and 26th. It hailed on the 17th and 18th. In May the weather was pleasant and seasonable. We had eight rains: 3d, 13th, 23d, 26th, 29th, 30th and 31st; hail on the 29th May. In June, five rains, 7th, 21st, 24th, 27th and 30th. There was a very cool spell in this month, from the 24th to the 27th. In July there was but one good rain, which fell on the 16th—a sprinkle on the 21st. Then we had no rain until the 16th of August; after this, we had frequent hard showers, accompanied with much lightning and thunder, until October. This month, and November, were dry, or rather the showers were very light. We had a small rain and a storm on the 10th October, and a sprinkle on the 29th. November, it sprinkled on the 7th, 25th and 30th. In December, we were deluged with rain, having had fourteen hard rains. It was generally healthy with us this year, except during the latter part of August, and the whole of September it was quite sickly—the fever generally was of mild intermittent form: occasionally a few cases of congestive and typhoid made their appearance. I shall make no further remarks on this subject, having been led into it much further than I intended, but will leave others to notice the breaches and coincidences, and draw their own conclusions.

Lancisi announced the malaria doctrine as a primary cause of fever, over a hundred and fifty years ago; how many have adopted his views since that period, it is impossible to say; perhaps thousands. But this shall not deter me from giving the results of my honest inquiries.

I have not denied the agency of bad air, in some cases, in the production of disease ; but this is to be regarded as an exciting cause in all instances, rather than as a remote or proximate cause. Small-pox and measles belong especially to this category.

Schönbien, a German of some notoriety, can see ample cause of fever in Ozone, an element produced from electricity under certain circumstances and conditions. A certain degree of temperature favours the electrical phenomenon, and when it is not given off in clouds, thunder and rain, its effects are more sensibly felt, and prove more deleterious to the animal system. We all feel depressed in very hot weather, especially of a dry season, and all have witnessed an increase of this debility or oppression, about the rising of a large thunder cloud, when attended by a calm. The atmosphere is now charged with the electric fluid, and the consequences would be serious, did not the laws of nature provide for its speedy exit by lightning, thunder, rain and winds, which soon cool the air, whereby we are refreshed and invigorated, the enervating means being dispersed. Then we have a direct proof of an agent of deleterious import, the existence and operation of which is so conclusive, as not to require visual demonstration ; neither does it call for a metaphysical code of reasoning to establish its existence. You may call this bad air, if you choose ; but let it not be understood to be that bad air, or malaria, upon which so much stress is placed, as producing our endemic fevers, pneumonia, and a hundred other diseases. I regard electricity, its changes and effects, as one, if not the principal proximate cause of fever ; whilst decomposed animal and vegetable matters, in a gaseous or vaporous form, are likely to be a coincident, secondary and adjuvant cause, but not at all necessary in most cases ; for the decay and decomposition of a good many vegetable substances are not as injurious to health, as the rotting piles of indigo weed, certain modes of making manure, &c. This I have already spoken of in a former essay, but hope to show more conclusively, in the course of the present one.

Gaspard and Leibig, I believe, were the first who spoke of collecting this malaria, and pointed out the manner of examining the putrid matters of animals and vegetables in a gaseous form.

Subsequently, Dr. Hume, of Charleston, S. C., has given his views and results on the subject, and thinks, from experiments he had made, that he has discovered the malaria which produces yellow fever, in a gaseous or vaporous condition by condensation.—See his essay on the subject, in the Charleston Medical Journal, Vol. 5, No. 1, Jan., 1850. It remains to be seen whether his discoveries are to be regarded as conclusive, and set down as such, or, like others, the results obtained are

nothing less nor more than compounds of carbon, hydrogen, nitrogen, oxygen and sulphur, &c. If this is the case, I fear his discoveries will profit us but little. I very much suspect that he has been experimenting with sulphuretted hydrogen, and carburetted hydrogen gases, concentrated in the places he found them.

His ideas, as regards the temperature of the atmosphere, the dew point, the barometrical status, and the meteorological changes, are excellent. For some time past I have endeavoured to give my attention to these matters, particularly the dew point, and the thermometrical variations. The last year was a very dry one, particularly the early part; the dew point, in consequence, was very low, there being no rains in April and May, except two or three small sprinkles on the 4th, 16th and 25th of April. The rains made a partial beginning on the 8th of June. We had three rains in June; fourteen in July; ten in August, and six in September; and then none until the 20th of October. This period embraces the sickly season. The dew point advanced very high as soon as the rainy season set in, and continued in this state until the first of October, when it again fell pretty low, but not so low as it had been, owing to cool nights. The weather was warm until the first of October, when the mercury in the thermometer (Fahrenheit's) began to descend, which it gradually continued to do until we had frost and ice, which was on the 20th of November. I am sorry I did not make a table of this matter, or of these changes, but I hope my observations are sufficient for practical purposes.

Now, it strikes me as something strange, that whilst the yellow fever was raging in New Orleans, Mobile, &c., and the cause attributed to the changes attendant on a dry and hot season, other things being favourable to the production of fevers, as assumed by the theory of Dr. Hume, that we should have no yellow fever in Charleston the last season, and so little intermittent fever in the Southern States generally, and in this State particularly. This, in part, militates with the theory of Dr. Hume and others. The same cause which produces yellow fever in cities, does not, therefore, produce intermittent fever in the country.

Yellow fever dies a natural death as soon as it reaches the pure coun-

Negroes are less liable to attacks of intermittent fever than whites, as a general rule, but both classes are subject to it all their lives. I have seen octogenarians have it, and I have seen children at the breast suffering with chill and fever. No respect is had to complexion, sex or condition, the enciente female, the rheumatic, the syphilitic, the whooping cough patient, &c., are all subject to the fevers of this climate. The present season bears so striking a resemblance, so far, to that of 1849, that I am impressed with the idea that we are to look for similar results. It will be recollected that that was the year the yellow fever prevailed extensively in the city of Charleston. We had a great deal of sickness in the country also, that year; our fevers began early in June, spread rapidly, were very pertinacious, and acquired some fatality; they prevailed till the first of December. The type was intermittent generally, though we had more cases of typhoid fever than a year or two previous, or since. We had a large snow on the 15th of April, and a freeze every day after to the 21st. This year we have had several late cold spells with ice. On the 27th March we had a freeze, with plenty of ice $\frac{1}{4}$ inch thick; and on the 3d and 18th of April, we had other freezes, with ice as thick as that of March 27th. And at this time, 24th April, it is cool for the season. I then remarked that we might look with caution on the events of the approaching season. I will now direct attention to the discoveries of Dr. Hume, which I have read with so much pleasure, but with whose ideas I cannot agree entirely.

Chemistry has demonstrated the fact, that the compounds of carbon, hydrogen, sulphur, &c., are diffused in the atmosphere, and are deleterious in their influences, when we come in immediate contact with large amounts of either of them, not to say, however, that they are the causes of fevers. This inclines me to believe, that the substance experimented with, was a deposit of one or more of these chemical compounds to which some organic matter may have been added; possibly, some putrescent animal substance was in or about the cellar of Mr. Gatchel, for these cellars are not notorious for cleanliness, and he does not tell us that he examined it very carefully, but says "it was dark and damp, and appeared clean." Alas! how often are we deceived in appearances. It would have been far more satisfactory, had he extended his experiments to different parts of the city, in many cellars, and given his results in each. Spring comes not with the singing of a single bird. Examination should be made in each summer and fall month; healthy as well as unhealthy years should be considered. The electrical condition should also be taken into consideration, in these unhealthy localities, and a strict comparison made. Examinations and experiments should be

made in cold weather. Then we shall learn something more of these things, that will be of substantial benefit. In my judgment, and I have not formed it hastily, I have not seen anything in this connection to establish the fact, that yellow fever, or any other fever, is produced from malaria alone, in its strictest sense or peculiar construction, without the aid of some other contingent cause, being primary or remote; and until this is established, I shall believe in other active proximate principles, as causative agents in fevers, more potential and more numerous than the world-renowned malaria.

I shall not attempt to erect a fantastic system on whimsical hypothesis; neither do I intend to consider one cause alone, as productive of all our different types of fever; nor shall I contend that the morbid agent enters the system through one medium, or that one organ, system, fibre or tissue shall be invariably implicated. The rocks, Scilla and Charybdis, must both be avoided, least we founder upon the one, or split upon the other. Many deservedly great and zealous labourers in the vineyard of medical science, have already gone into extremes; and were we, at the present day, to regard all that has been said by learned advocates, as cardinal doctrines of the divine art of healing, few, I guess, would be practising medicine at this time with any success, though we are indebted to many of them for much of the information we boastingly possess. Our science is progressive, and our art eclectic. We must not give exclusive importance to the solids and fluids as the seat of disease; neither inflammation of the brain, or its investing membranes; nor gastro enteritis, nor the respiratory organs, nor liver, nor spleen nor skin, nor the nervous system, as we find advocated by Clutterbuck, Smith, Broussais, Cullen, &c. It is true, in each of these parts or systems the attack may begin, and each in turn, may become entangled or implicated in the course of disease; indeed, in certain forms of fever, the congestive particularly, all of these seem to become involved simultaneously, as in rapid succession; but this is, by no means, an invariable rule—rather the exception.

We all have our peculiar ideas as regards the attack or invasion of disease; whilst I would regard the nervous system, or nervous extremities, as being most frequently the point of attack, more clearly traceable, and more permanent in their effects in our ordinary fevers, yet I will not assert this to be constant. The truth is, I have seen fevers in which there appeared in my diagnosis, no permanent feature or index, calculated to convince me of its seat; still, in all of those there were the characteristic symptoms of fever, such as the quick pulse, dry skin and increased temperature. Now, I could not say that the diseased action originated primarily in the cutaneous system, but I have no doubt, but

that either of the above named systems, organs, &c., under certain conditions, serve as the foundation, nay, actually serve as the *fons et origo* of disease, and I might indulge a little farther, and assume that the anatomist will yet discover other glands, tissues, &c., as complicated, as being first invaded in disease. But to enumerate some of the causes which serve to interrupt the normal conditions of organs, functions, &c., and thereby to establish an abnormal state.

I will again notice ozone, discovered by Schonbien. This agent seems to possess the power of bringing the system in that peculiar situation favourable to fever, which I have explained heretofore. This is no imaginary or fanciful object; the eminent German has clearly proved the existence of this agent, of which any one will be convinced who will take the trouble to read.

We have all felt the effects of electricity.

Since Leibig and others have failed to detect the poison of small-pox, &c., in the atmosphere, we should not assert too confidently, the existence of malaria or fever-poison, in the sense generally used. Dr. LaRoche, with his hundreds of quotations of eminent works, has failed to do this satisfactorily. It is no proof of this agent being causative of fevers, that the poison of small-pox and other cutaneous diseases has escaped detection. Itch being an exception—which is of animalcular production and dissemination—I do not think it an unreasonable assumption, to suppose that all our contagious maladies have the same origin and spread themselves in the same way. If these really exist, and have so great an agency in the production of an eruptive fever, as well as other diseases, which is highly probable, many of them are so small, as to elude detection in the manner now practised to discover them. What we know of visible objects, bear but poor comparison with the invisible things of creation.

Who could number the great panorama of living and moving creatures of but one day? whilst it is supposed, on the strongest grounds, that the hosts that bestir themselves at night, are far more numerous than those of the day.

Some insects, we are told, begin life in the morning, come to perfection in a few hours, meet in love, lay their eggs, grow old, and pass out of life before the setting sun, perhaps tired of this protracted state of existence. Each drop of water, as it descends in showers of rain, or

compass, and yet these infinitesimal beings might be considered rather below than above the standard of microscopic objects possessing animal life ; for, within the system, or upon the body of these, might be found animals or parasites of other living things, similar to those, or bearing close analogy to those, so troublesome to man and to the higher order of animals. By what physical laws these innumerable throngs are governed, I will leave to the Entomologists to determine. No doubt they are subject to many of the ills of life to which flesh is heir ; they therefore suffer, languish and die.

Now, being constantly in immediate contact with this cotrillionary, though invisible host, surely they exert a powerful influence upon us ; how great, we yet have to learn. In each inspiration, we take thousands no doubt into the lungs, which serves as a benefit, or acts as an evil to us. With each expiration, no doubt thousands receive their principal food, which they greedily devour.

We will suppose a law of nature to become violated, and this ultra-microscopic host become diseased ; what a fruitful source would this be for the production of diseases of mankind ! All the eruptive, and most other diseases, with the whole catalogue of contagious maladies, could here find their origin and cause, which I strongly suspect, and I would that I had the means of determining ; but I trust that science will, before long, prove the fact to an incredulous world, when we begin to see things in their proper light and form. May that time speedily arrive, when, for the honor of our profession, abstruse theories and false philosophy may hide their heads behind true reason and the light of science. Climate, season, temperature, &c., bear a prominent part in what I am attempting to elucidate. Sudden changes from sea to land, have brought on injurious results, as well as all sudden change of location will do, when latitude and season are disregarded. Long continued, great, or excessive heat, will not always produce fever, when other things are unfavourable, or we should have fever produced from stoves, furnaces and like places. A certain degree of heat and moisture are alledged to produce malaria, but a question arises, whether it is produced at all, from animal and vegetable substances, undergoing certain chemical changes ; for would not cooks, and those whose business it is to prepare our food, stand a poor chance for health, whilst continually exposed to the steam and vapour of these substances undergoing those or analogous changes ?—this too, before any condiment or seasoning is added to effect the change. On the contrary, this class of persons are the most healthy in the world. Blacksmiths, forgers, and castors of iron, and all who labour about the fire, are robust and healthy. That a certain latitude is

required for fevers to be produced, I readily admit, and there is proof in abundance to establish it; but it also requires certain seasons. As far as my knowledge goes, 36 deg. 30 min. north latitude, and the range of Fahrenheit's thermometer 80 deg. to 90 deg., are the maximums of bilious intermittent fevers; below this latitude, and above the degree indicated thermometrically, fevers of this class are rife, until we arrive at a high degree of heat, say 100 to 110 deg. or more, and latitude 20 to 30 deg.; and yellow fever seems to usurp the claim of others. Why it is that this principle loses itself in very hot climates, as asserted by some authors, we have no means of explaining.

Doctors Chalmers and Lining, of Charleston, South-Carolina, in an old work, speak of a fiery something in the atmosphere as being the cause of fever; yellow fever, it is presumed, as that was the subject they were writing upon. They make no definite remark about the fever-producing malaria of other authors. What is this fiery something of which they speak? I leave others to say, whilst I proceed onward. Neither fogs, nor excess of moisture produce fevers with us; nor am I certain that the night air is any more injurious to us than that of day, though many contend to the contrary. If a certain person be taken and made to sleep a definite number of hours in the night air of summer, and at another time to sleep the same number of hours in the rays of an August sun, the result will soon satisfy the most sceptical. A few hours' sleep in the hot sun of summer, is sufficient to produce fever; especially, to one unaccustomed to exposure to the sun. This happens so frequently, that no one will allow his negroes or children to expose themselves in that way. This is not the case with him who reposes himself in the arms of Morpheus in the night air.

Why the dread to travel and sleep exposed to the night air has obtained such notoriety, I know not; when it is a well known fact, that these fevers seldom or never attack at night, and never during sleep. Negroes on our plantations, prefer to sleep at nights out of their tight houses in warm weather, and many of them will select an open place on a simple broad board, and sometimes on the naked earth, without covering of any kind, and snore out the whole night; and yet, according to numbers, not half the whites, who do but little labour, and sleep in close and fine houses, escape the fever; house servants, and children's nurses, though better fed and clad, and who are more cleanly, and are better lodged, have doubly the amount of sickness as the field negroes, who are humanely used and worked, which is the case in this community. From this, I think a reasonable conclusion can be arrived at, that the poisonous agent of fever exercises its influence in the day, and not in the

night. Is malaria, then, a distinct morbid agent? and is it the special and universal cause of our periodic fevers? If this is answered in the affirmative, how shall we account for those fevers which are found in arid plains, destitute of water and vegetable matter, which are of strictly intermittent type?

Ferguson, and others, have spoken of fevers of this character, found in such places.

Is the cause of fever to be found in the gases, ammoniacal, hydro-sulphuric, phosphoretted hydrogen, carburetted-hydrogen, &c., &c.

"Azotized flakes may be found in the atmosphere, and flakes of organized particles are found during the precipitation of dew, as is proved by filling a vessel with ice; leave it exposed for some time, and the water on its outer surface, when examined, will exhibit this matter in solution." Do these results not always take place during that season when decomposition is taking place in the vegetable kingdom? I believe no experiments were made in winter or cold weather in those infected districts spoken of by Dr. Hume, or the results might have been far otherwise than what he found them, or they might have been analogous; therefore, they are not to be relied on, as morbid agents, causative of fever, until further investigation is made, and the facts proved to a demonstration. If the whole of the offal of any large city, the obstructed drains, the privies, rotten animal and vegetable matters, could be collected and examined, it would astonish the most incredulous, and present a frightful picture to the beholder.

If these were the sole or principal causes of fever, the wonder is, that there are any escapes from fever in our large cities, or any one found enjoying good health; and then, if a comparison be made with the country, with its sparse population, and strict attention to cleanliness, in regard to the above named objects, how fevers should ever obtain a place, is matter of surprise; and yet, the fact stands out in bold relief, that according to population, there are at least three to one of endemic disease in the country to those of most or all of the large cities. Contagious diseases, and yellow fever, are more numerous in large cities, from the fact of their constant intercourse with strangers and travelers from all parts of the world. Reasoning, then, from these premises, the quantity of this vaunted poison, malaria, must be evolved in great abundance in large cities to produce fevers, or any other disease, whilst in the country an

from human sight, but produced from vegetable and animal substances in a state of decomposition, and found more immediately in the vicinity of dead water lakes, ponds, marshes, bogs, puddle holes, prairies, &c., and when once generated, made to pass hither and thither, the harbinger of fevers, without regard to age, colour, sex or condition. Can all this be established? Let me sooner agree with Linnæus on the universal dependence of disease on parasitic life. I regard a great collection of names of authors, however illustrious, to ornament our pages, as rather pedagogical, than proof of our peculiar views; this task, I will yield to the productive minds of others so disposed.

I find it a rather more difficult task to prove the insalubrity of the Pontine marshes of Italy, to be of malarious nature, and attributed to the same, than the unthinking might suppose. A writer in this Journal, under the signature of F. P. P., writing from Italy and Switzerland, says he crossed the Pontine marshes after nightfall, and slept most of the time on the top of the diligence, but suffered no inconvenience. This was late in June, the weather cool for the season; the inhabitants looked sallow and sickly, but in reality were not sick. This I think is one proof at least, that pestilence is not confined to the darkness of the night, neither for its production nor invasion.

And, for further proof, I would call the attention to our contagious maladies, which exert their influence at all times, both of the day and night. Season, however, assists materially in the extrication and evolution of the causes of our endemic diseases. Oxygen, which is all-important to animal life, and of which is required such a large amount, would be death to vegetable life in the same proportion; and carbonic acid gas, so deleterious to animal life, is a great promoter of vegetable life. This latter gas is seldom found in a state of nature in great amount, hence animals are not injured by it when allowed to roam at large, and only feel its injurious effects when exposed to concentrated potions. It seems to act by passing immediately into the lungs, through the air passages; hence dyspnœa, syncope, coma and death. Different are the results when a smaller quantity is received, or when

results. The Grotto del Cane, near Naples, is an instance of this gas in concentration. This region is said to be almost entirely uninhabited; the few peasants who reside near are consumptive, and have a ghastly appearance—an effect of the poisonous exhalations that arise from this grotto. “Dogs, when thrown in the grotto, to gratify the curiosity of travellers, soon lose all sensation and motion, and are taken out apparently dead; but quickly recover when thrown into the neighbouring lake. This lake is of clear and wholesome water.” In any country where such poisonous exhalations are to be found, the health of the people is seriously affected, though not by fevers, as a general rule; and no one attributes to this gas an agency in causing fever. Carburetted hydrogen gas is found in marshy countries, and in the vicinity of coal mines. Sulphuretted hydrogen in the neighbourhood of sulphur baths, &c. Sulphurous acid gas in the vicinity, and at the craters of volcanoes, &c.

The deleterious effects of the fabled Upas is to be attributed to one of the above named agents. The cause of fever, then, is not to be traceable to any one of those gases, though found in great amounts at the places indicated; therefore, these, and all similar compounds and elements, are to be expunged from the roll of those considered as causes of fever.

Now, as the very existence of animals momentarily depend on oxygen for life and health, it is all important that an abundant supply of it should be found in all places; for animals find substance and nourishment not in one place, as plants and trees, but are migratory, traversing the globe in every direction—and man especially can make his home in the frigid or torrid zone, without violating a single law of his nature, and without hazarding his physical peculiarities.

Africa's Niger coloured race would be unchanged in Greenland's icy mountains—his progeny ever carrying with him his national characteristics and peculiarities; the woolly head, the flat nose, black skin, peculiar smell, &c., are always the same. The white races are also unchanged by transplantation; only that they carry with them a higher order of intellect, which they maintain through all time and change, which are indubitably and indelibly stamped upon their progeny. Climate has so little effect in altering the physical peculiarities of any and all nations

bles, as his habits are more peculiar, especially as regards his food and drink ; whilst he stands pre-eminently at the head of all vitalized beings, and all others must be regarded in a diminished ratio, until we arrive at the zoophyte and stationary objects of creation, when this principle of superiority is greatly diminished.

Then a new series of phenomena take place ; for, from our very losses, begins a new process which serves as food for the floral kingdom, if not alike to the floral, faunal and all the inferior kingdoms of earth.

All things are dependent upon this agent for life, in a greater or less amount ; and none are injured by it in a state of nature, dispersed in the atmosphere as we find it.

Like Chlorine, it is a non-conductor of electricity, and, in this way, acts as a protector of other disturbing agents, &c.

Should there happen to be a want of this agent at any time, which, we are informed, is equal and invariable at all times of the day and year, as proven by experiments with the Eudiometer, how soon would it be felt to our injury. This want, no doubt, sometimes happens, but in what manner is not well defined or understood ; but it is, unquestionably, the source of many of our diseases.

The laws of God are perfect and harmonious, but man is the violator, and as long as he continues ignorantly or wilfully to disturb those laws, so long will he have to suffer their penalty.

ART. III.—*The Causes of Parturition.* By W. W. GOLSAN, M. D.
Autaugaville, A'a.

CONCERNING the causes of the contraction of the uterus at the end of 280 days in parturition, and the source or origin of the stimulus which produces the contractions of that organ, there is a great variety of opinion ; some contend that the contractions depend upon an inherent power existing within the organ, and that the stimulus which causes it to contract just at the end of 280 days, is the foetus and waters which

to occur at one period than another. It is known that abortion almost always occurs at what would have been a menstrual period, and most of practitioners have learned to avoid all predisposing causes at that period, as they are well aware of the fact that causes that have no tendency to produce abortion at other times, may and do bring on abortion at that time. Now, why should abortion be more likely to occur at what would have been a menstrual period, than at any other time, if it was not influenced by the same law which that is governed by? If I am to answer the question, I must say that there is no reasonable cause whatever. Others contend that the contractions of the uterus are dependent upon a reflex action, and that the stimulus which excites that reflex action is derived from the ovaria. I fully concur with the latter opinion; and I think that abortion being more likely to occur at what would have been a menstrual period, substantiates me in holding to it. I really do believe that the same stimulus that causes the menses to be discharged at each menstrual period, causes labour to come at the end of 280 days, or at what would have been the 10th menstrual period. I think, also, that the same sympathies exist between the ovaria, mammæ and uterus during pregnancy, as exist during menstruation; but, as they are more equally spread, they are diminished in force, and consequently they permit the uterus to contain the fœtus for the space of 280 days, or for ten menstrual periods. Those who contend that the contractions of the uterus are excited by a distention of the organ by the fœtus and waters, bring forward cases in which labour came on, apparently, during the interval of two menstrual periods, as evidence against those who argue that labour commences at the time the menses would commence to be discharged, at the 10th period; but all that I have to say about this is, I do not believe that there is any well attested case on record, in which labour came on naturally between two menstrual periods. I admit that labour has and may come on at the end of 260 days, or even sooner, or that it may be protracted until the end of 290 days; but what does that prove—as all know that the menses appear in some women at the end of every 26 days or oftener, and in others only at the end of every 29 or 30 days? Now, if a woman who menstruated regularly every 26 days, became pregnant, she would be very apt to be confined at the end of 260 days; whilst if one who only menstruated every 30 days, should become pregnant, she would be apt not to be confined until the end of 300 days. The only way in which we can prove that labour does or does not come on at what would have been a menstrual period, will be by making especial enquiries concerning the interval of each menstrual

period, and whether the menses appear regularly at that period (as the menses might be irregular in its appearance in some women, and this irregularity in the appearance of the menses would interfere with the time of the commencement of labour); if such be not done, there is no certainty about any observations that we may make. I go so far as to assert, that were we to pay particular attention to the time of the appearance of the menses in women, and see whether or not it appeared at regular periods, and at regular hours of those periods, we would be able to tell on what day, yea, even on what hour, labour would commence in those women who were very regular. Even in my limited experience, I know of several instances in which labour commenced at the very day and hour on which the menses had been making their appearance. I think the subject is worthy of closer investigation, as it would afford much satisfaction to practitioners and the parturient women to know on what day, even if not the very hour, on which labour would take place. I hope that some of the medical profession will take it upon themselves to investigate the matter more closely; if I succeed in getting some one to do this, I shall have accomplished that which I desire.

ART. IV.—*The Alleged Connection between the phases of the Moon and the quantity of Rain.* By JOSEPH JOHNSON, M. D., Charleston, S. C.

I BELIEVE the opinion to be very prevalent, that the two great changes of the moon are the periods most frequently attended by falls of rain. Having, for many years, kept a diary of the weather, for the Medical Society of South-Carolina, I have some reason to doubt the correctness of this opinion. Having that diary on the table before me, I made an abstract from it, during the last twelve years, for the purpose of verifying that opinion, or of confirming my doubts. The following table of the weather, at each full and new moon, shows how many of the three days at each of these great changes, were attended by falls of rain, and how many were without rain. I submit it to the inspection of your readers, if you think it worthy of publication, premising that in some parts of the diary is noted the occurrence of a shower, but where the fall of water in such a shower was less than the quarter of one-tenth of a quarter of an inch, I took no note of it either in the diary or in the abstract. The result is 618 clear days to 289 days of rain, in the twelve years, and a proportionate result in each separate year. The

book from which these results have been drawn, is open to the inspection and scrutiny of any gentleman who may wish to examine it for this and other matters :

	Full moon.	Clear.	Rain.	New moon.	Clear.	Rain.		Full moon.	Clear.	Rain.	New moon.	Clear.	Rain.
1842.							1845.						
January,	26	1	2	11	2	1		23	2	1	8	3	0
Feb'y,	24	3	0	10	1	2		22	2	1	6	3	0
March,	28	3	0	12	3	0		23	2	1	8	3	0
April,	24	3	0	10	3	0		22	3	0	6	0	3
May,	24	0	3	10	3	0		21	3	0	6	1	2
June,	28	2	1	8	1	2		19	3	0	4	3	0
July,	22	1	2	8	2	1		19	1	2	4	2	2
August,	20	1	2	6	3	0		17	1	2	3	0	3
Sept'r,	19	3	0	4	3	0		16	1	2	1	3	0
October,	19	3	0	4	2	1		15	3	0	1	3	0
Nov'r,	17	1	2	2	2	1		13	3	0	29	2	1
Dec'r,	17	3	0	1	3	0		13	0	3	28	3	0
		24	12		28	8			24	12		26	10
					52	20							
1843.							1846.						
January,	16	3	0	30	2	1		12	3	0	27	3	0
Feb'y,	14	2	1	none	0	0		11	2	1	25	3	0
March,	16	1	2	1	3	0		12	0	3	27	3	0
April,	14	3	0	29	2	1		11	2	1	25	2	1
May,	13	3	0	31	3	0		11	3	0	24	3	0
June,	12	0	3	27	2	1		9	0	3	23	2	1
July,	11	1	2	27	2	1		8	3	0	23	3	0
August,	9	1	2	25	1	2		7	3	0	21	3	0
Sept'r,	8	3	0	23	3	0		5	1	2	20	2	1
October,	8	1	2	23	3	0		4	3	0	20	3	0
Nov'r,	7	2	1	21	3	0		3	3	0	18	1	2
Dec'r,	6	1	2	20	1	2		2	2	1	18	3	0
		21	15		25	8			25	11		31	5
1844.							1847.						
January,	5	3	0	19	3	0		1	3	0	16	1	2
Feb'y,	4	1	2	18	2	1		{ 31	3	0			
March,	4	3	0	18	3	0					15	3	0
								{ 1	3	0	16	3	0
April,	3	3	0	17	3	0		{ 31	3	0			
May,	2	3	0	17	3	0		30	3	0	15	2	1
June,	30	3	0	15	1	2		29	2	1	14	2	1
July,	29	2	1	15	2	1		27	2	1	12	1	2
August,	27	3	0	13	3	0		27	2	1	12	2	1
Sept'r,	26	2	1	12	3	0		26	0	3	10	1	2
October,	26	3	0	11	3	0		24	3	0	9	1	2
Nov'r,	24	3	0	10	3	0		23	2	1	9	3	0
Dec'r,	24	3	0	9	3	0		22	1	2	7	3	0
		32	4		32	4		21	3	0	7	2	1
									30	9		24	13

1848.	Full moon.	Clear.	Rain.	New moon.	Clear.	Rain.	1851.	Full moon.	Clear.	Rain.	New moon.	Clear.	Rain.
January,	20	3	0	6	3	0		17	1	2	2	1	2
Feb'y,	18	3	0	4	2	1		15	1	2	1	2	1
March,	19	3	0	5	2	1		17	2	1	2	1	2
April,	16	3	0	3	3	0		15	2	1	1	2	1
May,	18	3	0	3	2	1		15	3	0	1	3	0
June,	16	3	0	30	2	1		18	1	2	29	3	0
July,	16	2	1	30	1	2		18	1	2	28	2	1
August,	14	0	3	28	3	0		11	3	0	26	2	1
Sept'r,	18	1	2	27	3	0		10	3	0	25	3	0
October,	12	1	2	26	3	0		11	3	0	26	2	1
Nov'r,	10	3	0	24	2	1		10	3	0	25	2	1
Dec'r,	10	0	3	25	3	0		8	3	0	22	1	2
		25	11		29	7	1852.		26	10		24	12
1849.													
January,	8	1	2	24	3	0		7	3	0	21	3	0
Feb'y,	7	3	0	22	2	1		5	3	0	19	2	1
March,	8	2	1	24	2	1		5	1	2	20	2	1
April,	7	3	0	22	3	0		4	0	3	19	0	3
May,	7	1	2	22	3	0		3	2	1	18	2	1
June,	5	1	2	20	3	0		2	3	0	17	3	0
July,	5	3	0	19	1	2		1	0	3	16	2	1
								30	3	0			
August,	3	0	3	18	8	0		29	3	0	15	2	1
Sept'r,	2	0	3	16	3	0		28	3	0	13	2	1
October,	31	3	0	16	2	1		27	2	1	13	2	1
Nov'r,	29	3	0	14	8	0		26	1	2	11	2	1
Dec'r,	29	1	2	14	1	2		26	2	1	10	1	2
		21	15		29	7	1853.		26	13		23	13
1850.													
January,	27	0	3	13	3	0		25	3	0	9	1	2
Feb'y,	26	2	1	12	2	1		23	2	1	8	3	0
March,	27	2	1	13	0	3		26	2	1	9	1	2
April,	26	2	1	12	1	2		23	3	0	8	3	0
May,	25	2	1	11	3	0		22	2	1	7	2	1
June,	24	2	1	10	2	1		21	2	1	6	2	1
July,	23	1	2	9	1	2		20	2	1	6	1	2
August,	22	1	2	7	2	1		18	1	2	4	2	1
Sept'r,	21	2	1	6	3	0		17	0	3	3	0	3
October,	20	3	0	5	3	0		16	3	0	2	1	2
Nov'r,	19	2	1	3	3	0		15	3	0	1	2	1
Dec'r,	18	3	0	3	3	0		15	2	1	30	2	1
		22	14		26	10			25	11		20	16

My own opinion is, that the influence of the moon has been greatly over-rated, not only as to the changes in the weather by which, incidentally, the health of human beings might be affected; but, also, as to its supposed direct influence on some of the mental and bodily diseases of mankind, such as mania, menstruation, epilepsy, &c. Even where some of them appear to return at lunar periods, it is not gene-

rally the case ; and as to conception and parturition, it is unquestionable that the birth occurs much more generally after nine calendar months than nine lunar months. In epilepsy the attacks are at first after long intervals of time, and, of course, not at lunar periods ; then gradually reduced in the length of intervals, until so frequent that the paroxysms cannot be attributed to the influence of the moon. In menstruation, although it may occur with many at lunar periods, yet it unquestionably recurs at all ages and phases of the moon in different females ; and when suspended by pregnancy and nursing, it recurs frequently at different periods of the moon from those of its previous appearance in the same persons. As these affections are periodical in their recurrence, they are associated in the human mind with the periodical changes of the moon, and, without investigation, finally ascribed to its influence.

To doubt the influence of the moon on the ebbing and flowing of the tides, will, doubtless, be called heresy, blindness or madness. With your leave, however, I will point out some errors in the books, from which we and our children are taught the theory of the tides. The uniform recurrence of spring tides at the new and full moons, would appear to settle this question. But at the new moon, when it is in conjunction with the sun, the tides rise no higher than at full moon, when in opposition to the sun. The theorists assert that the tides are several feet higher at new moon than at the full, because of their united attraction : but I appeal to Dr. Bowditch, and believe that no higher authority can be adduced, that along the whole eastern coast of America the tides are not higher at the new than at full moon.

Again. They assert the highest spring tides occur in the winter ; because the sun at that time is nearer to the earth than at any other season of the year. But everybody knows that the tides are then comparatively the least, and the highest occur in the spring and fall, when the oceans are surcharged by the melting of snow and ice in both the Polar regions.

Again. They assert that as the influence of the sun and moon is greatest in the torrid zone, the tides rise higher there than in the temperate zones. But all navigators and all practical observers agree that there the tides only rise from one to three feet, while in the temperate latitudes of Nova Scotia and Great Britain they rise the highest—even as high as thirty or forty feet.

Keith—in Theorem 2d, p. 81, London, 1815—states that when the moon is in the horizon, the tide will be ebb or low water ; we all know that the reverse of this is true every where on the eastern coast of the

United States. In Theorem 3d, he says that the time of high water is a little after the time of the moon's coming to the meridian; we all know that it is then low water. Every almanac will prove the reverse of these two Theorems, and yet they are taught in the American schools as truths. This doctrine of the 3d Theorem is likewise advanced by Sir Isaac Newton in his *Principia*. It is certainly true as to the tides in London, but is not true as to the greater part of the kingdom. The following is extracted from some of the best English books on navigation:

	H. M.
High water at full and new moon at Lands' End,	4 30
“ Lizard,	5
“ Portland,	7
“ Ramsgate,	11
“ Portsmouth,	11 36
“ Margate,	11 45
“ Thames' mouth,	12
“ London,	2 46

This shows, conclusively, that the tides progress from the Atlantic Ocean up to the British Channel to the mouth of the Thames, and then westwardly up to London; and that the period of high water in London ought not to be taught as the time of high water everywhere.

Again. As the new moon is in conjunction with the sun, a great influence is ascribed to their united power of attraction. When they rise together in the east, we find the tide already high on the eastern shores of America. The theorists say that this is caused by their attraction; but I appeal to common sense to say, if the tides precede the rise of the sun and moon, whether they are not rather impelled forward before the sun and moon; and whether, if the tides were attracted by them, they would not have followed, and not preceded, their rise in the east.

But what are the changes of the moon, that such vast effects should be ascribed to them?—the control of mind and matter—the ebbing and flowing of the tides in all parts of the world, and the changes of weather at all seasons of the year. Are these changes of the moon any thing more than the greater or less quantity of the sun's light reflected from the moon, at the full and new moon, and at the intermediate quarters? If there be no other material difference, the theorists appear to base their doctrine on mere moonshine.

ART. V.—*A case of Muguet, occurring in a patient presenting some singular natural imperfections.* By W. T. WRAGG, M. D. Charleston.

Editors Charleston Medical Journal and Review :

GENTLEMEN—At the June meeting of the South-Carolina Medical Society, I gave the details of a case which had recently passed under my notice. It was considered sufficiently interesting to call forth a request from the members, that I would make a record of the facts for publication. It is in compliance with this request that I now give you the following narrative.

On the 15th March last, I delivered Mrs. ——— of her 15th child ; it was a male, remarkably large, and well developed in every respect, except that there was not the slightest appearance of hair on any part of the body. The skin of the head was smooth and shining, as in cases of senile baldness, and there was not a trace of eyebrow or lash to be seen. The nails were but partly developed, covering only about one half of the space from the root to the tip of the finger.

Very soon after birth the child began to suffer from pains in the bowels, and though furnished, within two days, with a healthy nurse, (procured with a view to dispense with the mother's milk, which had not seemed to agree with her previous children,) it continued to suffer, and about the fourth or fifth day the stools became frequent and green, as in cholera infantum.

This condition of the bowels continued throughout the child's illness ; the usual remedies failing to procure more than temporary and partial relief. Yet there was no interruption to the process of nutrition ; the patient growing and improving. At this time the appetite was good, and as the nurse had an abundant supply of milk, the child's wants were fully satisfied. Yet there was constant and increasing evidence of some derangement of the system, as shown by the green stools and the paroxysms of crying, which came on regularly as night approached. To these symptoms vomiting was added, which also increased till finally a large part of all the milk taken was rejected a few minutes, or longer, after being swallowed.

Two weeks after birth, small dark spots, resembling echimose, were observed about the head and face, coming on when the child was crying or making efforts to relieve the bowels ; and they gradually increased in size and number till they spread to the neck and body, and finally running into each other ; the entire body was of a dark lead colour,

like a patient in the last stages of cholera. The shade of colour also became darker and the paroxysms longer, till they finally left a tint upon the entire surface as deep as is seen in the worst cases of cyanose.

Under the impression that this state of the surface might be attributable to a weak capillary circulation, and, consequent upon that, an imperfect aeration of the blood, I employed iron as a tonic. The remedy seemed to answer the indications, for gradually the skin lost its permanent leaden tint, and finally the paroxysms ceased to recur. But in place of the dark colour the skin now presented the appearance of an entire absence of blood in the small vessels of the surface, so that the large veins resembled dark blue lines upon a snow-white ground.

The patient now showed signs of emaciation, and the skin, which had from the first been dry, was now hot and feverish. The stools were still frequent, consisting usually of greenish serum with specks of undigested milk. The paroxysms of pain were frequent and severe, being only partially, or not at all, relieved by the free use of paregoric, warm poultices, &c., which were constantly resorted to.

Soon after emaciation commenced, or about six weeks from the birth of the child, the mouth began to be very dry, and small specks of a purely white exudation were seen. These slowly but regularly increased in extent, till the whole mouth was coated with it. Soon it was so complete that scarcely a spot remained on the cheek, gums, lips, tongue, (both upper and under surface,) roof of the mouth, tonsils, palate or throat, as far as could be seen, which was free of the exudation. The consequence of this dry lining over the surface of the tonsils and palate, was a constant dry ringing cough, which was only temporarily relieved by the use of gummy cough mixtures.

The condition of the mucous membrane of the mouth and throat at this time, (it being coated with this exudation as far as it could be inspected,) taken in connection with the state of the bowels, the appearance of small portions of the exuded matter in the stools, and the extension of the disease to the orifice of the anus, induced me to adopt the conclusion that the entire length of the alimentary canal was invaded by it.

character. The result of this examination, as announced by Dr. Gail-
lard, who conducted the investigation, was, that the product was plainly
vegetable. The entire mass consisted of a cryptogamic growth, bearing
a strong resemblance to some of the algæ.

It was my good fortune to procure a portion of the exudation matter
from a case of thrush, just in time to submit the two to examination
together, and the comparison showed plainly that the latter differed
from the former entirely. Its structure exhibited the peculiarities of the
animal formation, being made up of cells and rudimentary hairs.

It was now evident that the patient was rapidly failing. The emaci-
ation increased, and the sutures of the cranial bones showed a depressed
appearance, as if the brain did not fill the entire cavity as completely as
is usual. It had been remarked, by those who had examined the child,
that the fontanelles were small, as if the ossification was further advanced
than usual at so early an age; and yet, when this degree of emaciation
had been attained, the bones exhibited an evident motion upon each
other. This was perceptible both to the sight and the touch, and was
particularly observable at the lambdoidal and coronal sutures. The
occipital bone became overlapped by the parietals, and the finger placed
on the suture indicated a considerable motion between these and the
frontal, whenever the child rolled its head uneasily about.

Two days before death, the eyes were drawn upwards and the pupils
contracted. Convulsions of short duration occurred at irregular inter-
vals from this time till the termination of the case.*

It was not possible to obtain an autopsy.

In reflecting on this case, it has seemed to me that the patient suffered
under some original defect of organization, and it was probably owing
to this defect that the capillary circulation exhibited such remarkable
symptoms. Whether the disease of the mucous membrane depended
on congenital causes or not, I am not prepared to offer an opinion,
having had but little opportunity for observing it.

It is very remarkable that of the fifteen children to which this lady had
given birth, three, besides this, had presented the same defects. One
took the whooping cough within two weeks of its birth, and soon after
this the cyanosed appearance coming on during the paroxysms of cough,
induced a belief that it was the result of injury to the foramen ovale
at this time. Another underwent a succession of symptoms not unlike
those of the present case, with the addition of an anomalous eruption
on the skin, in which vesicles and pustules were intermixed, resulting
in deep and inflamed sores. In this, as well as in the remaining case,

* The child having lived just 76 days.

the mucous membrane of the alimentary canal suffered in the same way as in the subject of the present remarks. No autopsy was obtained in either of these cases, for the same reason that prevented it in this.

This, Messrs. Editors, is about the substance of the remarks I made at the meeting of the Society. I did not then, for want of time, institute any comparison between the symptoms of my cases and those described by Valleix, &c., under the name of Muguet, nor can I do so now for the same reason. But this is of little consequence, since any one, sufficiently interested in the details given, may make the comparison himself.

ART. VI.—*The Influence of Quinine on the impregnated and non-impregnated Uterus.* By W. A. COCHRAN, M. D., of Alabama.

THE tonic and anti-periodic properties of cinchona and the salts of quinine, are too well known to need mention; no article of the *Materia Medica* possesses these properties in a higher degree, and none more commonly resorted to, when a simple tonic and stimulant are indicated. Its anti-periodic and febrifuge properties have established for it a reputation pre-eminent above all others, and from a happy combination of so many virtues, it has become a panacea of almost unlimited employment in many parts of our southern and western country. I am induced to make a few observations on some of the properties of the sulphate of quinine, from frequent opportunities of witnessing its effects in various forms of disease, as it constitutes the *base* of by far the majority of the prescriptions of the physicians practising in the South and West.

This indiscriminate use of quinine will be obvious, when it is remembered that in the sections above named, malarial fevers are most rife, and where nearly every intercurrent disease of a dissimilar character appears to be, to a greater or less degree, tainted with malaria, and amenable, in a great measure, to the remedy in question.

I do not intend to enter into a discussion of the various properties of quinine, nor its therapeutic application to particular diseases, as it has been the subject of much remark, particularly of late years, and there

but, so far as my experience with it has extended, I feel justified in stating that, in several cases, its effects appeared decided, and if my observations are correct, to a sufficient degree to claim attention.

I was first led to believe that quinine exerted an influence on the uterus, from noticing in the practice of my preceptor, Dr. Cobb, of Alabama, that females, when under the influence of quinine, if it happened to be at the menstrual period, often complained of a superabundance of the discharge; and, in some cases, it seemed to have the property of hastening on this period, if administered just before the expected return. These effects were attributed by the patients themselves to the quinine, and really could not readily be referred to any other cause. My preceptor informed me that he had seen more than one patient, who remonstrated against taking this article, upon the ground, as they said, that it brought their courses on when they did not want them. A case of this kind came under my own observation, in which the patient urged complaint against quinine; and, upon inquiry, she informed me that it always aggravated her catamenia, if taken at or near the time of her menstrual period.

I have also seen this article promote the flow of the menses, when they were suddenly arrested from cold, &c.; in some cases, a purgative was necessary, and after the patient was put fully under the influence of quinine and opium, I have had the satisfaction to observe, along with the copious diaphoresis that was induced, the menses reappear, with relief to all the symptoms.

My preceptor informed me that he has frequently employed quinine in similar cases, and with the same beneficial results.

I am fully aware that bark or quinine, in combination with iron, is a popular remedy in many uterine affections, as amenorrhea, or suppression of the menses of long standing, where a tonic is indicated; it is also useful in many other derangements of the uterus, as dysmenorrhea, menorrhagia, leucorrhea, &c., where these disorders are connected with a debilitated or anæmic condition of the system.

In such cases, the remedies just mentioned are useful by their tonic influence, improving the crasis of the blood, augmenting the red globules; in a word, giving tone to the system at large, and thus restoring the uterus to a healthy performance of its functions in common with

cases, to exert a peculiar influence on this organ, which showed itself early after a free administration.

Thus, the flow of the menses was augmented, if given at the time of the catamenial period ; and in other instances, it appeared to have the property of hastening on this period, and also of restoring them when suppressed.

The manner in which quinine promotes the uterine secretions, is hardly by any special affinity for this organ ; but, probably, it is by the impression it makes upon the nervous, the circulatory and the secretory systems. By it, the heart and arteries are excited to increased action ; the whole nervous system is thrown into a state of turbulent commotion, which is followed by sedation, languor, relaxation of the skin, copious diaphoresis, &c.

The action of quinine, particularly in large doses, is reputed to be sedative ; but this is not a true sedative ; in nearly all cases, there is a period of greater or less excitement, and when sedation is induced, it is secondary, and akin to that produced by large portions of alcoholic or other stimulants.*

Now, the uterus, as is well known, is a very delicate and sensitive organ, possessing an extensive sympathy with the general system ; and its function is variously influenced by whatever has a tendency to disturb the equilibrium of the circulation, or derange the balance of the nervous system.

Therefore, I infer that it is to this peculiar action of quinine upon the nervous and circulatory systems, together with its action on the skin, that its emmenagogue properties, if they may be so termed, are due.

The *modus operandi* of quinine, in arresting a febrile paroxysm, is, in the present state of our knowledge, very obscure ; the general opinion, however, is, that it is by the violent commotion it produces in the nervous system, and thus breaking into the chain of morbid actions which produce the paroxysm.

By whatever mode it operates, whether in the manner just related, or whether by a specific influence upon the malarial poison, we know that it is with a certainty and efficacy that at once strike our minds with wonder and admiration. The experiments of Magendie and Bernard, and those of Briquet and others, with quinine in large doses, are very interesting ; among many other phenomena, was observed hemorrhage from

* This view of the action of quinine is maintained by Drs. LaRoche, Guersant, McCraed and others. See *Charleston Medical Journal and Review*, November, 1853, p. 736

the uterus; but this hemorrhage was regarded as a toxicological effect, produced by too great a quantity, and similar to the effect produced by any other medicinal agent, in an overdose upon any other organ of the body.

That it possesses the property of promoting the uterine secretions, when given in medicinal doses, so far as I am aware, has not been particularly noticed; and if it really does exert such an influence, it is something singular that it has not been observed, and remarked upon, particularly in our southern and western country; and also, in the trans-Atlantic malarial regions, where it is so extensively and so indiscriminately employed.

Probably this may, in part, be accounted for upon the fact of the delicacy on the part of the female; they are always sensitive upon the subject of their menstrua, and will rarely ever make disclosures upon the state of their uterine functions, unless interrogated, or unless they become a source of inconvenience or concern to them; and, in some cases, where the remedy in question *did appear* to exert an influence on the uterus, which was made known to the practitioner, it has been attributed to some other cause.

Dr. Fenner, who is a warm and zealous advocate of large doses of quinine, and who would extend its use farther than his professional brethren at the South are willing to go, in a report on the prevailing diseases of the city of New Orleans, during the year 1850, observes, in his remarks on yellow fever: "Every woman I attended had hemorrhage from the uterus; or, at least, the catamenia appeared, and more profuse than ordinary, whether at the regular period or not. By some it may be supposed that the free administration of quinine influenced this discharge, but I think it was more justly attributed to the hemorrhagic diathesis produced by the fever." He also observes, that "hemorrhage from the mouth, stomach and bowels, were less common, though cases of each were seen." "I have heard it objected," he says, "that the hemorrhages seen this season, were of too mild a character to denote yellow fever," &c.—(*Fenner's Southern Medical Reports*, vol. ii, p. 89.)

If, then, the hemorrhagic tendency of the disease, as it occurred at that time, was mild; if this mark was so rare that it was doubted by some that it was yellow fever they had among them, I would ask, is it not possible that the frequent uterine hemorrhage seen by Dr. Fenner, was influenced by his liberal doses of quinine, which constituted the main part of his treatment? That one of the tendencies of yellow fever, to the production of hemorrhage from various organs, is notorious, I need hardly mention; and where this is observed to take place from the uterus,

when quinine has been freely administered, it is really a difficult matter to tell how much influence should be attributed to the tendency of the disease, and how much to the remedy employed in its treatment. Though it is a little remarkable that *uterine* hemorrhage should have been so constant in the practice of Dr. Fenner, when hemorrhage from the mouth, stomach and bowels were so rare; these latter organs almost always showing the signs of hemorrhage earlier than any other, unless it was that the *uterine* hemorrhage, as I have ventured to conjecture, was influenced by quinine. What tends still further to strengthen my conjecture, is the fact, which I have previously noticed, namely, that I have myself seen this tendency to hemorrhage from the uterus in diseases, and under circumstances too, where it could not readily be referred to any other cause than the remedy used in their treatment, namely, quinine, this hemorrhagic diathesis being perceived in no other organ.

Another important circumstance connected with the action of quinine, and one which may probably throw some light upon the mode of its action on the uterus, is, that when given in large doses, and frequently repeated, it *defibrinates* the blood, rendering it fluid and incoagulable. This fact has been clearly and satisfactorily ascertained by the experiments of Drs. Baldwin, Melier, Briquet, and other respectable authorities, all of whom furnish examples of serious results being produced by it.

The poisonous effects of this article have manifested themselves in several cases, even when given in very small doses. (See *Dr. Baldwin's Cases—Ranking's Abstract*, vol. vii., p. 190. Also *Dr. Van Buren's Report on the use of Quinine in Miasmatic Diseases—Medical Examiner*, Feb. 1846.) But how far this defibrinating influence upon the blood effects the uterine secretions, I am not prepared to say.

Having briefly and imperfectly noticed the influence of quinine on the non-gravid uterus, there yet remains another important question to be considered.

I would now ask, if quinine should be administered to the pregnant female freely and indiscriminately, and if it can be done with impunity? The tendency of violent impressions of an unpleasant character upon the nervous system to induce abortion, would, I think, excite in the reflecting mind a doubt as to the propriety of submitting a pregnant female to the influence of any medical agent, the action of which is so strikingly and so readily exerted upon the system as the one in ques-

violent commotion of the whole nervous system; and when sedation is induced, it is secondary, and, as I have previously remarked, akin to that produced by overpowering portions of alcoholic liquors.

Now, in our Southern and Western States, the pregnant condition of the female is not recognized as a state contradicting the use of quinine, nor as presenting the slightest obstacle in the way of its free administration; but, we are taught to give it boldly, freely, in large doses, regardless of idiosyncrasies, and without the slightest danger of abortion.

The shock imparted to the nervous system by full quininism, however great it may be, is not deemed adequate to the production of such a serious result. Dr. Coolidge, of the United States Army, stationed at Fort Gibson, Arkansas, in his report from that section, says: "In fevers, occurring in pregnant women, quinine was given more freely than in ordinary cases, it being deemed important to prevent a return of the paroxysm, in order to prevent abortion or miscarriage, a result never occasioned by the use of quinine."—(*Fenner's Southern Medical Reports*, vol. ii., p. 451.)

But, notwithstanding the freedom with which it has been administered to pregnant women by many practitioners, and the safety with which they have borne its effects, strong suspicions have been entertained against its safety in others, though, in the majority of instances in which it appeared to induce abortion, the evidence was not quite sufficient to establish the fact. This was owing to the circumstance of the influence of fever being associated with that of the quinine; and from the known abortive tendency of the former, it has been a difficult matter to determine how much influence was due to the quinine, and how much was owing to the fever. Among the first who particularly noticed the abortive effects of quinine, were, I believe, Rayer and M. Petitjean; the latter affirming that fifteen grains a day frequently produced the result. (See *Ranking's Abstract*, vol. iii., p. 313.) M. Petitjean asserts that he has so frequently seen abortion produced during the exhibition of quinine in the treatment of fever, that he has at length ceased to attempt the cure of that disease in pregnant women.—(*Ranking's Abstract*, vol. ii., p. 222. *From Gaz. des Hospitaux*.)

M. Rodrigues, however, contradicts M. Petitjean's assertion, that it is unsafe to administer quinine to pregnant women, and relates fifteen cases of intermittent fever occurring in pregnant women, which he treated with quinine without any evil results. He imputes the abortion to the fever, and not to the remedy.—(*Ranking's Abstract*, vol. iv. p. 263.)

Dr. Blair, in an article on the treatment of West India remittents

and intermittents by quinine, in the London Lancet for Sept. 23, 1848, speaks of such a result being imputed to quinine; but he adverts to it in such a light and passing way, that, it appears, he does not give it the slightest consideration. He says—"Quinine has been prescribed by the writer to patients of both sexes and all ages, and, where ascertainable, almost invariably to cinchonism, during thirteen years, and probably to the extent of several thousand ounces of the sulphate; and, during that time, he has seen no case of danger from its effects, with the exception of three or four cases of imputed abortion."

Cases of danger, not to include abortion, have been frequently met with elsewhere; if a lasting tinnitus aurium, deafness, blindness, and insanity, may be considered dangerous, not to mention a host of other transient, though, for the time, no less alarming effects, which every Southern practitioner, who has had much experience with this article, has observed, and to which he can bear witness, as resulting either from idiosyncrasy, too large a dose, or when given in forms or stages of disease which would not admit of its application.

The experiments of Briquet, and the experience of nearly every one, show that females, as a general rule, do not bear quinine as well as males; this is readily to be accounted for, when we reflect upon the mode of action of quinine, and the delicate state of the nerves of the female. What, then, could we expect from large doses of quinine, or even medium doses, given to the pregnant female, regardless of the conditions of her system, idiosyncrasies and other contra-indications, as is the practice of too many Southern physicians. I have myself seen this article, quinine, given to pregnant women, and without the slightest bad effects following its use, save the buzzing in the ears; but, I must say, that I know of *two instances* in which abortion followed its use, and, under circumstances too, which strongly intimated that quinine was the cause of it.

It is true, they were cases of fever, but were of a mild character, and could hardly be deemed sufficient of themselves to have produced such results.

My preceptor related to me that, in the course of his practice, he has noticed several instances in which it appeared to bring on contraction of the uterus, and which resulted in abortion; he has cautioned me against boldly and indiscriminately administering it to such patients, and enjoined, that when it was necessary to resort to it, it should be given with caution and circumspection.

The possibility of such a result is, I conceive, evident to any one who will think for a moment of the painful sensations of this state termed

quininism, especially when a delicate and sensitive female is the subject of it, at a time, too, when quietude and tranquillity are so essential to the carrying on of nature's favourite process. The remarkable proclivity of acute diseases to induce abortion, is proverbial; and there is hardly any affection that is so well calculated to bring about such a result as a severe febrile paroxysm, and nothing that so much excites the apprehension and anxiety of the practitioner; where, then, quinine has been used freely under such circumstances, and abortion is coincident, the miscarriage is apt to be referred to the disease, while the remedy is unsuspected.

Upon consulting the opinion of Professor Dickson, relative to the probability of such a result as abortion being produced by quinine, he replied in the affirmative; and stated that he himself had seen a few cases in which, he feared, abortion was produced. He also stated that, apart from the violent impression made upon the nervous system, the irritating operation it frequently produced upon the bowels was highly calculated to bring about such results.

In the opinion of Dr. Van Buren, of the University of New-York, the action of the sulphate of quinine, and still more of the super-sulphate, seems to be uniformly slightly irritating to the mucous membrane of the bowels, especially that of the colon and rectum.—(*Med. Ex.*, Feb., 1846.)

Professor Prioleau stated to me that he had frequently observed abortion coincident with the administration of quinine in fever, but he attributed it to the disease, and could not say that quinine exerted any influence. Dr. Cain stated that he had observed the same phenomena under similar circumstances, but that he had also imputed the abortion to the fever.

The question may be asked, what should be done when the pregnant female is attacked with fever? Should we stand with our arms folded, and allow the disease to make progress, which will itself be almost sure to result in abortion, or, what is still worse, the probable death of the patient? In such cases, I would reply, administer the quinine as boldly, and freely too, as the exigencies of the case demand; for, then, we will but substitute a lesser evil with a view to subvert a greater one. But, if I may be allowed to offer any restrictions in the use of quinine, I would offer that which reason, experience and conscience dictate, namely, that in the case of fever occurring in delicate, nervous and sensitive pregnant females, if the attack be mild and not threatening danger, a substitute should be employed for quinine if convenient, and more especially, if they possess idiosyncrasy against this latter, or if they be ad-

dicted or predisposed to abortion, and that they should not be submitted to the painful influence of quininism under any and all circumstances, where there is no absolute necessity ; and that where quinine is used, an eye should be had to its effects. While making these remarks upon quinine, and the evil consequences that appear to result from its indiscriminate employment, I would not be understood as wishing to derogate from the virtues of this inestimable article ; for the experience of the wide world has established its utility, and its introduction into the treatment of periodic fevers justly claims to be one of the greatest triumphs of modern times. It is a boon that progressive science has given to man, by means of which he can grapple with a fell destroyer which civilization has bequeathed to the inhabitants of the new world. I heartily subscribe to all that is contained in the eloquent lines of Professor Bartlett : " In all countries, and at all periods, since the discovery of the properties of this incomparable and invaluable substance, amidst all the conflicting dogmas of different medical doctrines, Peruvian bark has never failed to sustain its reputation, and to answer the expectations that have rested upon it. Amidst the manifold uncertainties of medical science, and the perpetual contingencies of medical art ; amid the disheartening scientific infidelity which has lately been taking possession of the medical mind, shaking to its deep foundations the firm old faith in the potency of drugs, and threatening to overthrow and demolish it altogether—it is gratifying and consolatory to feel and to know, that here at least we stand on solid ground, that here we may hold—that there is at least one great and important therapeutical relationship definitely and positively ascertained and established, defying alike the open assaults of quackery from without, and the treacherous machinations of indolent skepticism from within."—(*Bartlett's Treatise on Fevers*, p. 431.)

I shall forego the farther consideration of this subject, as I acknowledge my experience with quinine in the case of pregnant females is limited ; and while I leave the subject, I would appeal to those of more extensive observations for information.

ART. VII.—*Ozone in its relation to Intermittent and Remittent Fevers.*

By E. S. GAILLARD, M. D., Charleston District, S. C.*

WE will first state the most important facts which, in regard to this agent, we have been able to collect and collate from the most authentic sources, and then offer a few of the most reasonable deductions which seem to flow therefrom. - M. Schönbein, of Baslé, who first chiefly called public attention to the subject of ozone, regards it as "an independent body, and constituent of the atmosphere," uniformly present, though in varying quantities. Faraday, who has thoroughly and closely investigated this subject, and whose cautiously given opinions are entitled to the highest respect and deference, differs, in a few unimportant particulars, from Schönbein, but nevertheless, supports him entirely in all of those views which are of the most practical importance. Schönbein regards ozone as a binoxide of hydrogen, $H O_2$, but as this equivalent is not always uniform, he prefers to style it a peroxide of hydrogen. Ozone is generated by the passage of electric currents through the atmosphere, or rather by the discharge occasioned by the meeting of currents charged, as in the form of clouds, with different electricities, by electric combustion, and when the electric spark is discharged from the points of a conductor; by the oxidation of phosphorus, partly in water, and partly out of water, placed in large bottles with close stoppers, vapour is thus soon formed, and by washing the gas, which is insoluble, we remove phosphorus acid with the vapours of phosphorus, these last being soluble, and ozone remains behind; by passing a current of electricity across the surface of water in a vessel, &c. Plants, not excited by light, act upon the atmosphere, and produce this body. This gas, when concentrated, manifests an odour similar to that of chlorine, but when mixed with atmospheric air, the odour is "similar to that emitted from an electric machine when in motion." It is in its effects extremely injurious to the organs of respiration; mice perished in an atmosphere containing only 1-6,000th part of ozone. This agent is insoluble in water; destroys organic colouring matters, as well as those having albumen or lignin for their base; it has the greatest affinity for

* This is an extract from the Thesis, to which was awarded the premium of the Medical College of the State of South-Carolina, March, 1854. The observations of Dr. Gaillard are so interesting, the views expressed by him in reference to the agency of ozone, in the prevention of these fevers, so plausible and suggestive, that we present them to our readers, with the conviction that they will derive profit from the perusal of them.

oxygen of any substance known ; quantity prevailing in the atmosphere greatest in winter, least in summer ; purifies noxious inhalations, though exhibited in the minutest quantity ; destroys both idio and koino-miasmata, and is able to purify 540 times its volume of iniquated air ; hydrogen with selenium, sulphur and phosphorus, although in the minutest quantity in the atmosphere, yet render it unfit for respiration ; and ozone, from analogy, supposed to belong to the same class ; it is the most rapid oxidizer known ; rapidly destroys hydrogenous compounds, a constant source of miasm liberated in gaseous forms, by the decomposition of vegetable matter, &c. The true nature of these gaseous compounds is not known, but here it is sufficient to know, that if these gases were allowed to remain long in the atmosphere, this would be rendered unfit for respiration. Malaria may be either a mechanical or chemical combination of two or more of these gases, forming, by this combination, not a gas, or a compound gaseous in nature, as in the instance of water formed by oxygen and hydrogen. Faraday regards ozone as an allotropic condition of oxygen, but admits it to be present when oxygen is present, or where and when water exists in a vapouriform condition. The purifying properties of ozone are proved by the fact, that no deleterious substance has ever been separated from an atmosphere containing it in excess. Ozone rapidly diffuses itself in the air ; is decomposed by heat ; is manifested in increased quantity in the morning ; forms an oxide, or is capable of oxidizing nitrogen ; gives to the atmosphere peculiar chemical properties ; bleaching effect of sun's light attributed to it ; produced by the chemical agencies of electricity and magnetism. Schönbein mentions that in testing the atmosphere contained in a bottle half filled with turpentine (oil), which had been exposed to sun light and air, ozone was manifested. The decay, diseases and deaths of animals and plants, destroy ozone ; the ammoniacal and sulphur gases of the atmosphere are oxidized by it, forming thus acids and salts ; when in excess in the atmosphere, pulmonary diseases are rife. The method of forming the test paper for detecting its presence, is thus given : "Iodide of potassium, gr. i ; starch, grs. x ; distilled water, grs. cc,"—immerse a piece of bibulous paper in this solution, and then dry it for use ; when taken out there will be no change until damped with water. Iodine must be in its free state, to give the blue test with a cold solution of starch ; this is usually accomplished by a few drops of an acid, or a little chlorine water ; in the test paper experiment, ozone plays the part of this acid or chlorine water ; it disengages the iodine from the potassium, and uniting with the potassium forms potassa, while the iodine unites with the starch, and forms the blue iodide of fecula. The depth

of this blue will depend, of course, upon the quantity of iodine set at liberty, or, in other words, upon the quantity of ozone present. The air coming from the sea gives a deep blue colour; that passing over a city scarcely any. The current of air from a partly closed door or window, will give a marked hue to the test paper. We have given thus, without preparation or classification, the result of our examinations into the best authorities on this subject; these authorities we have found surprisingly few, and we believe that we have consulted most of those at present accessible. We proceed now to consider the relations of ozone to disease. The prevalence of this principle in the atmosphere, and the prevalence of certain diseases, have been lately said to be relative; that the greater the quantity or activity of one, the greater the malignancy of the other, and conversely; thus, the prevalence of pulmonary diseases and ozone are in a direct ratio, whilst the prevalence of ozone and the malignancy of malarious fevers are in an indirect ratio. This view has been assailed on the ground that the generation of ozone is not sufficient for it to become one of the controllers of the disease; that, on however large a scale it may be formed, during the electrical commotions of the atmosphere, these do not occur sufficiently often to furnish this principle in quantities at all proportionate to the fulfilment of the object claimed. Again, it is objected, that ozone is most abundant in winter, at a time when electrical disturbances are notoriously infrequent. Its being generated by the action of "plants not excited by light," is claimed to be not well established, and even if so, that this supply would be but a small fraction of the amount necessary; its preparation by the oxidation of phosphorus, under peculiar circumstances (from this being an artificial and complex process), is not at all regarded in the discussion. So far, we must confess that the objections to the views proposed, are plausible and just, but we cannot lose sight of one of the facts stated, in our desultory enumeration of the experiments, viz: that the passage of electricity across the surface of water in a vessel, was found to generate this agent with great facility. This, to us, seems one of the most important facts ever disclosed to the profession. What gives polarity to the magnetic needle, but those vast belts of electricity, constantly moving, and encircling our globe, and though the magnetic equator is constantly changing, its existence is as clearly demonstrable as the most elementary of philosophy's laws. To falsify this, would be to pluck the well merited chaplet from the brows of some of the most noble of those who have achieved distinction in the fields of science—Barlow, Brewster, Becquerel and others, toiling here, have rendered their names immortal. This fact is beyond dispute; and who shall undertake to assert,

that one of the objects of this subtle fluid, in its circle, is not by its action on the vast expanse of water which it spans, to elaborate an agent, bringing purity to an atmosphere which, otherwise, would soon contain within itself the seeds of a general and inevitable death. We cannot but think this entirely true, and examine as we may, there are facts which seem sufficient to support the suggestion in its minutest particular. Let us compare the general existence of ozone with some of these facts given. We find it to be the most rapid oxidizer known; that it is capable of exerting this action on all of those noisome gases and vapours which poison our atmosphere; that it is decomposed by heat, and that its excess is in winter. Is not the deduction plain? Ozone, it is reasonable to suppose, is formed nearly equally at all seasons of the year; when and where pestilential vapours and gases are greatest, is it not most largely consumed? These exhalations are, of course, greatest in summer, and as at this time we have high solar heat, the one consuming by oxidation, the other by decomposition, is it not a necessary consequence that ozone should be found "least" at this season of the year, and most in winter, when the causes of its consumption are in abeyance? Is there no reason why alluvial soils should be most unhealthy, as, from the gases and vapours which they evolve, consuming (with heat) the agent given for the purification of the atmosphere around them? Can we not thus understand why epidemics have prevailed in regions before unvisited by them? Thus, "chemical agencies produce ozone; decay, disease and death of animals and plants, consume it;" imagine this balance still preserved, but with increased solar heat, and is not disease developed? Again—have solar heat no higher than usual, but the balance between the production and consumption of ozone destroyed, will there not be disease? Lastly, as in the tropics, have intense solar heat, with the extreme of vegetable and animal decomposition producing mephitic exhalations, and must there not be those malignant forms of disease developed, which so characterize those regions of the globe? Fortunately, in that meridian, from the coincidence of the electric circle, there is reason to believe that this agent is most abundantly formed; but again, the least disturbance in the balance, from the causes there at work, must give rise to the fatal forms of fever peculiar to such latitudes. These remarkable properties of this agent, explain to us the cause of the marked mitigation of symptoms, so manifested in our fevers on the supervention of a summer's storm, and its highly oxidizing properties may serve to make intelligible the existence of malarious fevers in ferruginous localities, as in India, &c.; in the one, the mephitic vapours and gases of the atmosphere are neutralized by the in-

creased accession of ozone, and in the other, this agent is consumed by its extreme affinity for all oxidizable bodies. Ozone, from the absence of heat and diseased exhalation which, as we have said, respectively decompose and consume it, is most abundant in the morning; it forms an oxide with nitrogen, most probably the protoxide, and hence the exhilarating effect of the air at this period of the day. By the test paper, we find the air from the sea containing an excess of ozone; the reason of this is obvious and two-fold, as here the agent is most largely formed, and least consumed. The cheering and life-giving effect of sea air was known, and advised by the earliest writers of the profession, who, ignorant of the cause, yet profited by the effect. Is not the cause such as we have suggested, the absence of the *materies morbi* with the exhilarating combination of oxygen with nitrogen? Chemically explained, the combination of one atom of ozone with two of nitrogen, hydrogen being given off, would give us the protoxide of nitrogen. This cannot be too hypothetical, for Faraday, the highest authority, tells us that oxygen is capable in the atmosphere of existing in an allotropic state. "Ozone in excess is prone to the production of pulmonic disorders." It is a recognized fact, that eastern winds in America, and westerly winds in Europe, as coming immediately over the sea, have been usually attended and marked by this peculiar effect. Travellers in Africa tell us that pleurisies, pneumonias, &c., mark the coincident occurrence of certain winds, and that the natives suffer almost exclusively. This naturally leads us to the deduction of falsity in that theory which advocates the pulmonary inception of malaria; negroes, we know, suffer soonest and most (the remark above would substantiate this) from pulmonary affections, shewing thus their increased susceptibility to the action of those agents which interfere with the healthy condition of these organs; if the doctrine of the pulmonary inception of malaria be true, then this race should be the most susceptible, but facts notoriously prove the contrary. We are forced to make a reasonable deduction from the fact of ozone having been found, by test, in the atmosphere contained in a "bottle partly filled with turpentine, and exposed to the action of light and air." It has long been supposed, that the pines of our southern country "exerted a chemical effect" upon the poison of malaria; this, for a long time vague conjecture, now finds thus an interesting corroboration. What more probable, than that nature should, in the magnitude and magnificence of her laboratory, produce the same principle that we, in our miniature efforts, have boasted to originate; or what more calculated to impress us with the happiest conceptions of Omniscience, than that these stupendous agents, which produce it, should be

placed just where this purifying element is most needed and consumed. On reference to chemistry, we find the composition of turpentine to be $C_5 H_4$, and with this, the now interesting remark, "bottles in which rectified turpentine, not purposely rendered anhydrous, has been preserved, are often studded in the interior with groups of beautiful, colourless, prismatic crystals which form spontaneously; these crystals contain $C_{20} H_{16}; H_6 O_6$," or four parts of turpentine, with the incidental $H_6 O_6$ added. It needs but a glance to see that we have here the elements of ozone, though not in correct proportions; but we must again recollect Faraday's remark, that ozone is only an allotropic condition of oxygen; "no change of matter, but change of state." He instances this by phosphorus; heated up to 464° Fahrenheit, its composition remains the same, but its properties are entirely changed; it now requires a high temperature for combustion, but submitted again to a low red heat, it returns to its original state. Thus, then, any cause (farther exposure or chemical reaction) may change the relative properties or state of this new compound, thus forming ozone, whilst its actual composition, in elements, remains the same. Be this explanation correct or not, the fact of ozone being thus formed is undoubted. The balmy influences of our so-called terebinthinate atmospheres, here find a reasonable and plausible explanation. Lastly, we see that the dangerous accumulation of miasm is prevented by ozone, whilst conversely, the pernicious concentration of ozone is corrected and controlled by miasm; that miasm is chiefly the cause of malarious fevers, and hence, that any agent, capable of destroying it, bears, by its proportionate presence and action, the closest "relation" to the existence and malignity of such fevers; that where it is in excess, from the absence of those vapours and exhalations which consume it, fevers of this type are comparatively unknown, and where we have entirely the reverse, these fevers are malignant and often fatal. Much more might be said on this interesting subject; but we have already transgressed our limits, though we hope there has been shewn an intimate relation between the relative existence of this agent, and that of those fevers which form a part of our dissertation. We can not but think, that the variation of the magnetic meridian, as shown by the relative dip of the needle, would, if compared with the existence of malarious fevers, in various localities and at various seasons, lead to the most interesting and important results; or again, if the relative hue of the test paper be compared with the prevalence of various diseases, that a flood of light would be shed on the nature and effects of this highly interesting and important agent.

ART. VIII.—*Microscopical Examination of a Whale Fœtus.* By P. A. BROWNE, Esq., L.L. D., Philadelphia.

A SHORT time since Lieut. Maury sent me a small piece of the skin of a *fœtus whale*. I have examined it under the microscope, and cannot but consider it a very great natural curiosity. To my unpracticed eye* it would seem to be a sensitive skin, (or "*dermis*," as it is called by anatomical writers,) without a scarf-skin (or *epidermis*). My medical friends will not be startled at the idea of a "*dermis*" without an "*epidermis*," when informed, that it is not pretended that this forms the general condition of the covering of the cetacea; but only that of the fœtus of one species of animals belonging to that order. For all we know to the contrary, it *may* (up to a certain period of time) be the *fœtal* condition of the skin of the mammalia. The horny and insensible scarf-skin of this class of animals was intended to protect, from the action of the atmosphere, the highly sensitive *true skin* that lies underneath it: but, in the fœtal state, the skin of the young animal is not exposed to the action of the atmosphere, and, therefore, according to the wise laws which govern organization, viz: "that while everything that is necessary is amply provided, nothing unnecessary is produced and wasted," we ought not to be surprised to find a fœtus, at the earlier stages of its existence, enveloped in a *dermis*, but without an *epidermis*. The scarf-skin, whenever it exists after birth, is undergoing a constant process of formation and growth at its under part, to compensate for the wear that is continually taking place on its surface; but, in the case of the fœtus, this wear could not take place, and, therefore, the *epidermis* (if present) would become too thick to answer the purpose for which it was designed.

The true skin consists of two parts, viz: the corium and the papillæ, both of which are plainly to be seen on this specimen; but the *epidermis*, which in ordinary specimens of skins is situated immediately above, and resting upon, the papillæ, is entirely wanting.

I measured one of the papillæ and found it to be in length one and 1-40 of a millimeter, in breadth at the base 21-100, and at the summit 9-100 of a millimeter.

It is stated in the books, that "the scarf-skin is, originally, a transparent fluid, exuded by the blood-vessels, and *distributed as a thin layer*

* *Unpracticed* in the examination of *skins*, when compared with the great number of *hairs* that have been investigated.

on the surface of the sensitive skin." This presumes that the dermis is formed *before it*; but at what period after the perfection of true skin, the epidermis makes its first appearance, has not been noticed.

Medical gentlemen and those persons who have an opportunity of seeing the foetus of the lower animals, would do well to examine into this phenomena and publish the results.

ART. IX.—*Medical Heads and Medical Life in Paris.—Unpolished Sketches from personal observation.—The Chirurgeon-in-Chief of La Charité.*

"Crudelis ubique
Luctus, ubique pavor et plurima mortis imago."
Æn. 11, 369.

"Physicians mend or end us
Secundum artem, and though in health we sneer,
When sick we call them to attend us,
Without the least propensity to jeer."

Lord Byron.

SHOULD one enter La Charité at 7 o'clock of a winter's morning, pass through the first square, and, turning to the left, ascend the staircase, he will see long ranges of wards, much more than a hundred yards in length, with a double row of beds. Each of these compose only one side of a double set of quadrangular buildings, and there are two ranges, one above the other, for males and females respectively. Let him advance down to the extremity of one of these wards, and he is surprised at finding others leading off still as extensive as the first to which the stairs conducted him. Follow the last mentioned, and they branch again till they end in small blind rooms, containing each eight or ten beds, all with coverings and curtains of white linen. At different intervals, more or less distant, are visiting professors, in long white aprons, with or without velvet caps, and scarcely distinguishable from the half dozen Internes who accompany them; and around, in numbers varying with the popularity of the chief, stand crowds of medical graduates and students. The latter, though preserving to a certain extent the type of the genus, is more variant and less unique in Paris, than perhaps in any city in the world.

There are two or three, or a dozen, Frenchmen, generally small, bearded, and often badly dressed, and with note books containing fly leaves of white paper—the most industrious of all in taking down

everything that falls, for they have examinations to pass. The French medical student is often poor, but may rise to reputation, fame and wealth. The Italians are scarcely distinguishable from these, except that their beards and hair are blacker, and less of their faces is seen; they are also generally neater than a native. The Germans are more dignified, grave and sedate, for they are usually graduates, and men of age, and are seeking for something to compare with what they have seen in Berlin or Vienna; these are, consequently, extremely critical, and as they understand and speak French almost as universally and as well as a Russian, it is only their light hair and Teutonic physiognomy, which disclose their origin. There are not many English, and as the few remain but a short time, often merely to get a certificate of attendance upon Velpeau or Louis, to assist them at the College of Physicians and Surgeons, or at Apothecaries' Hall, we need not refer to them more fully. An Englishman never wears a moustache, and he is easily known.

Next to the Frenchmen in number, rank the Americans; they are generally tall, but with every hue, feature and appearance of every other race on the globe. Some are unmistakable, but others you could not distinguish from any body else. They adapt themselves, or offer without adaptation, any type that exists. They have an ambition to speak French—some cannot resist chewing the customary weed; many are very hard and regular students, others amateurs, who stroll in a morning or so, look at a great gun, find hospitals, before breakfast, not the things they are reported to be, and that is the last you see of them *here*. But many of our countrymen actually do not give themselves time to take their meals in Paris, so constantly and assiduously do they attend hospitals, lectures, cliniques, dissections, private courses, etc. An American, when he is a student, and that not seldom, goes ahead at it, and none surpass him. But he does not observe, reflect and draw conclusions as much as he crams. He does not lose a moment of the short time he has allowed himself for medical Paris, because he means to know the *Quartier Latin*, attend the Café Chantons, Mabilles, the theatres on the other side of the river, and see Europe besides. The American is, therefore, generally under high pressure. I can affirm that many do not give themselves time for digestion, consequently, with all the advantages of French cookery, they remain as they came, dyspeptic! On their way from one hospital or lecture to another, they fly into a restaurant, swallow down a breakfast at 10, or a lunch at 1, and away they go. If you follow him until 10 that night, he is still in motion. He has, in the meantime, taken a crowd of notes, which he puts up to read over and digest some day when he reaches home.

Now, instead of turning to the left as I proposed, should you open a

glass door to the right, here are still other ranges of wards, with branches leading off almost interminably. Those on the first floor are for males. Ask a patient what is the matter with him, and you will find it is a surgical case. Far from where you enter stand a throng of men, some crowding around a particular bed, some with hats on, others uncovered; from out their number moves an individual whom you did not, at first, observe, but whom every one seems to follow, as he passes rapidly from one bed to another, squeezing his way in among those encircling him. He is a person with a sharp, greyish eye, of middle height, more tall than short, inclined to be thin, and moving unobservingly by, unless when he stops, occasionally, as the inclination seizes him, to ask some question of a favourite chef-de-clinique, or of a sister. He will sometimes not speak or smile for a half hour, unless on the subject of medicine, or the case before him; at other times, he even smiles and makes a little dry fun with the sick man, though he never laughs, and often, if induced to relax at something particularly ludicrous, he soon recovers himself, becomes grave, or rather I should say earnest, and his eye looks as piercing as before. Neither his voice nor manner is soft or winning, although they show, when he speaks to a boy, or the gravity of the case is imminent, that his heart is feeling, and that even habit, time and the knife have not made him entirely callous to the sufferings of the unfortunate. I have heard him quiz a little *Interne* with white hair, upon the colour of his moustache; but it was not done in a genial way, it was as a surgeon would quiz, and you only laughed because the great man laughed. He, also, wears a white apron with pockets, from which dangle a pin-cushion, and on his head is a purple velvet skull cap, with tassel. Thin grey hair escapes from beneath, and you are quite surprised when, upon inquiry, you are told that that is the great surgical Amphictyon of La Charité and of France, M. VELPEAU.

Here is a true veteran for you; here is a surgeon with the three requisites—a lady's hand, an eagle's eye and a lion's heart; here is the man who enjoys the "*digitis monstror præterientium*;" you see, at last, him that you have been hearing of all your life, and before you ever thought of medicine; you have, perhaps, heard your cousin or your friend, who was in Paris years before you, speak of seeing Velpeau cut off an arm, or a hand, on such and such a morning, and you yourself also watch him do the same thing, on the same spot, fifty times; in fact, you see him perform two or three operations almost every morning you take the trouble to be at his amphitheatre at La Charité. He will cut for stone, operate two or three times for cross-eye, take out tumours from the breasts of two or three women, amputate a finger, a thumb or a leg, almost any morning

you choose to name, after he has visited these long lines of beds up stairs and down stairs, in male and female wards, and after he has lectured an hour, and all before breakfast. Heaven knows at what hour he has risen!—what he has not written in one of the works he is publishing, on the "*Maladies du Sein et de la Région Mammaire*," or some other. But I do know that he has not yet finished, because, before his carriage and pair leave the Rue Jacob, in front of La Charité, and when you have gone to the Café de Paris, to Madame Dijon's, to 66 Rue de Seine, or with me to my rooms on the garden of the Luxembourg, *he* has remained to prescribe for a score of what are called out-door patients, whom he examines as fast as they can be marshalled in a room on the ground floor. Then M. Velpeau's private consultations at his own residence, and his visits, have not yet commenced. But notwithstanding, you will sometimes meet him on the *Pont Neuf*, or the *Pont de Carrousel*, with a young lady on his arm, as like as father and daughter can be, as they walk to the Louvre or the garden of the Tuilleries. And you ask again, is this man, strolling quietly by, the hero of a hundred bloody operations, the creator of bold systems, and originator of new modes of practice—the bold generalizer, who has laid a master hand on almost every department of medicine; who has written a hundred volumes, whom everybody quotes, even men who study specialities, and who recognize that, during a life of ceaseless toil and exertion, *he* has made a speciality of every department of his profession, and has rendered himself as competent to pronounce upon their respective merits as any enthusiast of a single one, whether it be in surgery, materia medica, obstetrics, physiology, diseases of the eye, or anything you please to name. M. Velpeau is a bold, successful innovator in each and every one, and he has stamped his name in every text book that the student uses. He has written, and now writes, tomes filled with costly illustrations, which the medical world reads and consults, and swear at, sometimes, but always quote or follow. There are none so dauntless as not to do him reverence, none so bold but do not ask what Velpeau said this morning at a clinique. Does he discover anything, he goes to have Velpeau test, and make it go down. With Velpeau's sanction, he is confident against the world in arms, and goes forth with redoubled zeal. Does Velpeau damn the new project, it is like a dash of cold water, and the thermometer of his ardour falls instantly. I think that, generally, M. Velpeau, after his very large and most enlightened experience and observation, has a right to pronounce, and that he does so cautiously and with discrimination. He has, at times, been quick, and perhaps wrong, and now I think the microscopists will make him rue the

day that he decided too dogmatically against their true and false cancers ; for, already M. M. Lebert and Robin will show him that the glass is surer and more unerring than the unaided experience of even *his* eye and finger, and he will confess, or what is worse, posterity will do it for him, that true cancer and the unmalignant fibro-plastic growths reveal themselves by signs, more certain than those gained by observation and sight simply. Perhaps, too, M. Ricord, or his successors, will demonstrate, that in proclaiming his doctrine of syphilization, a true advance has been made which time will sanction and enforce, and that its cardinal points are *real*, which M. Velpeau utterly repudiates and rejects *ab ovo*, root and branch. Velpeau must recollect that Ricord, though young and vigorous, resembles him in one respect—he does not take vacation either.* An old man, however great, is apt to remember triumphs won in youth from giants, and he grows too confident thereby. He is apt to forget that the tables turn, as age advances and years roll by, and when the conflict lies between the old and the youthful, but, at the same time, the vigorous, the cautious and the observant.

As a lecturer, Velpeau is not remarkable, though he is sufficiently fluent and pleasant. Some new comers find his French easy to understand ; but though he does not mumble his words, and splutter, as M. Roux did, yet there are several in Paris who speak very much plainer and better than he. I could cite Trousseau and Maisonneuve for examples. His tone is somewhat monotonous, and however enlivened by an occasional dry jest, yet there is neither much eloquence nor any brilliancy. But Velpeau's attraction is his fame, his knowledge, and the perfect flood of light which his vast experience enables him to throw on every subject. You feel that he utters opinions which you can set down as rules and axioms for your guidance in practice. He speaks *ex cathedra* upon every topic ; his words are golden, for they flow refined from a vast mass of material collected during years of earnest toil, a toil directed by an enlightened understanding, and guided by an acquaintance and comparison, with not simply the stores of the past, but with the improvement of modern research.

Old and antiquated notions, and an adherence to early impressions, however false, cling to some who are only distinguished by age, arrogance and imbecility. But Velpeau, clothed in the ancient heavy armour of the veteran, still bears about him the keen and trenchant sword,

* M. Ricord informed me that, with the exception of the last, he had not indulged in the ordinary *vacance* of the Paris doctor, for twenty years. He is at the *Hospital du Midi* the year round.

as sharp and polished as that of any youthful champion around him. He may hedge himself in with precedent and experience, but he is constantly re-examining his precedent, and weighing and comparing his experience with that of others, whilst none contribute more fresh additions to them than himself. As his English compeer across the channel, old Liston, said : "Age is not the test of experience, but the possession of the greatest number of well assorted facts on any particular subject, whether they be got in five years or in fifty, constitutes experience."

Though Velpeau has a finger quite awry and misshapen, from some wound gained during the pursuit of his profession, I believe, yet this does not at all interfere with his manual dexterity. Not priding himself on it, he is yet ambidextrous. I shall be excused for saying that however skillful in this respect, he cannot surpass our countryman, Mott, who uses either hand, sinister or dexter, with the most perfect indifference to himself. I have seen Mott dissect out a tumour over the wrist of a patient, at a surgical clinique in New York, when he employed his left hand alone ; The individual did not lose an ounce of blood, and there were the vessels flowing just beneath, which had been evaded with a nicety surprising to the beholder. We know that, with a few rare exceptions, including some operations on the eye, the surgeon had always better avoid display, and manipulate with the usual and most natural member.

Verily, there is a charm and an attraction about surgery, which deceives and captivates the young, the inexperienced and the unwary. It operates directly upon the senses. There is a brilliancy and a decision about the movements of the glittering knife, which is entirely irresistible. It does its work so directly suddenly, and without appeal. The sight of the sudden gush of life blood welling up from the heart itself, has something about it which stimulates and excites ; so that most men start to be surgeon's as they do to be metaphysicians, but when they are past 30 they find they have been guilty of an expensive blunder, for the proportion of their medical to their surgical cases are as 10 to 1, and time and years are wasted in the attempt to make a Velpeau or a Malgaigne, when they might have been a much more respectable and distinguished as a Trousseau, Bouillaud or a Louis.

It neither requires talent nor genius to wield the knife, for the heavy and the phlegmatic, the stolid and the dull, operate when they cannot prescribe, and excise and amputate when they cannot cure. Traumatic injuries and morbid growths are rare in number, and slow in formation, whilst medical cases are frequent, and come thick and fast, at the same that they demand more time and care for their relief. To be a great

surgeon, however, in the enlarged application of the term, requires all the qualities of the mere operator, as well as those of the judicious medical man, but there must also be the material to give experience which only falls to the lot of a very few, even in the largest and most populous cities.

Among his peers, if peer he has, Velpeau is *facile princeps*. See him standing amid his gowned associates, as for example, when Orfila died, and his body was brought by them into the Church of the San Sulpice, or when he walks into the Academy of Medicine, or sits around the table at the Ecole de Medicine, when they are examining by *concour* that fine band of talented young Frenchmen contending for places. Hear how many of them will quote him, Malgaigne, Nelaton and Roux, and see how queer and characteristic he looks all the while. I always smile when I watch Velpeau!

His hand was sick last year, and it was curious to observe how the Internes flocked around it of a morning—how the sisters, in white caps, brought flax-seed, and the nurses flew for laudanum, and everybody offered their services, whilst old Velpeau disdained them all, and quietly dressed it himself. He never had it done at home, as everybody else would, by some member of his household, but, like a surgeon, he kept it to be taken care of by men in the midst of that theatre, where it had daily, for so many years, been performing such feats. I often looked on and smiled at the fun. I think, on one occasion, he did let one of them tie it up when he got through, and I presume it will be traditional, or like an heir loom in the man's family.

Singular to relate, this ambitious acquirer of professional lore does not speak English. Like most of his countrymen, he is content with the French, which they think everybody either does or should understand, and if they do not, that it is simply their loss. One morning, winter before the last, he stopped two or three of us on the stairs, to assist in translating what a raw Irishman said, who was attempting to obtain a certificate of six weeks' attendance upon the surgical clinique.

Velpeau has received a notice in the "*Biographie Universelle des Contemporains*," published as far back as 1834, from which we gather

He lodged at 7 francs per month, and expended ten sous (half-franc) daily for his meals. He was soon crowned at the *Ecole Pratique*, and nominated aid in anatomy. He gave private courses in anatomy, surgery and obstetrics. Before 1830, he had triumphed six times at the concours, out of ten essays, and he was nominated surgeon to the Hospital La Pitié. It would be needless for us to mention in detail, his subsequent successes, nor the list of works which he has written, though they are at our command. We may refer the reader to the paper cited above, and to a work by F. Campbell Stewart, of New York, on the "Hospitals of Paris," which we examined some years since. We may, however cite the following works, among others, written by Velpeau since 1830 :

Nouveaux éléments de médecine opératoire, accompagnés d'un Atlas de 22 planches in-4, gravées, représentant les principaux procédés opératoires et un grand nombre d'instruments de chirurgie, par A.-A. Velpeau, membre de l'Institut. *Deuxième édition, entièrement refondue, et augmentée d'un traité de petite chirurgie, avec 191 planches intercalées dans le texte.* Paris, 1839, 4 forts vol. in-8 de chacun 800 pages et atlas in-4.

Traité complet d'anatomie chirurgicale, générale et topographique du corps humain, ou Anatomie considérée dans ses rapports avec la pathologie chirurgicale et la médecine opératoire. *Troisième édition.* Paris, 1837, 2 forts volumes in-8, avec Atlas de 17 planches in-4 gravées.

Recherches anatomiques, physiologiques et pathologiques sur les cavités closes naturelles ou accidentelles de l'économie animale. par A.-A. Velpeau. Paris, 1843, in-8 de 208 pages.

Des Injections Médicamenteuses dans les cavités closes. Paris, 1846, in-8.

Manuel pratique des maladies des yeux, d'après les leçons de M. Velpeau, professeur de clinique chirurgicale à l'hôpital de la Charité; par M. le docteur G. Jeanselme. Paris, 1840, 1 fort vol. grand in-18 de 700 pages.

Exposition d'un cas remarquable de maladie cancéreuse avec oblitération de l'aorte. Paris, 1825, in-8.

De l'opération du trepan dans les plaies de la tête. Paris, 1834, in-8.

Embryologie ou Ovologie humaine, contenant l'histoire descriptive et iconographique de l'œuf humain, par A.-A. Velpeau, accompagnée de 15 planches dessinées d'après nature et lithographiées avec le plus grand soin, par A. Chazai, Paris, 1833, 1 vol. in-fol.

IATROS.

REVIEWS.

ART. X.—*On Rheumatism, Rheumatic Gout, and Sciatica, their pathology, symptoms, and treatment.* By HENRY WILLIAM FULLER, Cantab., Fellow of the Royal College of Physicians, London; Assistant Physician to St. George's Hospital, &c., &c. New-York: Sam'l. S. & William Wood, 261 Pearl-street. 1854. (From the publishers.)

It was Dr. Warren, we believe, who, when asked what was the cure for rheumatism, answered *six weeks!* The subject of rheumatism, or its allied affection, gout, is always an interesting one to the physician. The difficulties in the way of its treatment, the dangerous complications in which it is frequently involved, the pain it gives the sufferer, and the anxiety it imparts to the practitioner, always lead us to lend a ready ear to any detailed account of the elements upon which it depends for its existence, or with respect to the mode in which it should be managed. Let us proceed, then, to examine this work, by Dr. Fuller, which is most recently issued from the press, and which may contain new and valuable information. It is, we may say, a subject of regret that we are unable to obtain and compare M. Briquet's treatise on "Quinine," which has just been issued in Paris, from which his experiments at La Charite, upon its employment in articular rheumatism are given; however, we may detail some of the results of cases in which we have ourselves seen him employ it.

Dr. Fuller commences by discarding the influence of cold as essential to the production of the disease. The phenomena produced by cold are totally unlike, he says, the symptoms of rheumatism. No wandering pains, no redness or swelling of the joints, no symptoms, indeed, of any moment present themselves, unless the cold be intense, and even then they bear no resemblance to the symptoms of rheumatism. Both cold and moisture combined are asserted not to be the proximate or essential cause of the disease, are not essential to the production of rheumatism. These grounds are supported by various proofs drawn from

"It is so frequent an attendant on disordered conditions of the uterine system, that its intimate connection with retained uterine secretions has been suggested by Dr. Todd and other observers. M. Chomel has been struck by the frequency of its occurrence after excessive lactation, after inordinate indulgence of the sexual desires, and during tedious recoveries from fever; and all who have carefully studied the disease, in the vast field of observation afforded by the wards of our public hospitals, must have remarked how constantly its attacks have been preceded by functional derangement."

They go farther. Prout, and Todd, in his Croonean Lecture, pursuing Prout's idea as to the physiology of the disease, argues that its phenomena are referable to the presence of lactic acid "which is developed too freely in the system, in consequence of imperfect assimilation, and accumulates in the blood by reason of defective cutaneous action."

"It is no wonder," says Dr. Todd, "that as lactic acid is imperfectly excreted through its natural channels, in consequence of the influence of cold in checking perspiration, and is too freely developed in the alimentary canal, it should accumulate in the blood and become eliminated at every point. Moreover, the long continuance of the causes which produce the defective cutaneous secretion and the deranged gastric one, will rise to the development of lactic acid in the secondary processes of assimilation, thus infecting the blood from every source, and tending to perpetuate the diathesis."

Now, although the poison which gives rise to rheumatism has not hitherto received actual demonstration, yet many facts conduce to a belief in its identity with some natural excretion of the skin. In advanced life, when, from want of energy in the system, the skin's action is readily interfered with, pain or stiffness is so constantly produced by a draught of cold air, and subsides so soon after reaction has taken place, that we are bound to admit its intimate connection with temporary suspension of cutaneous action. Moreover, rheumatism is so common among persons suffering from renal disease, and other complaints accompanied by a harsh inactive condition of the skin, that it is impossible not to connect its appearance, under such circumstances, with the cessation of free cutaneous transpiration."

Its dependence upon the condition of the skin is further shown by examining into the phenomena of the disease; as for example, the excessive perspiration which is set up as if with the view of getting rid of some peccant matter, and the secretion is most profuse at the very part where local inflammation is taking place. The skin is also the true emunctory of lactic acid.

Cold, by depressing the system, may produce functional derangement, and so generate rheumatism. In the same way, damp and malaria act.

The hereditary character of rheumatism, Dr. Fuller contends for most strenuously, in chapter 2d. It is observed that a father affected with rheumatism, often imparts a hereditary tendency, not to rheumatism, but to gout in a child. A number of tables are given to show the age

at which the disease most usually affects the system, but the information is not sufficiently practical to be included in this notice.

The rheumatic poison shows an evident predilection for the serous structures, and a texture most usually selected is the white fibrous tissue which enters into the formation of the aponeurotic sheaths, the facial, the capsules of the joints, the ligaments, tendons, and its fibro-serous membranes in various parts of the body, as of the heart, and the strong white glistening sac of the pericardium. All these are examples of the albuminous and gelatinous tissues, "from the decomposition of which, in the wear and tear of the body, it is worthy of note, are formed those secondary organic compounds, the lithic and lactic acids, with which gout and rheumatism are ultimately connected."

"There are some curious facts in connection with this subject which require particular notice. It has long been observed, not only that the joints and their surrounding structures are particularly liable to be affected by rheumatism, but that some joints are more prone than others to be attacked. Those joints, for instance, which are most exposed, as the knees, the feet and the ankles, the wrists and the hands, are the parts most commonly implicated; next in order, perhaps, follow the elbows, and then the shoulders and the hips. The larger joints suffer more frequently than the small, and the small joints of the hands and fingers more commonly than those of the feet.

Those joints, again, which have been the seat of local mischief, are more than ordinarily apt to suffer. Thus, a man falls down and sprains his ankle or his knee, and, after a time, with care and attention, he entirely regains the use of the part, and experiences no inconvenience from his injury. But if he is afterwards attacked by rheumatism, the joint which had been formerly injured is almost certain to be affected, and will be so in every subsequent attack, and in all probability it will be the very part in which the earliest local symptoms are manifested.

The same holds good in regard to those parts which are chiefly exercised, more especially when the exercise has been violent and long continued. Blacksmiths, for instance, who wield a heavy hammer, very generally suffer most in the joints of the arm; washerwomen in the joints of the hands and fingers; gardeners, and others accustomed to digging or stooping, are exceedingly liable to lumbago and sciatica; and postmen, and others much employed in walking, are peculiarly subject to rheumatism of the legs. In all these cases the nutrition of the parts affected is unusually exalted, their power of attracting and separating from the blood such matters as correspond with them in chemical con-

ity of the urine also varying materially with the progress and increase of the disease. From fifteen days to six weeks is the usual period of duration of an attack, so that patients should not be impatient at tedious recovery. Among 136 cases of the disease observed by Dr. Latham, there were 24 of inflammation of the lungs, or its investing membranes. With reference to the *treatment* of rheumatic fever, we shall furnish in detail what our author says of it, with his own views as to its method of cure. He acknowledges the success of various means of eradicating the affections, but that "each and every plan of treatment which has been hitherto proposed, is regarded by the profession as unsatisfactory." Though when uncomplicated by cordeic affection, the disease usually terminates, sooner or later, in recovery, and "sometimes subsides, with marvellous rapidity, under every variety of remedy."

"If in one person's hands any particular remedial course has proved efficient, it has signally failed in those of another; if at one time a remedy has proved efficacious, it has been found inert or injurious at another, under different circumstances of age, sex, constitution, and the like. Nor does this appear strange to those who consider the true nature of the disorder, and the variety of circumstances under which the physician may be called upon to minister to his patient's relief. The bleeding, which in the young, plethoric and robust, may be necessary to allay excessive vascular action and cause free secretion, may, in the weakly, induce irritability of the heart, and a consequent attack of cardiac inflammation. The opium, which in one person may prove of the greatest service in promoting free perspiration, and in allaying the general irritability of the system, may, in another, check the biliary and other secretions, and thus prevent the elimination of the rheumatic poison. The continued use of calomel, and the constant purging, which may be beneficial to one patient by removing large quantities of unhealthy secretions, may unnecessarily exhaust the strength of another, and tend very greatly to impede recovery. And so in regard to every remedy which has been proposed. What is useful at one time, proves useless or positively injurious at another; and the conclusion is forced upon us, that what is wanted, 'is far less the discovery of untried methods of treating disease, than of discriminative canons for the proper use of those we possess;' far less the discovery of any new medicines, than the adaptation of our present remedies to the exigencies of each case."

Dr. Fuller commends a treatment of the disease based on our improved pathological knowledge. If it depends upon a *materies morbi* in the system, and that be lactic acid, let it be neutralized; let the system be purged of this substance in excess, and thus diminish irritation. We accomplish this by the free exhibition of alkalies and neutral salts, advancing the same object farther by the administration of purgatives, sudorifics and diuretics, to promote the activity of the various secretions, and eliminate the peccant matter.

But this is not all. Let us check the further formation of the poison, and restore that healthy state of assimilation, which, at the outset of the disease, is interrupted or arrested by the use of colchicum, mercu-

rials and alteratives. When the febrile symptoms are subsiding, employ tonics, quinia, for example, that the function of assimilation may be improved. The general irritability, particularly that of the heart, must, in the meanwhile, be allayed by opiates, conium, nitre and, if necessary, antimony, cautiously administered. If vascular action be excessive in the robust, blood-letting may be resorted to. Now, this is fairly explained, and we will see, in giving subsequently the precise method of cure adopted by the author, how they correspond. Before, however, citing this minutely, let us pass in review with our author, the consideration of the results of some of the principal agents that have been employed in the management of rheumatism, by some of the most distinguished authorities :

"And first, as to the heroic remedy of venæsection. In no disease is the febrile heat greater, the pulse fuller or more bounding, the local inflammation apparently more severe, and accompanied by more pain, than in acute rheumatism ; in none is the blood drawn more buffed or cupped ; in none, therefore, does depletion promise greater success. Accordingly, from the time of Sydenham, free blood-letting has been had recourse to more or less generally ; by many as an auxiliary to other treatment, by some as a substitute for all other remedies. Amongst the most recent and strenuous advocates of large and repeated bleedings, have been Mons. Bouillaud, in France, and Dr. Macleod, in England. These gentlemen recommended the abstraction of three or four pints of blood in the course of the first few days of the attack.* They endeavor thus to lessen the force of the heart and arteries, and so to moderate local action ; and they assert, not only that they are generally successful, but that, in many instances, they have arrested or cut short the disease.

Now, without denying the accuracy of their statements, I feel bound to enter an earnest protest against the mode of practice they purpose. In certain instances copious and repeated blood-letting does certainly appear to mitigate, for a time, the severity of the symptoms, and to render them more amenable to other treatment. But if some cases recover marvellously under its influence, so do they under the use of milder remedies : if, in some instances, patients go on to recovery without the occurrence of any untoward symptom, a far greater number experience relapses in which the disease returns afresh, with the fearful addition of inflammation of the heart ; if relief sometimes results from its employment, it much more frequently produces little, and but temporary abatement of suffering. Nor are these the only objections to its employment. Though pain and inflammation may be the most urgent and prominent symptoms of the disease, and may seem to demand venæsection for their relief, still, as these symptoms, if left to themselves, almost invariably subside without leaving any ill effects behind them, we are bound to hesitate before adopting a plan of treatment which cannot but affect the

* From three to six pints of blood were usually abstracted by M. Bouillaud, on the first three or four days of the attack ; and Dr. Macleod was in the habit of taking "from twelve to twenty ounces of blood several successive times, in the course of five or six days, from persons of average robustness."—(On 'Rheumatism,' p. 32.)

strength and ultimate well-being of our patient. Even if a predisposition to cardiac inflammation be not engendered, as I believe it to be, by copious and repeated blood-letting, still convalescence is retarded, the patient weakened and rendered liable to frequent relapses.* "Unde," says Sydenham, "non tantum ægri vires pro tempore franguntur sed si paulò funerit naturâ debilior, aliis etiam morbis ad annos aliquot obnoxior ferè redditur." Nor is this by any means a rare occurrence; many persons have remained in a weak and sickly condition for months, or even years, after having undergone Mons. Bouillaud's plan of treatment by bleedings repeated 'coup sur coup.'

But what of venæsection to a smaller amount at the outset of the disease? Would not a moderate bleeding be likely to modify the local action, allay the general irritability of the system, and favour the action of remedies? I confess that, according to my experience, phlebotomy is much too generally adopted. If it could be shown, that in the event of its being omitted, some serious mischief would arise as the effect of the articular inflammation, that the heart would be implicated, or that some serious consequences would result from the severity of the febrile disturbance, then there would be no objection to the use of the lancet. But the experience of all ages has proved, beyond dispute, that the febrile paroxysm is usually unaccompanied by danger; that disorganization of the joints very rarely occurs, and supervenes as often, when bleeding has been practised, as when no blood has been taken from the arm; and recent observation adduces evidence to show, that inflammation of the heart is not more common, if, indeed, it be not actually less frequent, where other remedies are employed, than when a treatment is adopted in which phlebotomy has its share. Mons. Bouillaud, the great advocate of copious and repeated venæsection, acknowledges inflammation of the heart to have been the rule, and not the exception in his practice: 'Coincidence a peu pres constante soit d'une endocardite, soit d'une pericardite soit d'une endo-pericardite avec un violent rhumatisme aigu.' And Dr. Macleod, who makes no mention of the frequency of endocarditis, reports pericarditis, alone in nearly one-fourth of his cases. On the other hand, Dr. Corrigan, who rarely had recourse to blood-letting, observed only one instance of it among many cases treated by opium; and out of thirty-nine cases in my own practice, it has occurred but twice after the commencement of treatment.

As, therefore, there is no absolute necessity for the use of venæsection, as it usually affords only temporary relief, and as by the treatment hereafter to be mentioned, the pulse may be quieted, the pain allayed, and the fever and articular inflammation subdued much more rapidly and certainly than by the most active depletion, I would not recommend the abstraction of blood as a part of ordinary practice. In some instances it is required, and is highly beneficial; but repeated observation has led me to believe, that it is so only in first attacks occurring in young, robust, and generally healthy persons, particularly in those which

* This was so remarkably demonstrated by M. Bouillaud's cases, that the doctor is forced to admit it to be necessary, "d'éviter avec le dernier soin le plus léger refroidissement. (Op. cit. p. 137.)

are marked by unusual severity of their symptoms, or are unaccompanied by free perspiration, and by other evidence of activity in the excretions. Even in such cases it should be cautiously employed, and carried to a small extent only, viz: from eight to twelve ounces, according to the age and strength of the patient, the object being to favour the action of other remedies, and to promote free secretion, rather than to arrest or cut short the disease. In the recurrent disease, venæsection, even to a moderate amount, is always unattended by benefit, and is sometimes followed by dangerous consequences; in mild cases it is unnecessary, and therefore to be avoided; in the delicate, or in those of a weakly constitution, it is clearly inadmissible; and in the strongly predisposed, or when the disease is hereditary, it utterly fails in modifying the morbid action, is often ill borne by the system, and therefore should seldom be practised."

Dr. Latham, in his "Clinical Medicine," only considered venæsection as preparatory and auxiliary to other remedies. Heberden (Commentaries,) thought the expediency of general bleeding quite questionable. Dr. Wilson, in his "Report on the diseases of London," says that "by bleeding repeatedly the *pains, swellings and febrile symptoms* are not only aggravated at the time, but often protracted indefinitely." Dr. Fordyce says in his "Third Dissertation on Fever," "that while it was his practice to remove the general inflammation by bleeding," yet, "that metastasis frequently took place to the interior parts of the body, and destroyed the patient." He adds: "This accident in the author's practice during the last fifteen years, has rarely happened. In this period he has entirely left off bleeding in acute rheumatism, and has not lost above two or three patients, although he has treated several hundreds in this disease." Allison, Hawkins, Watson and Seymour are quoted by our author, all of whom give testimony to the dangers of injudicious or large venæsection. Dr. Fuller himself, seldom has recourse to it; he attributes the tolerance of blood-letting to the irritant property of the rheumatic poison, and he asserts that the cupping and buffing of the blood will continue to the last; "will continue when depletion is no longer safe, and when it has been carried to such an extent, that an anæmic murmur accompanies the heart's sounds, and a loud *bruit de diable* is audible in the large veins of the neck."

We have ourselves frequently seen Bouillaud in his wards at La Charité, abstract blood to an enormous extent, and though he continued to proclaim its utility, yet there was not sufficient opportunity allowed us to decide as to the ultimate results.

Purging, particularly with calomel and senna, are servicable, no doubt,

the heart has supervened immediately after the exposure, and the increase of irritability consequent on several successive purgings, and more than once I have been led to believe that this unfortunate occurrence has resulted from the treatment adopted. Accordingly, our constant endeavour should be to obtain a free evacuation every morning, without the risk and great discomfort attendant upon repeated purging. The remedies by which this object is best obtained are, calomel combined with a full dose of opium at night, and followed, when necessary, on the following morning, by a draught composed of rhubarb or senna with colchicum, and the potassio-tartrate of soda, in just sufficient quantity to produce one full dejection. By these means, not only is an abundant secretion secured from the liver and bowels, but by waiting some hours before administering the purgative, and thus giving the calomel time to do its work, we carry off the excrementitious matters quite as thoroughly as by repeated active purging.

There are certain cases, however, in which larger and more frequent demands may be made upon the intestinal secretions. It not unfrequently happens that persons attacked with acute rheumatism have been constipated for some time prior to their illness. Such persons present all the symptoms of biliary and intestinal derangement in a very marked degree. They complain of a sour and disagreeable taste in the mouth, and of fulness and distention at the epigastrium; their conjunctivæ are yellowish, their breath is often foul, the tongue inclined to be dry, and very furred and yellow, the motions dark-coloured and offensive, the appetite altogether lost. In these cases, there appears to be so much torpidity of the primæ viæ, such an absence of healthy intestinal secretion, and withal, probably, so large an accumulation in the system, of matter, which ought to have been excreted by the bowels, that it is expedient to exact at least two full alvine evacuations for the first three or four days of the attack. And they are generally accompanied by signal relief. The aperients, though administered in full doses, produce no griping, no tenesmus, no appreciable distress; on the contrary, they are followed by copious, dark-coloured, offensive dejections, the getting rid of which is evidently conducive to the comfort and well being of the patient. He expresses himself sensible of immediate relief, and the correctness of his sensations is attested by the greater moisture of his tongue, the absence of the previously existing fetor of his breath, and the marked improvement of his general symptoms."

In addition the use of opium in acute rheumatism, is plainly indicated from the pain, irritation and excitement accompanying. But Doctor Fuller does not, by any means, sanction the ordinary practice. He says that the custom is to administer from two to four grains in the twenty-four hours, premising a blood-letting, and acting upon the bowels by

the nervous system when roused and excited by the agency of acute rheumatic inflammation; there is, then, so much of action and of suffering, that I have known four grains taken daily by a child only ten years old, without the production of any sensible effect beyond that of alleviating his suffering; and if this medicine is to be made available for relief, it must be given to adults in much larger doses.

Dr. Cazenave, of Pau, was the first to recommend the exhibition of large and repeated doses of opium for the cure of acute rheumatism; but the physician to whom the profession in this country is indebted for the practice, is Dr. Corrigan, of Dublin. He generally begins with a grain every two or three hours, and recommends that the dose be increased, "both in frequency and quantity, until the patient feels decided relief, when it should be kept up at that dose until the disease is steadily declining." The average quantity required in twenty-four hours, he considers to be about twelve grains; and even that quantity does not affect the head. Diarrhœa, he says, sometimes occurs while the patient is using the opium in full doses, and purgatives are seldom required. In one case no less than 200 grains were taken, in the course of a fortnight, with manifest relief.

Of this method of treatment, I cannot speak from personal experience, inasmuch as, by employing other medicines in conjunction with opium, I have seldom been obliged to make use of it so unsparingly. But I can testify most strongly to the value of the sedative in full and repeated doses; in doses far exceeding in amount the quantity usually administered. In the early and most painful stage of the disease, occurring in adults, it may often be given with the greatest advantage to the extent of six or eight grains in the course of twenty-four hours; and to children may be administered, without the slightest fear, in half-grain doses, repeated every three or four hours. In doses, such as these, when combined with the treatment hereafter to be mentioned, I have never seen it check secretion, or produce the slightest cerebral disturbance; rarely, indeed, has it even occasioned, but it has calmed the patient's irritability, alleviated his sufferings, and has thus prevented the wear and tear of the system, arising from a long and painful illness. More than this, too, I believe it has effected. I am satisfied that in many instances it has materially hastened the period of convalescence, and has lessened the frequency of inflammation of the heart. Therefore, whilst I join issue with those who would treat acute rheumatism by opium alone, I admit most fully the advantage of its employment in conjunction with

priety of its administration and of the extent to which its exhibition is required, and that whether ten or two grains only are needed in the course of the day, it may be given with impunity, and indeed with advantage, as long as pain and restlessness continue.* I know of nothing to contraindicate its use, save only the suspension of secretion; and I am aware of no circumstance calculated to point to its having been given in too large doses, except the supervention of stupor or narcotism. On neither of these points is there the least cause for alarm. The latter symptom will never arise, if the medicine be given cautiously, and its operation carefully watched; and observation has convinced me that it suspends secretion in exceptional cases only. Indeed, it appears probable that opium, when given in quantity sufficient to subdue the pain and allay the irritation, so far from impeding or suspending secretion, conduces, in some instances, to its promotion. Certain it is, that in the excited state of the nervous and vascular systems, arising from the irritation of the rheumatic virus, all the excretory organs, except the skin, perform their function slowly and imperfectly; the scanty urine, the dark coloured, unhealthy motions, and the coated tongue, testify abundantly to this important fact; and equally certain is it, that during the administration of opium, the urine frequently increases in quantity, the motions become more healthy in appearance, and the coated tongue cleaner and less red. So that, on the whole, the treatment by opium is a safer and a better plan than the treatment by purgatives, if the bowels have been acting regularly and still continue to do so; but the treatment by purgatives is the most efficient and most certain, if the bowels have been previously bound and still show a disposition to be confined."

In rheumatism, some have pursued the plan of inducing excessive perspiration by every means. But this Dr. Fuller condemns, except in cases unaccompanied by a free perspiration. He also rejects the method by mercury and opium, to induce salivation; he agrees with the observation of Macleod, "that the rheumatism has continued, although the mouth was affected, while it has speedily subsided on continuing the narcotics and purgatives, without the mercurial." At the same time, that the calomel has no influence over the rheumatic poison, nor does it assist in preventing the access of cardiac inflammation. Eudocarditis and pncarditis supervene as readily under these conditions as not.

Laennec proclaimed the value of tartar emetic in the disease in question, asserting that it relieved it in seven days, and that it was only more powerful in subduing one other affection, viz: pneumonia. Our author regards it simply as useful, and as an auxiliary, "being insufficient of

Our author in reviewing the influences of bark in the management of acute rheumatism, scarcely, if at all, refers to M. Briquet's employment of quinine in its management, during his service at La Charité, where he continues now to give quinine frequently in the disease. We have ourselves repeatedly seen this gentleman administer quinine, and with much success. He published papers on the subject, some years since, but one or two fatal having occurred, the profession generally, were deterred from its employment; but more recently the practice is again in vogue in Paris, both Briquet and Valleix, at La Pitié, using and recommending it strenuously. Although we have watched both of these gentlemen administering it, yet we regret being unable to obtain the testimony afforded by the former, in his work just issued. He gives from two to ten grains of quinine, frequently repeated during the day.

Of colchicum, guaiacum and nitrate of potash, our author says :

"Colchicum is a medicine which has been lauded as much too extravagantly by one class of practitioners, as it has been abused too indiscriminately by another. By some it has been called a specific for acute rheumatism, and by such has been exclusively relied on for its cure; and in proof of its efficacy, cases have been adduced in which the symptoms of the disease has not only disappeared under its influence, but their subsidence has been accompanied by profuse evacuations from the kidneys, the stomach, or the liver and bowels. The pain has abated, and the pulse has become more tranquil, coincidently with the occurrence of purging, or vomiting, or diuresis.

In stating these facts, I am saying all that can be said in favour of colchicum administered alone for the cure of acute rheumatism. All practitioners will admit, that when it causes purging or vomiting, or excites an abundant flow of urine, it may possibly, of itself, effect a cure; but few, I think, are disposed to ascribe to it a curative power, except under these conditions. Purging and vomiting, however, are among the first symptoms of poisoning by colchicum, and are often accompanied by such extreme prostration, and by so much gastro-intestinal irritation, that when it becomes a question whether the cure of the disease can be safely entrusted to colchicum alone, the prudent physician very properly hesitates in recommending or adopting so dangerous an expedient. He refuses to rely solely upon a medicine which is apt to produce such disagreeable effects, and tries to discover some method by which he may obtain from it its virtue as a remedy, without running the risk of its action as a poison.

Now this may be effected by administering it in small doses in combination with other medicines; and although in this form, it may not have the power of *curing* acute rheumatism, it has at least the property of alleviating its symptoms, and shortening their duration. It does so, I believe, not by operating simply as a sedative, nor by acting specifically upon the rheumatic virus, but by promoting its evacuation by the kidneys, and by exercising some influence over the process of assimilation, whereby the formation of the poison is checked. Be this as it may, immediate benefit so repeatedly results from its administration in conjunction with small doses of calomel, ipecacuanha, alkalies, and opium.

and in chronic cases is so frequently obtained by its administration uncombined with other remedies, and in doses insufficient to produce any sensible effect upon particular organs, that no one can hesitate to assign to it the improvement observed in the instances alluded to.

But it is not equally beneficial in all cases of acute rheumatism, nor in all forms, nor at all stages of the complaint. It is far less efficacious in the weak and nervous than in the more robust, and less easily depressed, and of less value in purely fibrous rheumatism, than in cases complicated by synovial inflammation. In my hands, too, it has proved less advantageous in proportion as the fever has exceeded the articular swelling, and as the urine has been less highly charged with the lithates.

But though colchicum is of the greatest value in the treatment of acute rheumatism, its operation must be most carefully watched. Throughout its administration it is impossible to insist too strongly upon the necessity of securing a daily evacuation from the bowels, and of attending to the other general symptoms of the disease. If the lithates disappear from the urine, if the pulse becomes weak, if faintness, or nausea, or purging supervenes, its use must be instantly discontinued. But until some one of these symptoms occur, a grain, or a grain and a half of the acetous extract, or the inspissated juice, or from fifteen to twenty minims of the wine of colchicum, may be safely and with advantage, administered two or three times a day.

Of guaiacum I have had considerable experience. I have seen it administered by my colleagues at St. George's Hospital, sometimes alone, sometimes in conjunction with other remedies, in a large number of cases, and I have myself prescribed it on several occasions, with some apparent advantage. Observation, however, has not led me to form a very favourable estimate of its curative power in this particular form of disease. In persons of a weakly, broken-down constitution, and in sub-acute cases unaccompanied by the characteristic acid perspiration, it has sometimes proved of essential service in stimulating the action of the skin; but in most acute cases, if it has done no harm, it has effected little good as compared with other medicines: it has neither appeared to mitigate the symptoms, nor to shorten their duration.

There is, however, as much difference in regard to the action of this medicine in different cases, and in different doses, as there is in regard to colchicum. Dr. Seymour, who was in the habit of giving the *mistura guaiacaci* of the 'Pharmacopœia' in full and repeated doses, says, 'It acts not as a stimulant, but as an evacuant, provoking purging, perspiration, and a flow of urine in a very violent manner: sometimes one, sometimes all these effects follow the use of the medicine.' Now, in my experience, the beneficial effects of guaiacum in the acute disease are observed precisely under the condition pointed out by Dr. Seymour: they are obtained in those cases, and in those alone, which are unaccompanied by perspiration, and in which the excretory organs are greatly excited by the action of the remedy. But in ordinary doses, and under ordinary circumstances, when the patient is perspiring freely, and when it neither purges nor causes diuresis, very little benefit results from its employment. It neither modifies the local action, nor limits its duration; and as the purging induced by its operation is sometimes most

distressing, its exhibition should be reserved for subacute or lingering cases, in which, when it provokes a free perspiration, its beneficial influence cannot be doubted.

The nitrate of potash has been largely employed for the cure of acute rheumatism, both in this country and on the continent, in doses varying from a few grains to two or even four drachms three or four times a day. In the smaller quantities I have frequently given it in combination with other remedies; and on several occasions I have seen it administered to the extent of an ounce in the course of four-and-twenty hours. Generally, it has been without obvious action on the excretory organs, and has exerted little influence over the intensity of the symptoms, or the duration or ultimate issue of the disease."

Dr. Fuller has watched the influence of nitrate of potash, when taken to the extent of an ounce daily, in seventeen cases of acute rheumatism, and to a smaller extent in several others, and "in one instance only, did it appear to exercise any decided control over the course, or duration of the symptoms." In the cases detailed by Dr. Barbar, the patients were on an average, four weeks under treatment; still Dr. Fuller acknowledges its influence in checking the force of the heart's action, and controlling the tendency to fibrinous deposition, by increasing the solubility of the fibrin. "It is valuable in acute rheumatism, not so much by affecting a cure of the disease, as by tending to prevent those untoward complications which render it so formidable."

Dr. Owen Rees supposes that *lemon juice*, given in doses of one or two ounces, repeated two or three times a day, cured rheumatism, by promoting the conversion of lithic acid into *urea* and carbonic acid, thereby favouring its excretion from the system; "while the small quantity of alkaline citrate which it contains, contributes also, in some measure, towards effecting a cure." Dr. Fuller says that this means, recommended by its simplicity, is now generally abandoned. The average of Dr. Rees patients was twenty-five days for the cure, and they were ill for a period of forty days.

The following lengthy extract we are now induced to give, in order to allow our author fully to disclose his views as to the method of treatment, and that our readers may fairly see what are the means which his recent experience has afforded:

"It will be gathered from what has been already stated, that my chief objection to many of the expedients which have been resorted to for the cure of acute rheumatism, lies not so much against the remedies themselves, as against the mode in which they have been employed. Each remedy, or class of remedies, has been too exclusively relied upon. There may be occasions in which blood-letting or opium, or calomel and active purging, are necessary; there may be circumstances which call for the exhibition of guaiacum, of nitre, of colchicum, or other agents; but it seldom, if ever, happens, that the cure of acute rheumatism can be safely entrusted to any single remedy. For, as the disease presents different aspects in different cases, so does it also at different stages in the same individual; and even were it not so, the constitutional disturbance

is so great, the cause of the derangement is so widely spread, and its effects extend to such a variety of organs, that every principle of medicine points to a compound method of treatment as most likely to lead to a successful issue.

The treatment to which I usually have recourse at the outset of the attack, is that alluded to at the beginning of this chapter. It is made up of alkalies and the neutral salts, with colchicum, calomel, and opium. Sometimes a little antimony is added, sometimes the aid of purgatives is had recourse to, and occasionally, though rarely, I deem it expedient to premise a moderate blood-letting. Baths are never employed if the skin is acting freely; but if, instead of being bathed in perspiration, it remains dry and hot, and burning, I then endeavor to stimulate its action by means of the vapour or the hot air bath.

As venesection, if employed at all in acute rheumatism, is to be made use of with the view of producing an impression on the train of morbid actions, and expediting the operation of other remedies, it must necessarily take the lead of all other measures; and the first question to be decided, therefore, in every case of acute rheumatism, is as to the propriety of having recourse to its employment. It has been already pointed out, that the use of the lancet is not necessary for the relief of the pain, or the tranquilization of the pulse, and that in the pale and weakly it exercises an influence prejudicial to the patient by rendering more irritable his already irritable and excited heart. But in the young, plethoric, and robust, in whom secretion is insufficient, whose pulse is full and bounding, and whose skin is dry and hot, and burning, it certainly does assist in expediting the action of other remedies, and so in promoting recovery. These, therefore, are the only cases in which it should be employed, and a single bleeding of from eight to ten ounces is generally sufficient. It relieves that excessive congestion on which the want of secretion, in great measure, depends, and which forms an obstacle to the action of those remedies on which we rely for effecting a cure.

The next is as to the expediency of giving calomel and purgatives. If the bowels are acting once a day, it is seldom necessary to make a more frequent call upon their activity, but a dose of calomel and opium may be prescribed with the view of modifying the character of their secretions. If the bowels are sluggish in their action, and the dejections dark-coloured and offensive, a dose of calomel, combined with opium, should be administered at once, and followed after the lapse of six or eight hours, by a draught containing the infusion of senna, together with half an ounce of the potassio tartrate of soda, and twenty minims of the vinum colchici. And the amount of opium should be so adjusted to the dose of the purgative, as to procure one full and copious evacuation without the distress attendant upon purging.

When once the bowels have been freely acted on, the state of the secretions must be our guide as to the repetition of the calomel and the morning laxative. If the tongue be rather dry; if the bowels continue sluggish, and the dejections dark-coloured and offensive, the mercurial and the purgative should both be repeated for several successive days. If, on the other hand, secretion from the bowels be healthy, the further

exhibition of mercury is unnecessary. If, again, the secretions be copious but unhealthy in character, the calomel and opium should be repeated at night, but need not be followed by a purgative in the morning, as after one or two doses of the mercurial, the motions become lighter coloured, more bilious in appearance, and of a less offensive character.

Whilst the state of the intestinal secretions are thus attended to, alkalies or the neutral salts should be administered in combination with colchicum, full doses of opium, and sometimes a little antimony. At one time I used to content myself by giving a saline draught, with the addition of 15 or 20 grains of the carbonate of potash, or the carbonate of soda, three or four times in the course of the day, but it soon became apparent that in order to obtain the full benefit of alkalies, it is necessary to give them in very much larger quantities—in doses proportioned to the extreme acidity of the system. In large but ordinary doses they generally mitigated the severity of the symptoms, yet failed in affording more than partial relief, but when they were exhibited in sufficient quantities, and in combination with other remedies, the most agonizing pain was speedily removed, and the fever subdued with marvellous rapidity. I have, therefore, ever since administered them largely, and have pushed them until my object has been attained. Nor have I seen reason, on any one occasion, to hesitate in following out this plan of treatment. It has now been pursued in a large number of cases, and in almost every instance has produced the most astonishingly favourable results. The patients have speedily lost their pains, and have proceeded rapidly to convalescence. In 23 out of 39 cases in my note-book, the pulse was tranquilized within 48 hours from the commencement of treatment, and in 28 the pain was lulled, and the local inflammation greatly subdued within the same time, whilst in the remaining cases a longer period was required, in consequence either of previous constipation, or of the co-existence of some internal complication.

The form in which I usually administer the remedies, is that of a simple saline or a nitre draught, to which, if the patient be a person of average strength and robustness, bathed in profuse perspiration, with red, swollen, and exquisitely painful joints, a furred tongue, loaded urine, and a full and bounding pulse, I usually add from two to three drachms of the potassio tartrate of soda, ten or fifteen minims of the vinum colchici, from fifteen to twenty minims of the vinum antimonii, and from ten to fifteen minims of the tinctura opii, or of Battley's sedative solution, to prevent the salt running off by the bowels. This draught is repeated, for the first 12 or 24 hours, at intervals of three or four hours, according to the strength of the patient, and the severity of the attack; and if the pain is excessive, I prescribe a pill containing from half a grain to a grain or a grain and a half opium, or an equivalent dose of Dover's powder to be taken once or twice a day, taking care to increase or diminish the quantity of the sedative, according to the circumstances of the case; on the one hand, avoiding constipation and narcotism, and on the other, guarding against diarrhoea.

Sometimes, though rarely, the stomach does not easily tolerate these large doses of the neutral salts, and in such cases, the greatest benefit is derived from the addition of a little lemon juice and an alkaline car-

bonate, forming a saline effervescing draught. With this variation, an instance rarely occurs, in which the medicine deranges the stomach, or produces the slightest disagreeable effects.

When once the medicine has begun to take effect, which is evidenced by the gradual decrease of the pain, the tranquillization of the pulse, and the increase in the quantity and specific gravity of the urine; it is repeated every fourth hour only, and then every fifth or sixth hour; and usually at the expiration of two or three days, I find its work in great measure accomplished: the saliva, by that time, has lost its acidity, the pains and inflammation have subsided; the pulse has fallen, probably, from 120 to 85 or 90 beats in a minute; the tongue has become moister and less red and furred; the urine more abundant, less loaded with the lithates, and of a higher specific gravity; and the perspiration less acid, less sour smelling, and less profuse. In proportion as these symptoms of amendment manifest themselves, so is the dose of the alkalies decreased, until after the lapse of three or four days, I usually feel justified in commencing the administration of quina during the day, taking care to maintain a free action of the bowels by exhibiting, now and then at bedtime, two or three grains of the acetous extract of colchicum, together with aloe or rhubarb, and if necessary, a grain of calomel or blue pill. Should there be the slightest return of pain, the least increase of coating on the tongue, or indeed, any evidence of returning fever, the use of quina is at once abandoned, and alkalies are again resorted to. But generally the case proceeds steadily to convalescence, and after a few days the pills are either omitted altogether, or repeated less frequently and in diminished quantities. Sometimes, if there be not much tendency to perspiration, the *mistura guaiaci* of the *Pharmacopœia* may be administered with the addition of a drachm of the volatile tincture of guaiacum, and 20 or 30 minims of liquor potassæ, whilst, if the patient appears to be cachectic, the decoction of cinchona, with the addition of the ammoniated tincture of guaiacum, or of half a drachm of the extract of sarsaparilla with two or three grains of iodide of potassium, and twenty minims of liquor potassæ, proves a more active and efficient agent.

But I do not confine myself to internal remedies. I endeavour to alleviate articular inflammation by means of external topical applications.

By many persons this practice is regarded as useless, and by some has been condemned, on the ground that, by interfering with the inflammatory action taking place at an affected rheumatic joint, we incur the risk of disturbing the attraction of the morbid element, and of driving it to some internal viscus. Doubtless, if cold topical applications were to be employed, and the elimination of the poison thus forcibly arrested, a very hazardous experiment would be tried, and serious internal mischief would probably arise. But against leeches, blisters, and warm fomentations, this objection cannot be reasonably urged: they do *not* check the continuance of perspiration; they favour rather than arrest the exit of the morbid matter, and the only fact, therefore, to be ascertained in regard to them, is their remedial or curative power.

It has been already stated, that rheumatic inflammation very rarely leads to disorganization of the joints, and moreover, displays a remarka-

ble disposition to shift or migrate from one spot to another. Hence, leeches and blisters are rarely called for in the acute diseases: they are generally unnecessary for the relief of the inflammation, and are useless in most cases, because of its repeated migration. They might, and indeed they do, afford very marked relief to the severity of the inflammation, but they afford no guarantee against its recurrence. Their use, therefore, should be restricted to those cases in which inflammation lingers about a particular joint, or in which the intensity of the action is so great as to threaten the integrity of the structures. But warm fomentations of every description may be always employed advantageously. They soothe the parts, promote perspiration, and thereby favour the elimination of the poison. As the object of the applications is to allay the pain, and to counteract the extreme acidity which always accompanies, if it be not the cause of rheumatic inflammation, it is manifest, theoretically at least, that an alkaline and opiate solution should prove the most effective remedy. And so in practice it is found to be. I have tried hot water; I have tried a warm solution of nitrate of potash, as recommended by Dr. Basham; I have tried a simple alkaline solution; and I have tried a mixed alkaline and opiate solution, and the latter has proved far the most powerful in allaying the pain of rheumatic inflammation. In every instance in which it has been employed, the relief obtained has been almost immediate, and the pain and inflammation have subsided rapidly.

That the effect observed to follow these saline fomentations has not been due to accidental causes, is manifest from the result of careful experiment; for in order to guard against any source of fallacy, I selected fourteen instances in which corresponding joints were affected, and applied a fomentation of warm water to the one joint, and an alkaline and opiate solution to the other, and almost uniformly the pain and inflammation continued in the former, and speedily subsided in the latter. Beside this, I have endeavoured to ascertain whether the occurrence of inflammation may not be prevented by impregnating the parts with alkaline matter. In nine cases in which the knees, and in seven cases in which the hands were in the first instance unaffected, a mixed alkaline and opiate solution was applied to one joint, and a simple water fomentation to the corresponding joint on the other side. In four of the former cases, and in two of the latter, the joint to which water only had been applied became inflamed, whilst in one instance only did inflammation occur, and that to a very slight extent, in the joint which has been bathed in an alkaline and opiate solution. So that my experiments, though not as yet sufficiently extensive to enable me to speak decisively on the subject, have, as far as they have gone, proved extremely satisfactory.

One point yet remains to be noticed. Whilst attacking the disease both by internal and external means, it is essential to pay strict attention to the diet. The patient must be kept low; yet, as there is an excessive drain upon the system, it is expedient to allow him more nourishment than would be safe or proper in other inflammatory complaints. Strong beef tea and jelly may be given in moderate quantity; and with the view of supplying the waste caused by the perspiration, and of pro-

moting the dilution and more rapid absorption of the alkaline medicines administered internally, diluents, such as whey, thin gruel, or barley water, should be taken from time to time."

We also take much pleasure in inserting a case related by the author, which furnishes us with his precise prescriptions :

The following case will serve to illustrate my plan of treatment :

William Made, a thin, pale, unhealthy looking boy, æt. 15, was received as a patient under my care, at St. George's Hospital, on the 24th of August, 1850. He had got wet through on the morning of the 21st, and in the evening of the same day had been attacked with shivering, succeeded by wandering pains in the limbs. In the course of a few hours the pain seized upon the joints, and the right hand and wrist, the left knee and both ankles became inflamed and swollen. The inflammation, however, had shifted rapidly from joint to joint ; and when I first saw him, his left hand and wrist, his right knee, his right elbow, and his left ankle, were the inflamed parts. He had been unable to sleep since the beginning of the attack, his complexion was sallow, his countenance distressed, and he was sweating profusely. Tongue white and furred. Bowels costive. Urine scanty, high coloured, and loaded with the lithates ; sp. gr. 1020. Pulse 126, full and bounding, but not strong. Heart's action regular, the sounds distinct and free from murmur. Saliva strongly acid. He had no hereditary tendency to rheumatism (?), and the present was his first attack.

The inflamed joints were at once wrapped up in flannels, soaked in a lotion formed by dissolving an ounce of the carbonate of potash in a pint of decoction of poppies ; his diet was limited to beef tea, gruel, and arrow-root ; and the following medicines were prescribed :

Rx. Hydrargyri Chloridi ;
Pulveris Doveri, ãã gr. iij. M. ft. Pulvis statim sumendus.

Hauftus Sennæ c. Sodæ Potassio-Tart., 3 iij, post horas quinque.

Hauftus Salini, 3 jss ;

Sodæ Potassio-Tartratis, 3 iij ;

Vini Colchici, mxv ;

Tincturæ Opii, mvij. M. ft. Haust. 6^{ta} quâque horâ sumendus.

August 25th.—Passed a tranquil night, though without much sleep, and to-day is comparatively easy. The redness has almost entirely disappeared, and the swelling has subsided considerably. Bowels acted on twice by the medicine yesterday, and to-day have been freely moved once. Perspiration still very profuse and acid. Urine about 1½ pints, besides what was passed at stool ; acid ; less high coloured and loaded ; sp. gr. 1031. Tongue moist and much cleaner. Pulse 98, soft. Heart's sounds clear. Saliva still acid.

Rep. Haustus 6^{ta} quâque horâ.

Rx. Pulv. Ipecac. comp., gr. x ;

Hydrarg. Chloridi, gr. iij. M. ft. Pulvis hæc nocte sumendus.

26th.—Slept well last night; to-day has neither redness nor swelling of the joints, and slight stiffness only remains. Still perspiring freely. Bowels have acted once. Tongue moist and cleaning rapidly. Urine reported about $2\frac{1}{2}$ pints, clear and acid; sp. gr. 1030. Pulse 86, soft. No exocardial or endocardial murmur. Saliva neutral.

Rep. Haustus ter in die tantum.

27th.—Passed another quiet night, and has no return of pain. Bowels have acted once. Tongue quite clean, except just in the centre, where it is rather white. Urine clear, and in great abundance; sp. gr. 1030. Pulse 76, soft. Is up and about to-day.

R̄. Haustus Quinæ c. Acid Sulph. dil., *mxv*. Sulph. Magnesiæ, 3ss, bis in die.

From this time he continued taking the Quina until the 30th, when having had no return of pain, and feeling quite well, the medicine was discontinued."

Other cases are detailed, but for these we have no space.

Afterwards, in speaking of rheumatic affections of the heart, and the essential nature of the disease, our author says :

"If, then, it be admitted that fibrin may be, and is, under certain circumstances, deposited from the blood without the coexistence of endocarditis, it is obvious, that if we would preserve the heart from mischief, it is necessary not only to take precautions against inflammation, but to maintain, at all hazards, the solubility of the fibrin. For both these purposes, alkalies and the neutral salts, if freely administered, prove most efficient agents. By counteracting or destroying the irritant property of the materies morbi, and by promoting its elimination, they tend to prevent articular and other local inflammations; they thus prevent any further augmentation in the quantity of fibrin; and by increasing as they do the solubility of that material, they prove excellent preservatives against its deposition on the valves."

And further on, respecting the fibrinous deposition on the valves, he adds that it is due to the presence of an unusual quantity of fibrin in the blood, and in the weak state of solution in which it is held, probably, in consequence of the extreme acidity of the system. It is essentially independent, he thinks, of valvular inflammation, and may or may not occur coincidentally with it, though the existence of valvular inflammation would add to the chances of the occurrence forming a nucleus for deposit. We cannot dwell upon this subject, nor upon the chapter devoted to the statistics of heart disease, in connection with rheumatism, nor of inflammation of the brain independent upon the rheumatic diathesis, but must demand a few more lines only for the consideration of rheumatic gout. Besides the treatment which was serviceable in acute rheumatism, he recommends the warm bath rendered alkaline by the addition of two or three ounces of the carbonate of soda. The value

of this auxiliary in combating the disease, he says, has already been pointed out by Dr. Wright, of Birmingham, and he himself had seen many cases which had long resisted ordinary treatment, yield readily as soon as the alkaline bathing had been commenced. Dr. Fuller also advises blisters to the swollen limbs or joints, a plan we have seen M. Val-leix adopt at La Pitie, in Paris. Alkaline sulphuretted baths to the swollen limb are very highly recommended, and more particularly, the vapour baths. The subjoined formula by Plance and Boullay, is given as an excellent one for an artificial sulphuretted bath, viz :

- ℞. Sulphuret of soda or potash 3 iii.
 Carb. of soda 3 ii.
 Muriate of soda 3 ii.
 Sulphurate of sodale 3 i.
 Distilled water Oi.

Mix, and then add the mixture to 20 gallons of water, at the temperature of 98 deg., Fahrenheit.

We see no mention of it in the work before us, but we should refer to the plan pursued by M. Bonnet, Briquet and others, in France, of injecting even into the synovial cavity of inflamed joints, the tincture of iodine, by making a puncture beneath the skin, and reaching the joint. We have seen both of these gentlemen, at La Charite, employ this method as well in this affection, but by the latter also in inflammation and hydatid enlargement of the ovaries, without dangerous consequences, and with relief or perfect cure.

Our readers might very well consult the tables in this work, when wishing to advise their patients as to the value and advantages of certain baths and watering places in Europe.

We regret not being able, for the benefit of the reader, to accompany our author in the statement of his views on the subject of neuralgic rheumatism. We will only say that he advises the bathing of the painful parts with the tincture of aconitine and veratria, by means of a camel's hair-brush, or with ointment containing these respective alkaloids. Here, in conclusion, is a formula for the alcoholic solution :

- ℞. Aconitine, grs. vij, or veratria, grs. xx ; spir. rectificati, 3 i ; misce.

We conclude with the general commendation of the work to the medical profession, as one which they can consult with much advantage. It is full of instruction, and contains numerous prescriptions which the author's experience has shown him to be serviceable.

BIBLIOGRAPHICAL NOTICES.

ART. XI.—*Fungi Caroliniani Exsiccati*. Fungi of Carolina, illustrated by Natural Specimens of the Species. By H. W. RAVENEL, Cor. Memb. of Acad. Nat. Sciences of Phila.; Memb. of Am. Association for the Advancement of Science. Fasc. II. Charleston: John Russell, 256 King-st. 1853. (From the author.)

This Fasciculus Second of a work on our native plants, by a native Carolinian and a Botanist of the first distinction, we hail with the most unfeigned pleasure. The satisfaction which we experience upon its reception is added to when we are assured that it will be welcomed as well abroad as at home. In Florence, Italy, we were shown by a friend a well-known scientific periodical in the German language, which contained a highly flattering notice of the leading fasciculus, and we have had the pleasure of seeing a MS. letter from the veteran cryptogamist Fries which contained the approval of this distinguished authority. Our own estimate and appreciation of the work, however humble our ability to decide, results from the practical assistance which it has given us on more than one occasion; having assigned us, by the American Medical Association, some duties connected with the medicinal and toxicological properties of the plants of this class which belong to the United States, it is to the author, as well as to his work, that we have been indebted for the localities of those found in this State as well as for the determination of the American origin of many which were before either supposed to be European, or of whose presence here, at least, botanists were ignorant. The work is of incalculable value to the investigator in this department, for by it he is enabled to ascertain the precise locality of species growing within our reach.

The "*Fungi Caroliniani*" will also never cease to be an authority, for the author, possessing ability and cultivation of a high order, sparing no pains nor labour himself, has, at the same time, subjected most, if not all, the species which are contained in it to the best living authorities. Many of them have passed under the eye of Mr. Berkley, of England, who was associated with Sir Wm. Jackson Hooker, in the preparation of the "*Cryptogamia of England*." The Rev. Mr. Curtis, of Society Hill, S. C., whose qualifications are well-known and whose experience is extensive, has his name united with the author's in the determination of many of the most interesting specimens presented to us. To

On account of the difficulty and labour involved in their multiplication, not more than fifty copies are published. The author may not anticipate pecuniary recompense, and we are assured that his wishes, with respect to their diffusion, is of the most enlarged and scientific character; yet we do think that his offering for sale, to libraries and men of science, such volumes, the result of so much toil and where their production demands such requirements, at the low price of four dollars, is a positive injury, not simply to himself, but to the cause of science generally. In a few years the *Fungi Caroliniani Exsiccati* will be rarer and more difficult to obtain than is a copy of Elliott's "Sketches" at present, and their possession will be considered quite an heir-loom. We repeat the suggestion offered in a previous number of the Journal, that Mr. Ravenel's services should be secured for some educational institution, where they would be of inestimable value.

ART. XII.—*Woman, her Diseases and Remedies*: A series of letters to his class. By CHARLES D. MEIGS, M.D., Prof. of Midwifery and the Diseases of Women and Children in the Jefferson Medical College at Phila.; Memb. of the Am. Med. Assoc. of the Am. Philosoph. Soc., etc., etc. Third Edition, revised and enlarged. Philadelphia: Blanchard & Lea. 1854. (From the publishers, through Mr. John Russell, King-st.)

This, the third edition of this work, which has just been handed to us, has scarcely been received in time to do more than acknowledge its presence. It has already been alluded to more in extenso when the preceding editions made their appearance.

We can only say at present that we regard it as presenting a greater combination of quaintness, singularity, learning, ability and interest than is contained in any other work that has recently appeared in this country. Its quaintness is oftentimes exaggerated, its singularity verges on the grotesque, its learning is tinged with pedantry, but the ability which characterizes it, the vein of research which runs through it and the amount of information it contains, if they do not reconcile us to its literary vices, at any rate renders them less objectionable. Even the pedantry of the author is pleasant. We feel that he has an earnest love for science, for knowledge and for his profession, and we are inclined to pardon the demerits accompanied by many excellencies of such commendable origin. Prof. Meigs' work contains, we repeat, a very great deal of valuable instruction, and though we condemn the precedent and would not covet the style, nor can we admire the method by which he teaches, yet we have always pointed to the volume as one which will command perusal. Its epistolary form is novel and pleasing, and, at the same time that this permits more license, the reader, having his curiosity excited and his fancy stimulated by all the peculiarities cited above, at the same time that his desire for solid aliment is gratified, is led on involuntarily to its completion.

Some future medical D'Israeli may consider "*Woman and her Dis-*

eases" as worthy of a place among the Curiosities of Literature, but the pleasantries of its exaggerations, as well as the abundant and most suggestive materials for thought, which the author's large experience has enabled him to introduce will always make it a readable and truly useful book.

We really do not see, too, why medical works should so frequently be dry, unentertaining and repulsive, unrecommended by any spirit, any enthusiasm or charm of description, but maintaining throughout an unrebukable level of moderate ability, which must be preserved as a duty incumbent upon their authors, with not a salient point that even a critic can complain at—like "Telemachus," which every body professes to admire, but which none ever finish. Our author, in following the Roman maxim which teaches that he best succeeds who learns to mingle the useful with the agreeable, may be breaking through the shackles of the past, and may be ushering in a new method of teaching.

ART. XIII.—*Pneumonia*: its supposed connection, Etiological and Pathological, with Autumnal Fevers; including an inquiry into the existence and morbid agency of Malaria. By R. LA ROCHE, M.D., &c., &c. Philadelphia: Blanchard & Lea. 1854. (Charleston, John Russell.)

This work—it is unnecessary to tell our readers—is the collection and embodiment of those admirable papers which appeared in this Journal during the past year; consequently we shall not enter into an analytical examination of its contents. We shall merely say, that for the severity of its logic, the perspicuity, the purity of its style, the great erudition it displays, the arrangement of the topics discussed in it, &c., this work stands in the front rank of the medical literature of the country. Dr. La Roche is a very accurate observer, a deep thinker, and a close and sound reasoner. These qualities, which are essential to give weight to opinions, are happily united in Dr. La Roche. To natural endowments of a high order, he has, in the composition of this work, brought the fruit of experience and mature reflection. He has knocked from under those who believe in the malarial origin of pneumonia, the foot-hold which they formerly had; he has so signally exposed the fallacy of this view of the causation of pneumonia that we trust it will not, in the future, be received. Had our Western brethren been content with admitting a blending of causes and, consequently, of effects in two diseases, no one would have gainsaid their statement; but when the startling assertion is made that pneumonia is nothing more than a form of periodic fever, its untenable nature should be exhibited in the strongest light.

ABSTRACTS

FROM FOREIGN AND DOMESTIC JOURNALS.

ANATOMY AND PHYSIOLOGY.

On the Excito-Motory System of Marshall Hall. By Dr. DAVEY. The author of this paper endeavours to show that the excito-motory phenomena, as explained by Dr. Marshall Hall, rests on a basis which even *he*, up to this time, has failed to recognise. His object is to demonstrate that the true spinal functions stand in no need of the distinct set of nervous fibres—the exodic and eisodic nerves—which Dr. Hall describes and Mr. Grainger endeavoured to demonstrate with the scalpel; but that the sensory and motor spinal nerves are of themselves sufficient to create all the phenomena with which Dr. Hall has made the profession so fully acquainted.

Dr. Davey contended that the excito-motory system can be roused into action when the spinal marrow has been separated from the brain, or even in the case of an acephalous monster born without a brain; but even more; that precisely the same phenomena have occurred in monsters born without either a brain or a *spinal marrow*; and quotes Sir Gilbert Blane to support him in this statement. He declares that the reflex actions properly so called, are nothing more than an amplification of the ordinary spinal functions; an additional exciting cause either from within or without.

An effort is then made to prove that the *organic system*, with the solar ganglion for its centre, is the great mainspring of all the phenomena of life; whether they relate to the organic or animal functions; whether to the brain, spinal marrow or any other individual part of the whole organism. He declares that the ganglia of the sympathetic system are in existence in the embryo anterior to the cerebro spinal organism, and that the medulla spinalis, as well as the brain; the heart and other internal viscera; all exercise their several functions in virtue of the power derived from the ganglionic system. He denies the correctness of Dr. Hall's experiments and states that the removal or destruction of the sympathetic system in the frog has always been found fatal to the true spinal phenomena. The motions so obviously perceptible are nothing more than retained nervous influence, and continue no longer than the peculiar contractile irritability remains. The following passage is quoted from Dr. Hall's book on the "Nervous System" to strengthen his position: "I must add that as the irritability of the internal organs depends much on the ganglionic system, so that of the muscles of the limbs may partly depend on the latter system. Although I have

observed this irritability to be greatly diminished by the removal of the influence of the spinal marrow, I think," he adds, "I never knew it to be completely annihilated under such circumstances."

Again, Dr. Davey says, the function of respiration is under the control of the spinal system, and when that ceases, it is an evidence that the spinal cord has lost its functions. How, then, can we account for the fact that the uterus has been able to expel a child after respiration had entirely ceased, which he declares to have occurred, unless upon the supposition that the ganglionic system still continued to give vitality and muscular power to the organ?

He therefore believes—

"1. That the more complicated anatomy of the spinal cord, suggested by Dr. M. Hall, and sought for by Mr. Grainger, has not yet been realised.

"2. That the mere sensory and motor nerves or roots are themselves sufficient to the integrity of all those phenomena called excito-motory, which emanate from the spinal cord; or, in other words, that there is no necessity for any distinct division of the nervous (spinal) system, for the production of the excito-motory phenomena.

"3. That the exercise of the functions of the spinal cord is due to the peculiar influence of the ganglionic nervous system, exerted on its organism.

"4. That both the external and internal excito-motory phenomena are alike dependent on the vital stimulus imparted by the great sympathetic nerve, and, in the former instance, through the instrumentality or medium of the medulla spinalis.

"5. That such is the relationship between the great nervous centres, viz: the brain, spinal chord, and the solar ganglion, that the second, (viz: the spinal cord,) being at one time under the dominion of the first, *i. e.* the brain, and at another time under that of the third, *i. e.* the solar ganglion, and being thus intermediate, will now manifest a cerebral or voluntary power (and this the result of consciousness, and, of course, volition,) and then (the antecedent circumstances being dissimilar) display one of an involuntary, automatic, or instinctive, or excito-motory character; but this only, of course, when the organic nervous apparatus is in the ascendant, and the brain, comparatively quiescent—or, in other words, when sensation (spinal) and motion, and not consciousness and volition (cerebral attributes) are in operation, if I may be allowed so to express myself.

"6. That the great sympathetic nerve is endowed with a specific and independent power, to which both the brain and spinal cord, and with them, of course, their functions are subordinate."

Researches on the Sympathetic Nerve. By M. CLAUDE BERNARD. (Virg. Med. and Surg. Jour. from Gaz. Médicale.) When the great sympathetic nerve is divided in the neck, or its ganglia excised, the results which follow are, retraction of the eye-ball, contraction of the pupil, redness of the conjunctiva, and the projection of the cartilage of the third eye-lid. These phenomena were observed first by Pourfour du Petit (1727), and afterwards by Dupuy, Brachet, and John Reid.

In 1846 M. Biffie noticed that by galvanising the cephalic end of the divided nerve, the pupil which was contracted in consequence of the section of the nerve, would become again dilated.

The experiments of MM. Budge and Waller proved that the influence transmitted by the sympathetic was derived from that portion of the spinal cord situate between the last cervical and sixth dorsal vertebra, which they named the cilio-spinal region.

M. Bernard announced to the Academy of Sciences in March, 1852, his important discovery of the great increase of animal heat and of sensibility on that side of the head on which the sympathetic was divided; and also ascertained that upon the application of galvanism to the upper end of the nerve this animal heat would disappear. Budge referred this increase of heat to the cilio-spinal region. Waller and Brown Séquard explained the increased heat and vascularity by the paralysis of the arterial system caused by section of the nerve; Brown Séquard claiming to have been the first to show that galvanism of the sympathetic diminished the temperature and produced contraction of the arteries.

M. Bernard then proceeded to prove by a series of experiments,

1st. That a division of the sensory nerves in addition to the abolition of sensation also diminished the temperature of the part.

2nd. The division of the motor nerves destroyed the power of motion and also diminished the animal heat of the parts.

3rd. The destruction of the sympathetic producing neither a loss of motion or sensation, greatly increased the temperature.

This augmentation of the animal heat is, therefore, a property peculiar to the sympathetic nerve. The elevation of temperature begins instantaneously with the section of the nerve, and amounts to four or five degrees centigrade. The excess of calorification upon a section of the nerve lasts in rabbits from sixteen to eighteen days, and in dogs about two months; but after extirpation of the ganglia, its effects have remained very intense a year and a half after the removal of the ganglia.

Upon the application of galvanism to the divided nerve, reverse phenomena are produced. The pupil dilates, the vascularization of the parts disappear, and their temperature falls.

"I have only wished," says the author, "in this communication, to establish one point in the complex history of the great sympathetic, namely, that the section of the filaments or ganglions of that nerve possess constantly the privilege of increasing the calorification of the parts to which it is distributed. These phenomena are only the exaggeration of what naturally takes place in the production of animal heat. In giving the means of increasing the production of heat and localizing it in external parts which are easily observed, I had thought of rendering more accessible to our means of research the study of that important function which is still so little known, but which cannot be investigated elsewhere than in the greater or less activity of the chemical metamorphoses which the blood undergoes in the living textures under special influences of the nervous system."

On the Venom of Serpents. By J. GILMAN, M.D. (St. Louis Med. and Surg. Jour.)—There is much in the history and habits of the reptile tribes, however repulsive they may be in appearance, that is very interesting. During a sojourn of two or three months in the interior of Arkansas, which appears to me to be the paradise of reptiles, I paid some attention to that branch of natural history called ophiology. I found four distinct varieties of rattlesnakes, (*Crotalus*) of which the *Crotalus Horridus* and *Crotalus Kirtlandii* are by far the most numerous. The former is the largest serpent in North America. The family of moccasin snakes (*Coluber*) is also quite numerous, there being not less than ten varieties; most of which are quite as venomous as the rattlesnake. By dissecting great numbers of different species, I learned that the anatomical structure of the poisoning apparatus is similar in all the different varieties of venomous serpents. It consists of a strong frame-work of bone, with its appropriate muscles in the upper part of the head, resembling and being in fact a pair of jaws, but externally to the jaws proper, and much stronger. To these is attached by a gingivoid articulation, one or more moveable fangs on each side, just at the verge of the mouth, capable of being erected at pleasure. These fangs are very hard and sharp and crooked, like the claws of a cat, and hooked backwards, with a hollow from the base to near the point. I have occasionally seen a thin slit of bone divide this hollow, making two. At the base is found a small sac, containing two or three drops of venom which resembles thin honey. The sac is so connected with the cavity of the fang during its erection, that a slight upward pressure forces the venom into the fang at its base, and it makes its exit at a small slit or opening near the point, with considerable force; thus it is carried to the bottom of any wound made by the fang. Unless the fangs are erected for battle, they lie concealed in the upper part of the mouth, sunk between the external and internal jaw bones, somewhat like a pen-knife blade shut up in its handle, where they are covered by a fold of membrane, which encloses them like a sheath; this is the *vagina dentis*. There can be no doubt but these fangs are frequently broken off or shed, as the head grows broader, to make room for new ones near the verge of the mouth; for, within the *vagina dentis* of a very large *crotalus horridus*, I found no less than five fangs on each side—in all stages of formation—the smallest in a half pulpy or cartilaginous state, the next something harder, the third still more perfect, and so on to the main, well-set, perfect fang. Each of these teeth had a well defined cavity like the main one. These fangs on each side were frequently found in copper-heads, vipers and others.

The process of robbing serpents of their venom is easily accomplished by the aid of chloroform, a few drops of which stupifies them.

ner, they were found, after two days, to be as highly charged as ever with venom of equal intensity with that first taken.

During the process of robbing several species of serpents, I inoculated several small, but vigorous and perfectly healthy vegetables, with the point of a lancet well charged with venom. The next day they were withered and dead, looking as though they had been scathed with lightning. In attempting to preserve a few drops of venom, for future experiments, in a small phial with two or three parts of alcohol, it was found in a short time to have lost its venomous properties. But after mixing the venom with aqua ammonia, or spirits turpentine, or oil of peppermint, or of cinnamon, or of cloves, or with nitric or sulphuric acid, it still seemed to act with undiminished energy. It is best preserved, however, for future use by trituration with refined sugar or sugar of milk.

A very fine large cotton-mouth snake, being captured by putting a shoe-string around him, became excessively ferocious, striking at even the crack of a small riding whip. Finding himself a prisoner, without hope of escape, he turned his deadly weapons on his own body, striking repeatedly his well-charged fangs deeply into his flesh. Notwithstanding this, he was put in a small basket and carried forward. In one hour after, he was found dead, and no amount of irritation could excite the least indication of life. Four hours after, while removing the skin for preservation, the blood oozed slowly from the vessels in a dissolved state. No violence was done to his snakeship, except what he did to himself.

Another moccasin, shot by a pistol about two inches back of the head, and skinned immediately, gave decided evidence of vitality four hours after being flayed, by writhing the body whenever it was irritated by a scalpel.

A large rattle-snake beheaded instantly, with a hoe, would, an hour and a half after, strike at any thing that pinched its tail. Of several persons who were testing their firmness of nerve, by trying to hold the hand steady while the serpent struck at it, not one could be found whose hand would not recoil in spite of his resolution, and one man, a great bully, by-the-bye, was struck on the naked throat with considerable violence by the headless trunk of the serpent, and staggered back, fainted and fell, from terror. Mr. Stewart, of Miss., tells me he witnessed a similar scene once. An old hunter shot a rattlesnake's head off, and after re-loading his gun and standing sometime, he stooped to pull off the rattles, and the bloody, but headless trunk of the snake, struck him in the temple, and he fainted and fell down with terror.

Seven venomous serpents, belonging to five different species, were made to fraternize and dwell amicably in one den. A beautiful pair of long bodied speckled snakes, known as king-snakes, and found to be fangless, and consequently without venom, were duly installed as members of the family. Some uneasiness was perceivable among the older members, but no attempt was made to destroy the intruders, though they might have been killed instantan. The next morning four of the venomous serpents were found to have been destroyed by the king-snakes, and one was still within their coil, and the two remaining ones

would make no effort at self-defence. A large rattlesnake seemed stupid and indifferent to his fate. He could not be made to threaten or give warning even with his rattles. The smallest king-snake was afterwards inoculated with the poison of one of the serpents he had destroyed, and died immediately after—thus evincing that they must have exercised some power besides physical force to overcome their fellow-creatures.

In short, the results of a great number of experiments performed with the venom of a great variety of serpents, seem to lead to the following conclusions :

1st. That the venom of all serpents acts as a poison in a similar manner.

2d. That the venom of some varieties is far more active than that of others.

3d. That a variety of the coluber, known as the cotton-mouth, is the most venomous serpent in Arkansas.

4th. That the venom of serpents destroys all forms of organized life, vegetable as well as animal.

5th. That alcohol, if brought in contact with the venom is, to a certain extent, an antidote.

6th. That serpents do possess the power of fascinating small animals, and that this power is identical with mesmerism.

7th. That the blood of small animals, destroyed by the venom of serpents, bears a close resemblance to that of animals destroyed by lightning or hydrocyanic acid ; it loses its power of coagulation and cannot be long kept from putrefaction.

PATHOLOGY AND PRACTICAL MEDICINE.

Prognosis and Treatment of Epilepsy. (Association Medical Journal, American Journal of Medical Sciences.) The Union Médicale for May 17th and 19th contains an article by Dr. Herpin, of Geneva, on the above subject, of which we now give an abstract.

In the Union Médicale for December 1, 2, and 7, 1852, M. Moreau, of Tours, relates nine cases of epilepsy, in which oxide of zinc had failed to arrest the disease, a remedy stated by Dr. Herpin to be most amenable to treatment, and the medicine was administered according to the rules laid down by him in his essay, *Du Prognostic et du Traitement curatif de l'Epilepsie*, published last year at Paris. Dr. Herpin points out the causes of M. Moreau's want of success, in the following manner :

1. The first remarkable point which may account in a great measure for the different results obtained by M. Moreau and Dr. Herpin was,

Tissot, Odier, De la Rive, and C. Vieusseux, all believe in the curability of a fair proportion of epileptic cases. A principal cause of the difference between the opinions of the two classes of practitioners is, that those in private practice generally see the disease from its commencement, while hospital physicians almost always have to treat severe or obstinate cases.

2. M. Moreau had only male patients ; Dr. Herpin had more females than males. From an analysis of his cases, Dr. Herpin arrives at the following results :

Of twenty-six female epileptic patients, sixteen were cured, six were improved, and four were incurable.

Of twenty-four male epileptic patients, twelve were cured, four were improved, and eight were incurable.

There were thus twice as many incurable cases among males as among females.

3. With regard to age, Dr. Herpin has obtained the following results :

Of thirty-five patients under 20 years, eighteen were cured, nine improved, and eight were incurable.

Of nine patients aged from 20 to 50, five were cured, one was improved, and three were incurable.

Of six patients aged from 50 to 80, five were cured, and one was incurable.

The period of life from 30 to 50 furnishes a third of incurable cases ; while the other two do not together supply a fourth. All M. Moreau's cases were from 19 to 50 years of age, the most unfavourable period.

4. With regard to the previous duration of the disease, Dr. Herpin finds that—

Of twenty-three cases, which had existed less than a year, fifteen were cured, five were improved, and three were incurable.

Of twenty-seven cases of from one to twenty years' duration, thirteen were cured, five were improved, and nine were incurable.

While nearly one-half of Dr. Herpin's cases were of less than a year's duration, three of M. Moreau's patients had been ill from fourteen to twenty months, one for about two years at least, three for six years, and one for about twenty years ; the ninth had recent attacks of vertigo, but had probably had an epileptic attack six months before.

5. With regard to the number of attacks previous to treatment :

Thirty epileptic patients, who had had less than twelve attacks, furnished only three incurable cases.

Twenty-two patients who had had at least from thirty to a hundred attacks, furnished twelve completely obstinate cases, being at least five times as many as in the preceding category.

Of M. Moreau's nine cases, one, who was seized with vertigo, had perhaps had a fit ; one patient had had only four attacks ; one had had about fifty ; four from seventy to eighty ; one more than a hundred ; and one more than five hundred. Besides this, one of the patients had, before the commencement of treatment, paralysis, denoting organic lesion of the brain, which was proved by the autopsy ; and another had been twice insane. This latter circumstance was met with in one

of Dr. Herpin's cases, in whom, though the conditions for treatment were otherwise favourable, the disease remained incurable.

Besides these causes of failure in M. Moreau's cases, Dr. Herpin points out that the want of sufficient judgment in the choice of treatment is, perhaps, a more powerful obstacle. He observes that as long as we are unacquainted with the indications of each remedy for epilepsy, we must begin by giving that which experience has shown to have succeeded in the greatest number of cases; then, in case of failure, we must have recourse in succession to other remedies of efficacy. By employing only one, especially in a number of patients placed in the same conditions as to age, sex, etc., we render ourselves liable to fall on the medicine which is not indicated. This is precisely what, it seems, has accidentally happened to M. Moreau.

Oxide of zinc is believed by Dr. Herpin to fail generally in epileptic patients in the vigour of their age, especially in men. Taking the whole of the cases placed in favourable conditions as regarded the number of previous attacks, and which were treated by oxide of zinc, he finds that there were twenty-six cures and five failures—all the latter being in patients between the ages of seventeen and fifty-nine years. On examining into the results of the treatment by oxide of zinc in men of between 20 and 50 years, in order that the conditions of sex and age might be the same as in M. Moreau's patients, Dr. Harpin finds six patients who were almost all in the most favourable conditions for treatment. In one, venesection appeared to have more influence than the zinc in producing improvement. Of the remaining five cases, there were one cure without relapse in a patient who had had only three attacks; two cures followed by relapse—in one of these the oxide of zinc failed on the subsequent trial; one in whom improvement was produced at the age of 15, but in whom the same remedy failed ten years later; and lastly, one in whom the disease altogether resisted treatment, although it had been commenced five days after the first attack. Thus, while the total number of favourable cases treated by zinc are in the proportion of five to six, adults furnish only three cases out of five, and in only one of these was the cure permanent.

In adult age, it is necessary to give zinc in large doses and for a considerable time; in childhood and old age, the same result is obtained from smaller doses, and, in some cases, from almost insignificant quantities.

The preceding observations appear to Dr. Herpin to afford sufficient reason for arriving at the following conclusions:

1. Oxide of zinc seems to be indicated as an anti-epileptic in children and old persons.
2. It often fails in persons of middle age, especially in men.
3. If it be employed in females, it must be given in large doses and for a long time.

Whatever, Dr. Herpin observes, may be the remedies employed, it is of the highest importance that the disease be treated at as early a period as possible. He is convinced that, by perseveringly treating epilepsy from its earliest manifestation, there is a certainty of cure in a large majority of cases. At present, some mistake the first symptoms of the

disease; others treat it for a time by means almost always inefficacious, such as bleeding, anthelmintics, etc.; others, again, try useful remedies, but timidly, and without effect. A small number, chiefly hospital physicians, form a tolerably accurate notion of the choice of medicine and of the results obtained; but they are placed in the worst conditions for acting at the most favourable moment.

Dr. Herpin promises, at a future period, to publish in the *Union Médicale* the details of some cases in the private practice of himself and others, giving both the successful and the unsuccessful cases in the proportions in which they have been met with.

Gonorrhœa. (British and Foreign Med. Chir. Review.) Dr. BOINET (L'Un. Méd., Sept.) speaks highly of the effect of tincture of iodine when applied to the mucous membrane of the vagina in the gonorrhœa of women; a *single* application was sometimes sufficient. At the same time a solution of equal parts of tincture of iodine and water was injected into the urethra, but was not allowed to penetrate into the bladder. Dr. Boinet has employed the local application of iodine in inflammations and ulcerations of other mucous membranes, and with great success.

Report upon the cases of Tetanus in the Jamjetjee Jejeebhoy Hospital, Bombay, from January 1845, to December, 1851. By J. PEET, Assistant Surgeon to the Hospital. (Ranking's Abstract from Trans. of Med. and Phys. Soc. of Bombay, No. 1, New Series, Bombay, 1853.) Mr. Peet informs us, that during his seven years, one hundred and ninety-five cases of tetanus were admitted into the hospital, exclusive of some seventy-nine or eighty persons who were attacked subsequently to their admission, making the whole number treated during this period about two hundred and sixty. This is the largest number of cases of tetanus treated in any single hospital in the world. Mr. Peet has carefully analyzed this collection of cases, and the following are his conclusions:

1st. That the idiopathic form of tetanus is much more frequent in Bombay than in other parts of the world; and that, contrary to the experience of the disease in other places, it is more severe and fatal than the traumatic species.

2nd. That it is often traceable to direct exposures at those seasons during which there are the greatest alterations of temperature.

3rd. That of the traumatic form, many cases are ushered in by distinct febrile symptoms; but there is not sufficient reason to conclude that this constitutional disturbance is evidence of any more decided state of inflammatory action in the nervous centres than is present in cases where febrile symptoms are altogether absent. Nor would it appear that this febrile state is any indication of the severity of acute nature of the attack.

4th. That there is little doubt that there exists in tetanus, as in other diseases, a period of incubation, but there are no facts to determine the length of time over which that state may extend.

5th. That in many cases the more marked symptoms are preceded by a peculiar expression of face, to which sufficient prominence is not,

perhaps, usually given, as a means of detecting the approach of the disease, and that this changed expression may exist for several hours before any other symptoms of tetanus is present.

6th. That no reliance can be placed upon the frequency of the pulse as evidence of the severity or otherwise of the attack; but the condition of the pulse in regard to strength is of much importance.

7th. That hurried respiration and dysphagia almost invariably indicate a fatal termination.

Mr. Peet remarks, in regard to the various modes of treatment pursued, "that the result has been nearly the same in each; namely, that of leading to the conviction that there is no one mode of treatment better than another in the management of this disease." He would, however, except from this general statement the assiduous use of nutritious food; this should be considered an essential element in whatever plan is adopted, for on this, rather than the medicines employed, will the favourable termination of the case depend. "The inhalation of ether and chloroform," he says, "has been well tested. That it affords great, and, for a time, complete relief, there cannot be a question, but it has not appeared to have any influence in preventing, or even delaying, the fatal termination." The majority of cases were treated with preparations of the Indian hemp, but Mr. Peet does not consider it superior to other narcotics. The report closes with a table of all the cases treated during this period.

Treatment of Tetanus. Dr. LAWRIE gives (Glasgow Med. Jour.,) the statistics of this disease with the following conclusions as the result of his investigations into the success of different methods of treatment.—"It may be asked if, as the preceding summary would appear to show, no remedy yet tried has done good, and many have done harm, how are we to treat tetanus? I reply, negatively; put the patient into a dark room, keep him absolutely quiet, don't torture him with remedies proven to be useless, give him as much nourishment as he can swallow, and trust the result to the powers of his constitution. If the spasms are severe, alleviate them by chloroform. Still, I do not despair that a remedy will be found, if not adequate to cure the disease, yet powerful enough to enable us to "fight against death," until the chronic stage is established, and the result can with some confidence be entrusted to nature. Such a remedy I would look for among the anæsthetics. The discovery by Mr. Richardson of the virtues of the *Lycoperdon Proteus*, encourages the hope that many substances in the vegetable and mineral worlds will yet be found, possessed of powers analogous to those of chloroform, but so far different as to make them curative in cases in which that wondrous agent is only pallative, if not at times injurious. Unfortunately, chloroform excites tetanic spasms, and interferes with and arrests respiration and circulation. Shall we ever discover a remedy which will relax and never excite spasm, and continuously deaden pain without causing pulmonary or cardiac congestion? When we do, I shall hope that tetanus may be cured.

In the mean time, as an encouragement to lay aside medicine and trust to nature and mild nourishment, I shall shortly relate the follow-

ing case : —, aged 12, October 8th, 1853, was amusing himself with some of his companions shouting at a marriage party. The bridegroom, being irritated, threw a collier's boot among his tormentors, the heel of which struck this boy on the face. The skin was not wounded, but the bones of the nose were broken; and on the 13th, they were partially replaced by my friend, Mr. Tindal. On the 22d, fourteen days after the accident, and nine after replacement of the bones, tetanic symptoms appeared. I saw him on the 23d with Mr. Tindal, and found the pulse from 115 to 120, the lower jaw, face, and neck rigid, abdomen tense, with acute diaphragmatic pain. Purgatives, followed by small doses of Dover's powder, had been given. I proposed that the opiate should be omitted, that the violence of the spasms should be met by inhaling chloroform, and that infusion of digitalis should be freely given. The chloroform acted well, but he struggled against it, and did not like it; of the digitalis he had four ounces, and then refused to take more; purgatives would not act. Under these circumstances, at our visit on the 28th, we agreed to lay aside all medicine, to open the bowels by occasional enemata, to keep him quiet and give him as much milk as he chose to swallow. To avoid multiplying details, it is sufficient to say that for eight days the paroxysms recurred with great severity every twelve or sixteen hours, at times almost threatening immediate death; he had a troublesome cough with considerable expectoration, but otherwise he breathed easily, and swallowed well. He constantly kept a small wedge of wood between his teeth, notwithstanding which, the spasms of the lower jaw were so severe as to force out four of the incisors. He drank milk freely and without difficulty through a quill. He is now quite well. Cured by milk!

Conclusions drawn from the Results of Re-vaccination of 257 Cases.
By W. BIRD HERAPATH, M. D., of Bristol.—(Assoc. Med. Jour.)—The inferences to be deduced from a consideration of these experiments are the following:

1. That three cases re-vaccinated within seven years were not again susceptible to vaccine.
2. That vaccine, after the interval of from eight to seventeen years, does not prevent the reception of vaccine again, except in 22-174 per cent.
3. That the distinctness or imperfection of the vaccine cicatrix does not materially alter these results.
4. That variola does not prevent the formation of the vaccine vesicle, except in about 23-53 per cent.
5. That the occurrence of small-pox subsequently to vaccination does not destroy the susceptibility of the human system to again receive the vaccine poison, except in about 10 per cent.
6. That in all the previous cases, whenever the secondary vaccine vesicle assumed its perfect form, its subsequent history was the same as if the system had not previously laboured under vaccine variola or varioloid.
7. It is probable that the protective influence of vaccination has di-

minated in consequence of repeated transmission of the vaccine matter through the human body.

8. It is desirable that re-vaccination should be extensively followed, as one means of giving additional protection to the masses.

9. That, when possible, the stock of vaccine should be renewed by going back to the original source.

Typhoid Fever.—(From Transactions of the College of Physicians, of Philadelphia.)—Dr. Wood presented a brief summary of his experience in the treatment of typhoid or enteric fever in the wards of the Pennsylvania Hospital during the past three months.

Since the first of November last, the number of cases under treatment was twenty-eight; some of these were in the house at the time Dr. Wood commenced his term as prescribing physician, but much the greater number were admitted subsequently. The last of the patients is now convalescent; only one case terminated fatally.

The fever exhibited, in different patients, every diversity in respect to character and severity. The head and lungs were frequently implicated in the course of the disease. In all the cases, the rose-colored spots were detected upon the abdomen, though in some they did not exceed two or three in number.

The treatment pursued by Dr. Wood was that recommended by him in the work on the *Practice of Medicine*.

In the case which terminated fatally, the patient was extremely ill when he was admitted, and died three days afterwards.

Dr. Wood remarked, that pneumonia occurring in the advanced stages of typhoid fever was a frequent cause of death, and he has been struck with the very great relief produced, whenever pneumonic symptoms supervene under these circumstances, from the application of a few cups to the chest, or, in cases of extreme prostration, where even a few ounces of blood could not be drawn without danger, from the application of dry cups.

In one instance, where the patient had been long ill—with serious affection of the head and bowels—pneumonia set in. The presence of the subcrepitant r le was detected over the whole posterior portion of the lungs; the breathing became extremely laborious, the countenance purplish and anxious, and the entire aspect of the case alarming. Dry cups were freely applied over the whole back of the chest. The relief was prompt, and, by a proper course of treatment, the patient finally recovered.

Dr. Wood adverted to the necessity of pretty free stimulation in the advanced stage of typhoid fever, even in those cases in which, from the occurrence of pneumonia, cupping may be indicated.

to commence. This statement was confirmed by Dr. Beale of London, who, in the 25th volume of the Transactions of the Medico-Chirurgical Society of London, furthered our knowledge regarding it by additional valuable researches. My attention was directed to this remarkable fact during the present session by Dr. Robert Cartwright, a gentleman attending the Clinical Wards of the Infirmary, who informed me that he had seen it occasionally of great service in a diagnostic point of view, in the clinical wards of Professor Oppolzer at Vienna. It so happened that a man, John M'Donald, æt. 25, had just been admitted, labouring under well marked simple pneumonia at the apex of the right lung. He was a labourer, who had enjoyed perfect health until two days before admission, when, on being exposed to wet and cold working at drains, he was seized with shivering, followed by fever and the usual symptoms and signs of pneumonia. On adding a drop of nitric acid to some of his urine in a test tube, and then dropping into it a little solution of the nitrate of silver, the fluid remained clear, although so great is the delicacy of this test, that a white cloudy precipitate is at once formed, if a very minute quantity of the chloride of sodium be present. It was on the fourth day of the disease that the observation was first made, and the chlorides remained absent during the fifth and sixth days; during which period the disease extended from above downwards, until it occupied the upper two thirds of the right lung. On the seventh day a slight haze was observed in the urine, indicating that the salt was returning to that fluid, and the man expressed himself as being much better. On this day there was great dulness on percussion, all crepitation had ceased, the breathing was tubular with bronchophony. On the eighth day slight returning crepitation was audible, the dulness had diminished, but the urine, owing to some accident before the visit, had been thrown away. On the ninth day, however, the chlorides were abundant in that fluid, together with lithates, loud crepitation was now universal throughout the lung, and the dulness had nearly disappeared. From this time the man made a rapid recovery, never having been bled, and was discharged quite well on the sixteenth day.

I now requested Mr. Seymour, one of the clinical clerks, to test the urine of all the patients in the ward, and others who might subsequently be admitted, which he did, and thus collected a large number of observations, the results of which I shall allude to immediately. In the mean time another case entered, which seemed to point out the value of this test in a diagnostic point of view. It was that of a man, Donaldson, æt. 26, labouring under typhus fever, in whom the disease ran its usual course to the tenth day, when chlorides were demonstrated in it. On the 11th day, however, pulmonary symptoms came on, and the chlorides were entirely absent from the urine. This led me to make, with the clinical class, a careful examination of the chest, when all the signs of pneumonia were detected in the lower half of the right lung. On the

monia of the lower two-thirds of the right lung. No consistent account could be obtained from him as to when the disease commenced, and it was impossible, therefore, to determine whether the coarse crepitation which was audible over the inflamed lung was the advancing or returning crepitation. But the chlorides were absent from the urine, which indicated that the disease was advancing. The following day complete consolidation had occurred, with dry tubercular breathing and absence of crepitation, and a minute quantity of the chlorides was found in the urine. The patient, however, instead of getting better showed no improvement, and the next day the chlorides had again disappeared, indicating extension of the pneumonia. On the evening of this day he was seized with acute meningitis, of which he died. On dissection, in addition to universal cerebral meningitis, the whole of the right lung presented the usual characters of grey hepatization.

These cases serve to point out a remarkable connection between the absence of chlorides from the urine and the onward progress of pneumonia. I forbear from offering any opinion as to the theories which have or may be advanced on this subject. The fact requires to be more extensively investigated clinically than has yet been done to test its value.

Mr. Seymour tested with great care, and at repeated times, the urine of upwards of fifty other case in the wards, embracing a great variety of disease. He found the chlorides absent in one case of phthisis, with intercurrent pneumonia, but in no other. They were also absent in one case of peritonitis, and in all the cases of small-pox. Further investigation will probably discover these salts to be absent in other diseases, which, although it may diminish the importance of the sign as distinctive of pneumonia, leaves unaffected its value as pointing out the onward progress of that disease.

I need only now allude to one other point, viz: that if any phosphates exist in the urine, nitrate of silver throws down a faint sediment, which, although it cannot be mistaken for the precipitate of chlorides in healthy urine, may be confounded with the appearance it presents when small in amount. In such a case the action of ammonia, by dissolving the chlorides, is at once distinctive.

Treatment of Porriga and of Thrush with sulphurous acid and sulphate of soda. By Dr. W. JENNER. Agents, the effects of which is to destroy directly the parasite, are called "parasitocides." Several agents having such an action have been brought before the profession. Some physicians use a solution of corrosive sublimate. Acetate of copper has been also employed; but these agents are not sufficiently powerful parasitocides for the small quantity of them that finds its way into the hair-follicles to kill the growths occupying that situation. Therefore.

observed, and diseased hairs alone be operated on, the patient suffers no pain from what seems, on paper, a very terrible operation.

It is highly probable that the employment of sulphurous acid, as a parasiticide, will altogether do away with the necessity of epilation. This agent was suggested by Prof. Graham, as a possible remedy for cholera, at the time that disease was supposed to depend upon the presence of entophytes in the intestinal canal. It was first employed by myself to check fermentation, and to destroy the *sarcinæ* Goodsiri and *torulæ cerevisæ*. When lecturing on this subject some time since, I said: "Considerable benefit may be anticipated from the employment of sulphurous acid, in all diseases attended with the development of parasitic plants. I would especially mention *porrigo*."

In regard to *porrigo* (*tinea favosa*,) these anticipations have been fully realized, and the results of a single case make it probable that the beneficial effect of this parasiticide will be equally great in *tinea decalvans*.

I may mention, too, that in some forms of thrush, this agent acts most rapidly, one application of a solution of sulphite of soda (3j. to 3j. of water,) sufficient to remove the disease from the mucus membrane of the mouth in twenty-four hours. The secretions of the mouth being acid, (?) the salt is decomposed and sulphurous acid is set free; in this, as in all other cases, the sulphurous acid is the active agent in the destruction of the parasite.

I can not conclude without expressing my confident belief, that a great advance was made in pathology when the vegetable nature of the disease I have referred to, as well as some others, was demonstrated; and my equally confident belief that the foundation for a great advance in therapeutics was laid when Professor Graham called attention to the power of sulphurous acid to destroy vegetable life, and explained how it could be given internally without injury to the patient.

Dependence of Furor Uterinus and Nymphomania: From observation of it in a Mare. (Condensed from *Journal de Médecine et de Chirurgie Pratiques*, February, 1854.) M. Reboul, veterinary physician at Coursan, (Aude,) has recently published, in the *Journal des Vétérinaires du Midi*, a very curious observation of *furor uterinus* occurring in a mare, and which may throw some light upon the same affection in the human species.

It occurred in a magnificent creature, six years of age, which, on the 24th of June, gave signs of heat, for the relief of which some insignificant means were employed; but the accident having assumed a certain intensity, M. Reboul was called, and found the animal in the following state: The mare, which had been for some moments sufficiently calm, raised suddenly its head beyond measure; its eyes were animated, hagard and salient from their orbits, and the ears thrown at one moment in front at another behind, seeming to give it the attitude of a beast listening attentively. There was a continual frisking of the tail; her flanks were hollow and drawn in; the lumbar region vaulted, and the contractions of the vulva repeated so as to eject without ceasing the viscous mucus and a yellowish white substance. Some times the head would sway from side to side in its stall; the mare neighed loudly and

energetically, seized the trough with its teeth, lifting it up; bit the laths of its rack with rage and presented all the symptoms of an attack of phrensy (*accès frenétique*). To this agitation soon succeeded a profound calm characterized by a state of torpor and sleepiness which lasted twenty-five minutes, and which was interrupted by another paroxysm as violent as the first.

These attacks soon became more and more frequent, and the life of the animal being seriously menaced, the owner decided upon having a stallion brought to her. After approaching, however, she was refused by him, notwithstanding all the pains taken to induce an opposite course. The mare, although dying, was disposed to receive him; allowed herself to fall like an inert body upon her litter, dying without relief at the end of a few hours—the erythism of the generative organs persisting even to the moment of death. The autopsy exhibiting considerable engorgement, with inflammation of both ovaries, particularly of that of the right side.

A second observation, almost similar to this was collected by M. Reboul, and the results of the autopsy had also proven that there existed a well pronounced ovarite, which induced the author to suppose that this lesion is the point of departure of the furor uterinus.

Treatment of Itch. (Condensed from Gazette Médicale de Paris, Feb. 1854.) M. FELIX JACQUOT, in referring to the great improvements in France and Belgium in the treatment of this disease, by which its length is lessened from twelve to two and even one day, says :

In Belgium, according to the report of M. Vleminckx, as soon as the disease is recognized the patient is immediately covered with block soap, which is rubbed in for an hour; he is then placed in a bath where the frictions are continued for another half hour. Upon leaving the bath the friction is continued the same length of time with from sixty to ninety grammes of calcarious sulphur; the skin is then cleansed and the cure is completed. This process is sometimes rendered more simple still by the use of sulphurous soap, obtained by a mixture of (calcarious) sulphur and soap. The skin of the diseased person, first softened by immersion in the bath, is subjected to continued and prolonged frictions with this compound substance. A soldier, for example, who has been subjected to this, may rejoin his companions on the same day, without fear of ill consequences either to himself or them.

In France, the council of health rendered obligatory a treatment quite similar to the above. Either black, green, or flax soap is employed also with the sulphuro-alkaline pomade.

SURGERY AND SURGICAL PATHOLOGY.

Observations on the Human Eye by means of the Speculum Oculi.—(The Amer. Med. Monthly).—[The first and second numbers of the Deutsche Klinik (Berlin) for January, 1854, contains an able article by Dr. Oscar Seemann, on the *Speculum Oculi* of Helmholtz, and its ap-

plication to the diagnosis of diseases of the organ of vision. The first part of this paper is chiefly devoted to a description of the mechanism of the instrument, the manner of its application, &c. The second part is that which we present to the readers of the Monthly, under the above title. H. N. B.]

In order that we may be able to distinguish with our eye the pathological alterations existing in any organ, it is necessary that we should be familiar with the appearances which that organ presents in its normal condition. It becomes necessary, therefore, that I here give a brief sketch of what we perceive in the healthy eye by means of the *speculum oculi*.

If we give the reflector such a position that it sends the rays of the taper passing through the convex glass into the eye to be examined—by which the dark spot, which corresponds to that point of the speculum not covered with foil, must fall directly upon the pupil—and look through the transparent portion of the reflector, we see the pupil clearly illuminated. The degree of its lucidity is, under otherwise equal circumstances and with a sufficiently dilated pupil, different in different eyes, and depends upon the greater or less capacity of the background of the eye to transmit and reflect light. A part of the rays of light, for example, is reflected by the retina and its vessels; another part passes through these to the choroidea. Of this transmitted light, again, one portion is reflected by the vessels of the choroidea, whilst another is absorbed by the pigment of this membrane, and the remaining portion penetrates the more transparent parts of the same, to be at last reflected by the sclerotica. The stronger the pigment of the choroidea is developed, the more the light is absorbed; the less it can penetrate, so much the more dimly will appear the illumination of the pupil: the less the pigment is developed, the more the light is reflected; so much the clearer will be the pupil. The colour of the light presents all shades, from whitish-yellow to yellowish, from yellowish-red to the finest rose, which latter colour I observed in an albino. Van Trigt directs attention to the fact, that the abundance of the choroid pigment is in direct ratio with that of other tissues, especially the hair; and that consequently in blonde individuals the pupil appears much clearer than in individuals with brown or dark hair. If we now apply to the instrument the concave lens corresponding to the state of refraction of both eyes, we perceive, in the background of the eye, vessels of larger or smaller size, which sometimes run isolatedly, sometimes so that artery and vein lie together. The background of the eye itself appears in a reddish light, which passes to a dark brown when the pigment is very abundant, but shines of a clear rose colour when the choroid pigment is less. The particular parts of the retina also do not appear equally coloured; the colour is clearest about the *optic nerve* and grows gradually darker towards each side. If we direct the eye somewhat inwards, it is not difficult, after some practice to discover the *papilla nervi optici*, which exhibits a truly splendid appearance. It presents itself generally as a circular, more rarely as an elliptical, clear white disk, which, shining like the full moon in a blood-red sky, is more or less netly bounded by the surrounding parts, and at its periphery is girdled by a dark, often

not wholly closed ring of various breadth. At times, here and there, single dark spots may be distinguished upon the shining disk, caused by little inequalities of the *papilla*. Somewhat inward from its middle point issue the *arteria* and *vena centralis*, the first of which is marked by its clearer red colour and smaller circumference, sometimes single, sometimes forming a coil. For the most part of these vessels extend upwards and downwards over the papilla, and divide near its periphery into two or more branches; but they do not always take so regular a course, but wind about in all directions, sending out their twigs over this disk. The artery, as well as the principal trunk of the vein, exhibit at the summit of their curve a light streak, which proceeds from the reflected light, and is not observed in the neighbouring venous branches, because their walls are too little arched, more level. The falsiform line of shade lying inwards from the papilla, which Helmholtz has always, and Van Trigt never seen, I have also, at times, but not always, been able to discern.

If we cause the eye of the individual examined to look directly at the image of the taper present in the reflector, we have before us the point of direct vision, the *macula lutea*. Helmholtz says it rises less abruptly from the surrounding parts, has a darker gray-yellow colour, and shows no vessels. According to our observations, however, it is distinguished in nothing from the rest of the background of the eye, neither by a different colour or a greater want of vessels.

If we examine the normal eye with convex glasses, through which the eye of the observer is adapted also for the anterior parts of the same, *cornea*, *iris*, *lens*, *corpus vitreum*, we see the magnified pupil shine with uniform clearness.

After this short description of that which we perceive by means of our instrument in the normal eye, I proceed to the observations which we have made upon the diseased eye.

Diseases of the Lens.—The most frequent diseases of the lens are opacities *cataractæ*. When the opacity is far advanced, and its colour a clear gray, there is no difficulty in the diagnosis; but the cognizance of opacities in their first commencement is extremely difficult, and the difficulty is increased from the fact that in elderly persons generally the pupil does not appear of a pure black. But, by the aid of the speculum, it is possible to detect even the slightest opacities of the lens and its capsule, since the latter are very accurately defined when the background of the eye is illuminated, and we examine through a convex lens of 3—6th focal distance.

We have observed commencing opacities of the lens, which were wholly impossible to characterize according to the ordinary methods of

from the smallest point to a deep black disk of nearly 1^{mm} in diameter, and was always conformable, no single dark atoms being distinguished as composing the opacity. Its periphery was mostly circular, in two cases elliptic, with the greater diameter running obliquely; also, dentate, stellate. It occurred in 20 cases bilaterally, and had but rarely reached the same stage of development in both eyes. This deeply dark disk was sometimes surrounded by gray, irregularly formed specks, which, in one young lady, represented a second circular concentric disk in both eyes; in other cases by a large number of dark points of various size, but which never equalled the nucleiform cataract, and were sometimes very irregularly placed, sometimes appearing, more or less clearly, as a concentric layer, sometimes as a vicular, or as a stellar stratum. We saw these dark points, also, in those three individuals in whom the nucleiform cataract was wanting. In seven cases an opacity was seen at the outer border of the lens, which twice was encircled by a ring accurately defined and circular within, once by two concentric rings, separated from one another by a clear line, and consisting of thickly crowded points, while four times dentations were sent out towards the middle of the lens, by which the existing nucleiform cataract was rendered dentate or stellate.

The examination of more perfectly developed opacities of the lens, which may be already diagnosticated by the mere sight, shows that such cataracts always have a greater circumference than one would suppose from an interior view. Here the speculum can accomplish something in the determination of the time at which the cataract will attain sufficient maturity for operation, since we have, in the visible progress of the opacity, a sure guide independent of the self-delusions of patients. In the examination of such strongly developed opacities of the lens, at times a deep black figure appears to us, which consists of three radii proceeding from the pole of the lens, growing smaller towards the equator; their direction corresponds exactly to the meridian lines, and there is no doubt that we have to do in such cases with the cleaving of these, often observed in cataractous lenses. Since the meridians of the anterior and posterior hemisphere of the lens have an opposite course, so we can easily determine from the course of these dark radii in which hemisphere the cleaving has taken place.

For the exact history of the development of cataracts, the speculum will be of the greatest use. Already it is proved with greater certainty that most opacities of the lens, taking their origin in the nucleus, extend in a centrifugal direction. This centrifugal extension is at times met by a second opacity, beginning at the border of the lens, and progressing in a centrifugal direction; but the latter is exceedingly rare. The opacities of the lens are composed of single dark atoms, which increase in number, become thickly crowded together, and at last conglomerate to a conformable mass. This conglomeration often proceeds with a certain regularity, and two special forms of it may be observed, namely, either these dark atoms unite to form concentric rings, or radiate lines. Only the opacity of the nucleus of the lens has appeared to us, hitherto, always conformable; probably the cause of this lies in the denser stratification of the fibres which exist in the nucleus, and the

want of abundant uniting tissue. If the speculum enables us on the one hand to recognize the least beginnings of the cataract formation, so also, on the other hand, it often teaches us that there is no opacity of the lens existing, when, from the mere sight, we should conclude that such was the fact. The pupil, especially in older persons, is not a pure black; it appears grey-yellow, whitish yellow. Several cases have occurred to me, in which physicians had declared to eye-patients that they had a cataract, and might be eventually cured by operation, when the speculum proved that there was absolutely no opacity of the lens existing, and that the disturbance of vision was dependant upon a very different lesion of the eye.

Diseases of the Corpus Vitreum.—The circumstance that the vitreous body is almost completely withdrawn from observation, without illumination of the background of the eye, had made the diagnosis of its diseased conditions impossible. The disturbance of vision dependant upon this body, on account of the want of all externally perceptible alterations, were referred to lesions of the retina, and placed in the Augean stable of amblyopia and amaurosis. The speculum now teaches us, that diseases of the vitreous body, especially obscurities of the same, occur no more rarely than in the lens. These obscurities are of a twofold character:—either the vitreous body loses its natural transparency, the capacity to transmit rays of light to the retina, in which case, by the application of a convex lens, No. 6, the background of the eye appears wholly or in greater part very dimly illuminated, and the retina-objects to be found behind the obscured spots, as well as the vessels and papilla of the optic nerve, are either not at all or only very indeterminately made visible by the convex lens; or black corpuscles are found floating in this body.

The first, from the analogy of opacities of the lens, we must consider as true obscurations of the vitreous body; they appear to us like cloudy opacities, which have a diffused extent, and almost always involved the greater part of the vitreous body. In sixteen individuals, six of whom were completely blind, and the rest all suffering from important disturbances of vision, we diagnosticated this diffuse obscurations, since the retina objects could not be perceived by any concave lens, and the background of the eye appeared dim, as if washed away. A confounding with opacities of the lens could not occur here, since cataracts, which so materially obstruct the vision, could scarcely escape an exterior view, to say nothing of the examination by the speculum. Besides, in five cases there simultaneously slight opacities of the lens, which, however, appeared as small points, and could by no means explain the above symptoms. The conclusion may be drawn, that obscurations of the vitreous body of relatively slight intensity produce serious injury to vision, whilst caractous lenses, which show exteriorly a tolerably intensive gray colour, often allow the patient the reading of coarser prints.

Still more frequently than these obscurations, we observe those dark floating corpuscles in the vitreous body, which are set in lively motion by the slightest oscillations of the globe of the eye, and cross before the clear, shining pupil in the most different directions. These corpuscles

present the most manifold forms; sometimes they resemble small coiled serpents, sometimes polyhedric cells, sometimes long, irregularly formed coagula, sometimes they appear as innumerable floating points. We observed, also, the most various forms in the same eye, and, indeed, sometimes in such great number, that the vitreous body, after a movement of the globe, appeared like dirty swamp-water. When the globe returned to rest, these corpuscles sank to the bottom from the force of gravitation. We made this observation upon 26 individuals; four of them had normal sight; the most, however, complained to us of their own accord that they saw dark bodies floating in the air, and the description which they gave of their form frequently agreed very exactly with that observed through the speculum. They were often very short-sighted, so that they required acute concave spectacles. The vessels of the retina and choroidea were most clearly visible, nevertheless, we were obliged to use strong concave glasses, which corresponded well with the nearness of vision.

Concerning the nature of these corpuscles, nothing definite can be said; pathological anatomy and microscopy must furnish an explanation of them. We have not, hitherto, detected in them a spontaneous motion, so that we cannot at least consider them *living* entozoa. They may, indeed, often be blood or exudation-coagula; and the circumstance that in two cases we found a tolerably extensive extravasation of blood upon the retina, favours this view in some measure. From their exterior form, we might often also be led to consider them as cells; but it would be remarkable that such cells should remain so many months in the same stage of development. Perhaps they are many times residua of the lamellæ of the vitreous body, which, according to Bowman, exist in early life and later are broken up.

The frequent, manifestly swift movement of these corpuscles in the substance of the vitreous body, which, according to K lliker's latest investigations, consists, in adults, of a more or less consistent mucus, permit us, nevertheless, to decide upon the fluidation of this body with some certainty, which accords also with the short-sightedness of such patients so frequently observed by us. Fluidification of the vitreous body makes its co-efficients of refraction smaller; but this is not sufficient to explain the frequent high degree of nearness of vision if we do not admit that through this means the diameters of incurvation of the refracting media are altered. In one man, who complained that upon motion of the eye he saw small, clear, shining corpuscles floating about in the air, I observed in the vitreous body little glittering points, visible sometimes here, sometimes there, which disappeared upon the rest of the globe. Could they have been crystals of cholestrine?

Diseases of the Retina Choroidea.—The background of the eye appears to us, as we have already described above, as a field of vision shining with a reddish light, intercrossed by larger superficial vessels of the retina sending out single smaller branches, and by a deeper lying convolute of smaller vessels of the choroidea, upon which the *papilla nervi optici* is accurately delineated through its intense brightness and the central vessels so clearly visible upon it. The truly surprising clearness

with which all this is seen, did not allow Helmholtz to doubt that vascular distensions, varicosities, exudations before the retina, in its substance, and between the retina and choroidea, would be easily recognized. The observations hitherto made public have dispelled every doubt; and we also have not rarely observed pathological processes of the retina and choroidea. If we have not been able hitherto to recognize all visible abnormalities in their true essence, if we even overlook much which is abnormal, nevertheless, we can already assert that a large number of amauroses, which have heretofore been considered as neuroses, depend upon visible textural alterations of the retina and choroidea.

Most frequently we have observed distention of the vessels of the retina, by which their main trunks appeared enlarged, sent out many branchlets not formerly visible, and the whole background of the eye appeared of an unusually intense red color. It is true, that the size of the vessels, the number of their branchlets, the colour of the background, are very different in different men, and therefore, error is very easy; but the correctness of the diagnosis is favoured on the one hand by the subjective symptoms, the pain in the eye and frontal region, the feeling of unusual fulness in the eye; on the other hand, by the frequent favourable result of treatment by the abstraction of blood.

The true inflammation of the retina with exudation seem also not to be rare. The spots covered with exudation appear, sometimes, whitish, reflecting the light strongly; sometimes reddish; sometimes they have a more greenish tint, and when of greater extent, are surrounded by an irregular dark border; sometimes, especially upon the *papilla*, they appear as dark specks. The exudation-mass, for the most part, enclose the vessels of the retina in such a manner that they become completely invisible, or at least appear only very dimly as white-red, not well defined marks. Only twice have I seen tolerably clearly the vessels of the retina running over the exudation-mass. In one case, already alluded to above, in which the vessels of the retina were surrounded by a reddish exudation, we observed simultaneously in the vitreous body those coagula-like corpuscles, which probably had become loosened from the floor of the retina, and floated free in the vitreous body.

Injuries of the retina and its vessels from external violence very rarely occur, and are then mostly connected with such important lesions of the whole bulb, that examination with the speculum can furnish no results; it may also happen that a cataract needle in unskilful hands, or the lens dislocated by it, injure the retina. We have not observed cases of this kind. Van Trigt has wounded the interior eye of cats, dogs, and other canines, with needles, and in this manner made highly interesting observations upon the results of these injuries and cicatrization of the wounds. On the other hand, we have observed a case of spontaneous rupture of the central vessel, which I may be allowed to report in detail.

A man, 55 years of age, plethoric, was suddenly seized with giddiness, and sank into unconsciousness. As he aroused from this condition after an hour, he was blind in his right eye, which before was perfectly normal. The physician called first, made a venesection, and otherwise conducted the treatment *lege artis*. Upon the fifth day after this mishap, he came to my office to consult me on account of his eye. The

pupil of the right eye was more strongly dilated than that of the normal left eye; both had a yellowish grey mucus, otherwise nothing abnormal exteriorly. The examination with the speculum gave the following result: the *papilla nervi optici* was accurately bounded on the outer side, nevertheless even here was more dimly illuminated than usual; at the inner and inferior side it was covered with a red blood-coagulum. From the upper border descended a vessel, which, irregularly dentated, terminated free; all the other vessels of the *papilla* were covered with the coagulum. Otherwise all is normal. There could be no doubt that here a laceration of the central vessel had taken place at the point where it issues from the *porus nervi optici*. Ten days later, the coagulum, now of a less dark red, was still visible only at the inner border; all the rest of the *papilla* was of a greenish colour, with occasional clear spots interspersed; out of its midst was now seen passing downwards a rose-coloured, not well defined line, which glimmered but dimly, and at its superior border vessels were distinguished which represented two arches. In the vitreous body floated single dark specks. The power of vision continued totally extinguished.

Morbid dilatation of the vascular walls we have never observed except in veins, which then appeared like dark, undulatory or spiral cords running over the field of vision. We never found them upon the *papilla*.

In a young man, who saw objects perfectly clearly in a bright sunlight, but recognized them very imperfectly with a cloudy heaven, at evening, and by lamp-light, we found the retina covered round about the *papilla*, here and there, with black specks; the *papilla* itself likewise contained several dark specks, but appearing less black, and less accurately circumscribed. Otherwise nothing abnormal. The supposition that here a pigment-transudation had taken place, sufficiently explains the perfectly normal perceptibility of the patient existing only in an intense light.

In conclusion, I will make mention of one other cause, in which the speculum showed us, with tolerable certainty, that a lesion of the optic nerve itself was the cause of the blindness.

F. S., 11 years of age, with brown hair, brown-yellow iris, and very dilated pupils, was born amaurotic in the left eye; the right eye normal. The left ocular globe is a little smaller than the right, and sits deeper in the orbit. The *papilla nervi optici* appears as an unusually small elliptic disk whose lesser horizontal diameter is about half as large as the greater perpendicular diameter; the *arteria* and *vena centralis* give off as usual two branches, but very fine, and besides the branchlet passing inwards, a second is also here visible running outwards; the *macula lutea* and the rest of the background of the eye present nothing abnormal. By means of a concave lens, No. 3, we discovered a central punctiform opacity of the lens. The right *papilla* is circular and quite large. While here, on the one hand, the circumstance that the amaurosis of the left eye was congenital, and, on the other, the obstructed development of the bulb warranted us in concluding upon an analogous condition of the optic nerve, so also the examination with the speculum confirmed this diagnosis.

Sanguinous distention of the choroidal vessels is a very frequent phenomenon, which is characterized by abnormal size and intense dark colour of the choroidal veins. We have also observed, in many cases, genuine choroiditis with secretion of the exudation mass. When the exudation is deposited in small circumscribed spots, the background of the eye appears uneven, and reflects the light from those spots stronger than from others; if the exudation has a greater extent, the retina becomes mostly separated, as it were, at the point of entrance of the optic nerve, and projects like a large eminence, whilst it crowds the *corpus vitreum* out of its position; such an eye has then lost the normal relation of internal parts, and the vessels coursing upon the hill-like tumor may be seen by mere illumination from the mirror without the application of a concave lens. Since such extensive exudations are mostly serous, of a fluid nature, so the retina, together with the vessels, may be seen floating after a movement of the globe. Ruptures of the retina from exudations crowding upon it from behind, in consequence of which it floats about freely in the exudation and disorganized vitreous body, we have not observed. In one case of choroiditis, which had had its seat more in the region of the *ova serrata*, we found the following. If we looked through a convex glass, No. 6, in the direction of the axis of the eye, the pupil being nearly normally alluminated, a dark body appeared at the lower border of the papilla, which was bounded above by three convexities, and floated hither and thither upon motion of the bulb, without leaving the border of the iris; simultaneously there appeared several other dark dentations, also black coagula-like corpuscles floating free. If we looked inward, the eye being directed inferiorly, we saw behind this dark body a yellowish green exudation mass projecting inwards deep into the eye, which formed several strata lying behind one another, representing mountains as viewed from a distance. Through a concave lens we beheld, in the very depth of the eye, other well defined black specks; but the vessels were seen only very imperfectly with the eye directed strongly upwards. The exudation, in this case, penetrated the vitreous body.

If we now review the truly surprising results which the *speculum oculi* has furnished us in so short a time, we can not only concur in the expectation cherished by the highly esteemed inventor, that all the alterations of the transparent media, the retina, and choroidea, found in the corpse, will also be recognised in the living eye; but also assert, with safety, that it will be possible for us now to pursue with exactitude the progressive development of the pathological processes of these structures, as well as their recession, whether effected by nature or not.

The advantages which will be derived by the practising physician, irrespective of the cure of these diseases, we have not hitherto considered; but one thing is certain, that an exact knowledge of the diseased objects must be the basis of a rational therapia: without this knowledge we grope in the dark. We shall not be able to cure all diseases, even in this manner; but the physician gains a great advantage thereby, when, from his knowledge of their incurability, he holds himself aloof from fruitless, perhaps injurious, attempts to cure.

Treatment of Varicocele by the direct Cauterization of the dilated Veins. By M. GAILLARD. (Med. News, from Lancet.) M. Gaillard, of Poitiers, has just published, in the *Gazette Médicale de Paris*, a case in which he cured his patient by a very ingenious and safe operation. The subject was a young man eighteen years of age, who was very anxious to get rid of the deformity, as it prevented him from entering the army. M. Gaillard, not being satisfied with the operations of Breschet, Velpeau, or Vidal, thought that he should endeavour to fulfil the following indications :

1. To obtain the obliteration of the dilated veins without any danger of phlebitis.
2. To include all the veins of the cord, to prevent a relapse.
3. To avoid with certainty the vas deferens and the spermatic artery.
4. To obtain a symmetrical cicatrix, owing to the peculiar situation of the patient.

To obliterate the veins, M. Gaillard resolved to use Canquoin paste; and to fulfil the other indications, he proceeded as follows :

An incision, about three inches long, was made from the ring to the scrotum, and the cord laid bare; the latter was then raised upon a spatula, by tearing the cellular connections existing posteriorly with the sheath. M. Gaillard now dissected the different parts of the cord from each other. (One of the veins was much hypertrophied, and adhered to the vas deferens; much care was required to isolate that vein.) The vessels were thus separated from the vas deferens and the spermatic artery to the extent of one inch. The two latter portions of the cord were then pushed backwards, and kept out of harm's way by small pledgets of lint. Little strips of linen were now passed under the veins, and between the latter and the former a thin plate of lead was glided, on which was spread a thin layer of the cauterizing paste, the latter not, however, reaching the margins of the plate. This plate was then folded over the venous mass, which was thus surrounded and compressed by the sheet of lead, the ends of which were brought over one another, or, as it were, imbricated. The strips of linen, first passed under the veins, were then brought over the plate to maintain the latter *in situ*, and the whole secured by a thread.

M. Gaillard states that his patient was thus radically cured without a bad symptom; the cicatrix was linear, and hardly perceptible; and when the young man applied again to the military authorities, he was admitted into the service.

Case of Death by Chloroform.—(Condensed from Journal de Médecine et de Chirurgie Pratiques for May.)—Cases of death from chloroform have rarely occurred in France, scarcely at all in Paris. One, however, has just happened in the hospital of Saint Antoine, which, notwithstanding its having been carefully reported, leaves us in complete ignorance concerning the cause of a death so exceptional and yet so terrible.

The case has just been communicated to the Société de Chirurgie. A woman, æt. 40, had a uterine polypus. Though pale and anaemic, she had yet the appearance of good health. Nevertheless, taking into consideration the numerous hemorrhages to which she had been sub-

jected and the little pain which the operation would cause, M. Ad. Richard, the surgeon, refused to have recourse to chloroform. The patient, however, insisted upon the administration of the anæsthetic. M. Richard placed her in the horizontal position, poured a certain quantity of chloroform upon a compress, which he himself kept at a certain distance from the respiratory organs. The respiration continued freely and regularly. At two different times he poured fresh portions of the anæsthetic upon the compress. The symptoms of anæsthesia did not delay to manifest, but the period of agitation was sufficiently pronounced. M. Richard prepared to operate; he confided to a student the care of holding the compress and even to pour on a few drops of chloroform. He had scarcely applied the ligature to the polypus, when he was informed by a student that the pulse had ceased to beat. The skin was almost cold, the face of an extreme paleness; the respiration, although slower, was still regular. The patient was soon placed with her head down and her lower limbs elevated. Regular pressure was exercised upon the thorax and upon the abdomen; stimulants were applied to the nose; haggellation to the skin were employed, and the nares tickled with a feather. At the end of ten minutes the heart had ceased to beat, and the respiration scarcely sensible. M. Richard then practised tracheotomy, and finally resorted to electricity, but all with no avail—the patient was dead.

The autopsy was practiced before several members of the Society. Nothing was discovered which could explain the cause of death, there being only established that there was a little chloroform in the blood.

As every precaution had been used in its administration, we are bound to attribute it to a peculiar idiosyncrasy, which we are some times destined to encounter, happily at long intervals, but in which the action of chloroform is promptly fatal; and, as we have not yet been able to expose the signs of its existence, it follows that we are always exposed to a catastrophe that all our prudence cannot prevent. This proposition taken in a general way is assuredly true, but in its application to practice, there is nothing in it which should turn us from the use of chloroform; for, as we have several times said, the cases of death in France are extremely rare, when we consider the daily use of anæsthetics—so much so as to be equivalent to almost complete innocuity. Nevertheless, it teaches us not to have recourse to it without a certain necessity. It is also for the reasons cited above that local anæsthetics have been so well received.

MATERIA MEDICA.

Quinic Ether.—(N. O. Med. News and Hos. Gaz.)—A discovery which has lately been made in Italy, and which has excited much attention, is illustrative of the results of perseverance and industry.

In the month of June, 1852, a young man, M. Louis Manetti, a student of the University of Pavia, happened to witness the death of a patient with congestive fever, who died apparently from the impossi-

bility of introducing into the system in a short time a sufficient quantity of quinine. Manetti was struck with the idea that the principle of the bark might be effectually administered through the medium of pulmonary absorption. Encouraged by Professor Pignacca, Manetti began a series of investigations, the results of which are detailed in a letter from Prof. Pignacca to Dr. Stambio of Milan, a translation of which is found in the "Annales de la Société Médicale de Grand."

Professor Pignacca has called the new agent for inhalation, *Quinic Ether*, probably for want of a better name, for it is not, properly speaking, an ether, and its positive chemical composition is not known. It is a liquid of a special inconstant odor, and is obtained by the distillation of quinate of lime (*quinat de chaux*) combined with alcohol; and is analogous to the ethereal bodies in general, volatilizing like them.

Professor Pignacca states in his letter that he has administered this fluid by inhalation to eight patients; seven of them had tertian intermittent fever, the last neuralgia of the fifth pair. The neuralgia was of an intermittent type. The remedy acted admirably both in the cases of fever and in the case of neuralgia.

The quantity of the agent given is about a scruple at a time, repeated three or four times a day. It is administered in the same manner as chloroform, and it produces sensations somewhat similar.

Professor Pignacca concludes his letter by saying: experiments in the wards of the hospital at Pavia are being made by himself and other physicians, and many observations have been collected, all of which will be embodied in a memoir, soon to be published by Louis Manetti.

[From a number of the *Gazetta Medica Italiana Toscana*, recently received in exchange, we translate the following additional cases. The subject is novel and interesting, and we gladly give place to them.—*Eds. Charleston Med. Journal and Review.*]

Doctors Manetti and A. Scarenzio on the Inhalation of Chinic Ether in Intermittent Fever.—This paper is a continuation of a preceding one upon the new combination chinic ether in intermittent fever. By means of this quinine is inspired into and absorbed through the mucous membrane of the lungs. A number of cases of recovery are detailed with sufficient minuteness of detail to establish the beneficial action of the novel means used. We will transcribe several of the cases reported:

Experiment 9th.—Zara Luigi, 42 years of age, a native of Lodi, but living in Pavia, entered the Medical Clinique the 17th July, 1853. Robust in person, in his youth he had been subject to intermittent fever of the tertian and quartan type, which decreased under the use of bitter tonics frequently repeated. At 23 he also suffered from attacks, which were treated with quinine. On the 10th day of July, 1853, about 2 o'clock in the day, he had a severe chill, which lasted about two hours and a half, accompanied with pain in the head; burning thirst accompanied by heat and sweating, which disappeared at 6 in the evening. The apyrexia lasted until 3, A. M., of the 18th, when the access again recurred. The chill endured more than an hour; was attended with the

usual symptoms, and the access persisted till four of the following morning. From this time he enjoyed an apyrexia, till half-past two of the 10th, when he was again seized with chill, which stadium maintained itself sensibly till 6 in the evening. The heat of the body. (calore) was 40° ., the pulse 98° . The other stages succeeded, as did the previous accesses. The 21st was a day of complete apyrexia. Heat, 36° to 80° ; and after dinner the inhalation was commenced. The first to 6, P. M., the second to $9\frac{1}{2}$. On the 22nd, the inhalation was repeated at $7\frac{1}{2}$ and $11\frac{1}{2}$. The access appeared as usual; but the heat was reduced to 36° . On the 23d, the inhalation was commenced at 8 o'clock and at $10\frac{1}{2}$, at 2 and at 6, P. M. On the 24th, it was commenced at 6 and 8. The heat 26° to 20° . There was no access. At 3, P. M., was from 37° to 20° , the pulse 76° ; and the patient only complained of a slight sense of fatigue. At half-past five he continued well. Pulse 76; the heat, nevertheless, was from 36 to 60. At $8\frac{1}{2}$, pulse 60. On the 25th, heat $37\ 1\cdot5$ —no increase in frequency of the pulse. The apyrexia continued during the days of the 26, 27, 28, and 29: and on the 30th, he left the hospital to enter upon his usual duties.

Experiment cited as 10th.—The following case was transmitted to the author, by Dr. Emilio Marnardi, with the accompanying comments: "I send you an account of a patient cured by the inspiration of chinc ether. Albina Olivati, 6 years of age, had suffered for some days of intermittent fever of the tertian type. The day on which I was called to see him, I found him without fever, suffering only from malaise and weariness of the limbs—prescribed a mild purgative. On the following morning, of the 10th, he was in a chill which had just come on, which lasted about a half-hour, and accompanied with an increase of heat, and afterwards profuse sweating. In the evening the paroxysm declined, and I recommended him to keep his bed. On the next day he was perfectly quiet, having acquired his accustomed cheerfulness. He was subjected to the chinc ether—repeating the exhalation every 3 hours and giving him four of these. On the following day, at 1 o'clock, the child had a slight chill followed by a slight increase of heat and perspiration scarcely recognizable, passing through the access in 3 hours. On the following morning the apyrexia was complete, resumed the inhalation and repeated it five times. There was no further disturbance nor paroxysm. This patient continued without any return.

Experiment 11th.—Bellinzoni Caterana, a married woman, in one of Salles of the hospital first named, had six accesses of tertian fever with intercostal neuralgia. On the 10th of July, a day of apyrexia, she was subjected to two inhalations of chinc ether. She had still an access of fever. On a subsequent day, the 13th, repeated four times the inhalation, and the fever did not again reappear. About the middle of the month of August, she again suffered from intercostal neuralgia, but without being accompanied with a paroxysm.

Experiment 12th.—Luigi Bussetti, profession baker, having had tertian fever, remained some months in a village in the lower portion of Lombardy, and was again taken with periodical fever, the 4th of August, 1853. The paroxysms were ushered in with the chill, followed by heat and sweating. On the first day it lasted from the middle of the day

to 8 o'clock in the evening. On the 8th and 10th, when he was better, commenced its treatment with chinic ether. During the apyretic period six inhalations were made, each of them for about five minutes. The fever did not return, and the patient henceforward enjoyed perfect health.

Experiment 13th.—Isodoro Pelligrino, stable boy, a native of the valley of Pulicelda, a robust person, after some derangement of the digestive organs, was assailed at about 3 o'clock, of the 7th of August, by an ague which lasted for about half an hour, followed by burning heat and copious sweating, which lasted till 12, P. M. On the morning of the 8th, there was an apyrexia, but about 4 o'clock, after dinner, there came on an access similar to the preceding. The same thing occurred on the 9th and 10th. Premising a purgative, the cure was completed with chinic ether. The first two inhalations not sufficing to destroy the access of the 14th, which, nevertheless, was repeated with less severity than the preceding two other inhalations, sufficed any subsequent attack.

Experiment 14th.—Luige Paleare de Monza, laborer, having suffered for five years from periodical fever, which remitted for some days under the use of chinchona, was subjected, on the 10th of August, to the cure by chinic ether. The accesses were severe and exhibited the quartan type. Six free inhalations did not suffice to destroy the renewal of an access less severe, nevertheless, six others cut it short completely, so that for two months he has had no renewal of the fever.

Experiment 15th.—La Signora R. B., living at Monza, formerly tormented by a periodical fever, suffered from severe tooth-ache, each day about sunset. There being a neuralgia of the third branch of the fifth, with a painful spot sufficiently well limited. Being advised to have recourse to chinic ether, the first inhalation so inflamed the neuralgic pain that it did not make its appearance on that day. Three inhalations on the subsequent day prevented any new access of tooth-ache.

Experiment 16th.—Mascandolo Guiseppe, of Zeccone, in the province of Poria, a soldier by profession, entered the medical clinique on the 4th August, 1853. He had been indisposed from youth. Received into the clinique he was found with an acute gastro hepatitis. He was put under the use of anti-phlogistics, when on the 5th August, about 5 o'clock of the evening, he was seized with a severe chill, followed with heat, (40°) pulse 104: he had ringing at the ear. Being examined, he was found with head-ache, pain in the eyes, sclerotic coat yellow, tongue dry, respiration disturbed, pain in the epigastric region at deep inspirations; the spleen surpassing by a little the free margin of the false ribs was elevated as high as the fourth, the left portion extended to the left hypochondriac region, lifting itself so as to furnish on percussion an obtuse space in continuity with the cardiac dulness. Upon pressure there was pain in the epigastric and right hypochondriac region; pain also in the ilio coecal region, excessive meteorism and prostration of strength. He was ordered a bleeding. The access terminated on the evening of the 8th, with profuse sweating, in consequence there was a notable diminution of the weight and pain of head, the patient being left with but a slight feeling of heaviness; he was languid during the night. At 8 o'clock of the 8th, the time of the morning visit, the pa-

tient presented a notable diminution in both local and general symptoms; the heat of body was at 37 to 40, the pulse 70, the tongue moist; (*nessuna cotenna sul sangue estratto.*) At 3 o'clock, P. M., he was again assailed with a severe chill, and about six o'clock he commenced to pass into the hot stage, the heat of the body was 40, pulse 104, head painful, tongue less moist, thirst ardent. On account of constipation a purgative was administered. Running through its accustomed stadii, the fever ceased about 11 o'clock that evening, and with it the pain in the head, and thirst. On the day of the 7th, the inhalation of chinic ether was commenced. Three inspirations repeated three times in three hours, were not sufficient to impede the return of the access, which came on an hour later, was less severe, the heat of surface raised the thermometer to 39, the pulse elevating itself to 93, there did not appear during the paroxysm the heaviness about the head, nor the thirst. The night was passed tranquilly after the cessation of the paroxysm, which appeared one hour before its accustomed time; on the morning of the 8th, the inhalation was continued at 5 o'clock of the same day, followed by two in two hours, which prevented the return of the paroxysms at the usual time, but not preventing, about 7 o'clock, some appearance of it, however, with less force and less duration. No other medication was administered, except the inhalation, when having remained a few days longer in the clinique, of continued improvement, he returned to his house.

Experiment 17th—The Signora G. R., 17 years of age, marriageable, of scrofulous habit, a native of Milan, but living in Pavia, found herself exposed, in August, 1852, in a neighbouring province, to paludal miasm, when she contracted an intermittent fever, of the quartan type, with very strong accesses. Rejecting every means of cure, she was left to attacks of more or less severity, which repeated themselves periodically, even extending through the month of February of the succeeding year, at which time they ceased spontaneously. She remained free from fever for three months, when, on the first of June, the attacks re-commenced, and for the same cause as in the preceding year the periodical quartan type was maintained until the present attempt at treatment was begun. The paroxysms usually appeared at 5½ of the afternoon, ending between 2 and 3 A. M. With the advice of my colleague and friend Mansueto Faraoni, the cure with chinic ether was commenced, and on the 6th of August, at which time appeared an access, two inhalations were made, the first in the middle of the day, the second at 3 P. M. The paroxysm delayed its appearance to 8 o'clock, and at 10 the heat rose to 39–75, pulse to 112; the last stage having ended at 2 A. M. The patient passed the entire day of the 7th in a perfect state of apyrexia. On the 8th the inhalations were repeated four times, every three hours, after which no further attack made its appearance.

Experiment 18th.—Andreoli Giuseppe, of Bissonè, living in Scarpone, a province of Pavia, 42 years of age, postillon by profession, entered the Medical Clinique the 10th day of August, 1853. He had before suffered from intermittent fever; had had a phlegmon of the left leg and a pleurisy. For fifteen days he had had a quotidian intermittent fever, the accesses of which denoted themselves by a chill about

3 in the afternoon, to which succeeded the other stadii. The attacks did not always present equal intensity, but on one day were always lighter than on another. The first visit was paid the patient on the 10th, when he was found with slight headache, a dirty tongue, red at the margin, thirst, epigastric pain upon pressure, and increased aortic impulse. On the 11th the gastric symptoms still existed, notwithstanding there was an apyrexia. At 3½ an attack supervened, and at 5 the patient was a prey to the chill, light, however, as it should have been on that day; heat 39-60; pulse 90. At the advance of night the access terminated. On the morning of the 12th there was an apyrexia, the gastric symptoms, however, persisting; at 3 in the afternoon the chill was ushered in with more intensity than on the preceding day, and it lasted till 5 P. M.—pulse 100, heat 40; at 9 o'clock the access was about ending, when the first inhalation was made with chloform ether. On the following morning, at 6 o'clock, the pulse was 72, heat 37; the inhalation was repeated for the second time. The same was done for the third time at 9. At 4 the patient was still apyretic, pulse 75, heat 37-20; inhalation repeated for the fourth time. At 6 the patient persuaded that the access would not manifest itself, the pulse nevertheless at 90; heat 40; the inhalation repeated for the fifth time. On the morning of the 14th the pulse numbered 70 strokes, the heat rose to 37, the gastric symptoms persisting, tongue dry, thirst ardent. The inspiration was repeated for the seventh and last time, when Andreoli did not suffer from any further attack, and on the 20th returned voluntarily to his house, if not entirely cured, yet much relieved of the gastric affection.

[To be continued.]

Syrup of Elderberries (Sambucus Canadensis) as a substitute for the compound Syrup of Sarsaparilla. By W. H. WORTHINGTON, of West Chester, Pa. (Medical Reporter.) There being much dissatisfaction attending the use of the Compound Syrup of Sarsaparilla in the hands of some physicians, the syrup of elderberries was recommended to my notice by Dr. Benjamin H. Stratton, of Mount Holly, N. J., who for some years has been in the habit of using it in all cases of disease, in which an alternative action upon the system was desired, and for which the sarsaparilla is usually employed. In the treatment of gout, chronic rheumatism, eruptive and syphilitic affections, he has used it combined with the iodide of potassium, with marked benefit. The formula used by him is the following:

R. Juice of elderberries, Ovj.

Sugar crystal, lbxvj.

Mix and boil to a syrup; after allowing it to cool, add to every pint of syrup one ounce of the best fourth proof French brandy, bottling and keeping in a cool place.

Dose, from a desert to a tablespoonful three times a day.

Flattering myself that an improvement could be made in the preparation of the above syrup without injury, I have prepared a compound syrup of elderberries, containing some, if not all of the most active ingredients of the compound syrup of sarsaparilla, (*Guaiaci lignum* and

Senna fol.;) by this means, as I think, increasing the alterative virtues of the syrup, giving it a more marked and active character in the treatment of gout, rheumatism, &c., than it possessed without them. To this syrup may be added the iodide of potassium to suit the views of those prescribing. The formula is as follows:

Rx. Juices of elderberries, Ovj.

Sugar crystal, lb.xvj.

Guaiacum wood, ℥iv.

Senna leaves, ℥iii.

Put the sennæ fol. and the guiac. lig. in three pints of water, boiling it down one-half, and strain. Put the juice and sugar in a kettle, place it on the fire, and when it comes to a boil, add the decoction of guaiac. lig. and sennæ fol., allow it to boil to a syrup, when it must be taken off, strained, and let cool. To every pint of syrup add one ounce of the best fourth proof French brandy, bottling and keeping in a cool place.

Dose, the same as preceding.

The syrup of elderberries is given to the profession chiefly upon the recommendation of Dr. Stratton, whose skill and experience as a practitioner is entitled to the confidence of his medical brethren. If, as he believes, it possesses more certain and prompt remedial virtues as an alternative than sarsaparilla, it ought to be added to our catalogue of official articles. The difficulty of obtaining at all times good sarsaparilla, and especially in the country, increase the claims of this syrup upon our rural practitioners, who can command with facility, and in great abundance, the material for its preparation.

The use of Coffee as a beverage considered in a chemical and physiological point of view. By Prof. LEHMANN. (London Jour. of Pharm., Feb., 1854, from Annalen der Chemie and Pharm., Sept., 1853.) In the experimental consideration of this subject, the author has adopted as a principle, that under a well regulated and uniform diet and mode of life, the quantitative relations of the substances excreted in the urine, present a trustworthy view of the general assimilative process in the individual.

He has, therefore, selected patients whom he considered fit, and while administering to them either coffee or its various constituents separately, has made such regular observations of the urinal excretions as would enable him to infer what is the nature of the efficacy of coffee and of its constituents respectively.

In determining the amount of urea, he has adopted the method recently put forward by Liebig.

The general results of his investigation are—

1. That a decoction of coffee exercises two principal actions upon the organism, which are very diverse in character, viz., increasing the activity of the vascular and nervous system, while at the same time it retards the metamorphosis of plastic constituents.

2. That the influence of coffee upon the vascular and nervous system, its re-invigorating action, and the production of a general sense of cheerfulness and animation is attributable solely to the mutual modification in the specific action of the empyreumatic oil and the caffeine contained in it.

3. That the retardation of the assimilative process brought about by the use of coffee, is owing chiefly to the empyreumatic oil, and is caused by caffeine only when taken in large quantity.

4. That increased action of the heart, trembling, headache, &c., are the effects of the caffeine.

5. That the increased activity of the kidneys, relaxation of the bowels, and an increased vigor of mental faculties, passing into congestion, restlessness, and inability to sleep, are effects of the empyreumatic oil.

Professor Lehmann considers it therefore necessary to regard the action of coffee, and in a less degree that of tea, cocoa, alcohol, &c., upon the organism, as constituting an exception to the general law, that increased bodily and mental activity involves increased consumption of plastic material.

He considers that these substances have the capability of rendering the individual insensible of a certain deficiency of food in virtue of their retardation of the assimilative process.

He thinks it probable, likewise, that these substances have a direct nutritive value, especially coffee as drunk by the Turks and Arabs with the grounds.

He attributes the rapid increase in the consumption of coffee to an instinctive tendency to provide some remedy for the compulsory use of the potatoe as food among the lower classes; and in confirmation of this points to the facts, that the introduction of the potato from the west, and that of coffee (as an article of food) from the east, were simultaneous, and that the increase in their consumption has progressed equally, both as regards amount and locality.

Professor Lehmann considers that the singular preference of one or other of these beverages by particular nations, as well as the eastern custom of drinking coffee with the grounds, are not accidental, but have some deeper reason. This reason, he thinks, is to be found in the different effects of the coffee, tea, &c., and the various requirements of the nations by whom they are used, and instances the use of tea by the English, and of coffee by the Germans and French, as in accordance with this view. The diet of the former affords a larger supply of plastic material than that of the latter people, and while, consequently, the retardation of the assimilative process is an important influence for the German, the proportionably greater nervous stimulus caused by tea is more desirable for the former.

The use of coffee with its grounds, has its analogue in the use of tea mixed with meal, milk and butter, among the Mongols, and other inhabitants of the central Asiatic steppes.

Aconite as a Local Anodyne. (Medical Times and Gazette, November, 1853.) The comparative value of several of our local anæsthetics, was well exhibited in a case recently under the care of Mr. Curling, in the London Hospital. The patient, a woman of 47, had a patch of dry gangrene on the outer side of the left foot. Its slow increase was attended with such intense pain, that for several weeks she scarcely slept at all. To obtain relief, chloroform, on lint beneath oil-silk, belladonna lotion, and the solution of opium, were severally applied to the part,

with, however, but very slight and temporary benefit. A liniment, consisting of equal parts of the soap liniment and of tincture of aconite, was then ordered to be used in the same manner; and so great was its efficiency, that under its influence the poor woman was frequently able to obtain a very fair night's rest. This case is only one among many in which we have seen proved the powers of aconite as a local remedy.

Prize for the Cure of Cholera. (Am. Jour. of Pharmacy.) The Academy of Sciences of Paris, has received a legacy, 100,000 francs, (\$20,000), left by a generous philanthropist, M. Breant, as a recompense for him who shall discover a remedy for cholera. As this is not likely to be soon awarded, it is proposed to give the interest annually for the most useful discovery connected with cholera. During the first empire, a similar sum was left for the curer of croup, which has not yet been awarded. The new legacy will only serve to swell that large sum which the Academy annually distributes in the form of prizes.

New Mode of administering Sulph. of Quinine. (Assoc. Med. Jour.) In the *Revue Médico-Chirurgicale*, for September, 1853, we find a notice of an article by Dr. BARTELLA, on a new method of administering sulphate of quinine. This plan consists in combining with the quinine an equal quantity of tartaric acid, which has the effect of not decomposing the salt, but of rendering it more soluble, and of causing absorption to take place more readily. He gives, at a dose, three grains of disulphate of quinine, and three of tartaric acid. The dose at the commencement is from six to twelve grains of each.

Lac Varnish to preserve Surgical Instruments. Lac Varnish, to preserve iron and steel instruments from rust, may be prepared by mixing five parts of shallac varnish, made by dissolving shallac in linseed oil, with four parts of oil of turpentine.

Coating for Pills. Take a thick mucilage of flax seed, add a quantity of sugar, and evaporate carefully to dryness. This preparation being finely powdered, adheres sufficiently to pills, slightly moistened, to form a coating which is not readily dissolved by exposure to a moist atmosphere.

Influence of Belladonna in Counteracting the Poisonous Effects of Opium. By Dr. ANDERSON. (Dublin Med. Press, from Ed. Monthly Jour.) Dr. Graves had first suggested, that in continued fever, with protracted pupils and coma, if an agent administered internally would occasion dilatation of the pupils, it might also relieve the other symptoms of cerebral derangement. Dr. Anderson, acting on this theory, administered large doses of belladonna in two cases of poisoning by opium, which he related as follows:

A patient of whom I had charge, and labouring under delirium tremens, having received an over-dose of a solution of the muriate of morphia, became comatose. He had taken, in thirty-six hours, two ounces of the solution of the muriate of morphia, and it had been continued

by the attendant after sleep was procured. When I saw him he was in profound coma, his breathing was stertorous, amounting to no more than four or five per minute, and his pupils were contracted to mere points. His pulse was excessively weak, and rather slow; it was quite impossible to rouse him. I ordered him immediately the following mixture: Tincture of belladonna six drachms, in five and a half ounces of water, of which an ounce was to be given every half hour. Three ounces of the mixture were administered with great caution, after which his pupils began to dilate. The six drachms of the tincture of belladonna were taken, and in four and a half hours after the first dose of it was given, the patient was in the following condition: The coma was entirely gone, respirations were between twenty-two and twenty-five per minute, the pupils were much dilated, the pulse had risen to nearly one hundred and twenty in the minute, and was also increased in strength. His countenance, also, from being cold and pallid, had become much flushed, and the whole body was much warmer. He replied readily and coherently to all my questions. He continued to improve for three days after, when suddenly rising to stool, he fainted, and before the assistance of the nurse could be procured he was dead.

A fortnight afterwards a woman about 50, took, at 4 P. M., two drachms of laudanum, and at half-past 5 P. M. three drachms more. She was brought to the infirmary at 8 P. M. After making vain attempts to rouse her from coma, by walking her about, &c., the stomach-pump was used at a quarter past 8 P. M. By this means her stomach was thoroughly evacuated, but no trace of opium was detected by smell or sight. It had probably been all absorbed. A current of electricity was then applied to her hands for nearly ten minutes, but without rousing her. I saw her at a quarter to 9 P. M. for the first time, and on being told that she had been poisoned by laudanum, I determined to try the effects of belladonna.

At that time her pupils were contracted to mere points, her respiration was stertorous, ten per minute, the pulse was feeble, and the extremities rather cold. Between 9 and half-past 9, I gave her one ounce of tincture of belladonna in three ounces of water, which was all swallowed, but with difficulty. In the course of the next half hour two drachms more were administered. At 11 P. M. the first alteration on the size of the pupils was observed; the respirations had also then increased to twelve or thirteen in the minute, and the pulse was much stronger. The symptoms continued to improve till 2 A. M., when all indications of opium poisoning had disappeared. The woman was then sitting up in bed talking to the nurses, with pupils dilated to a little more than their natural size, and still slightly sensible to light. The extremities were quite warm, the pulse about one hundred, and of good strength.

She gave me a coherent account of her motives for taking the poison, of the amount of money she had spent in purchasing the laudanum, and the names of the druggists where it had been procured. She also replied sensibly to questions about her family, and the age and occupations of her children. She continued awake till nearly 4 A. M., after which she slept till 9 A. M. In the morning I found her pretty well,

her pupils being no more dilated than they were four hours after the first administration of the belladonna. She complained, however, of nausea, but unaccompanied with vomiting. This symptom, along with the dilated pupils, had entirely disappeared in the course of two days. She was kept in the hospital, under observation, for ten days after the accident, at the end of which time she was dismissed, perfectly well. The tincture of belladonna, used in both of these cases, was of the strength of four ounces of the leaves to two pints of rectified spirit, and prepared by percolation. Half a drachm is considered a full dose. I have seen dilatation of the pupil produced by a drachm given at once.

The committee on Dr. Anderson's communication reported, that they had designed to test Dr. Anderson's views by experiments on animals, but had found, on inquiry, that the animals commonly used for experiment were almost entirely insusceptible of the poisonous action of opium or belladonna. Where the effects were so different from those observed in man, it is obviously impossible to pursue the investigation which they had intended; the committee, therefore, while recognizing that Dr. Anderson's views require more extended observations in order to confirm them, deemed them worthy of the attentive consideration of the Society.

Iodide of Zinc in Venereal Affections. By Dr. SEWELL.—(Boston Med. Jour.)—Dr. S. C. Sewell, of Canada, writes as follows in the Montreal Medical Chronicle respecting the use of iodide of zinc in his practice:

"Its value in reducing enlargement of the tonsils is now pretty generally acknowledged by the profession, and I have used it in several cases with satisfactory results. I had a case of venereal ulceration of the throat which resisted the acid pernitrate of mercury. As an experiment I employed the iodide of zinc with immediate improvement, followed by a speedy cure. Since that I have used it in several cases of venereal ulcers, both of the throat and nose, with like success. Since 1837, I have used chiefly the acid pernitrate of mercury (Ricord's formula,) in such cases, and am, therefore, competent to form a comparative estimate of their value, and I give the preference to iodide of zinc. To apply it, twist a little cotton wool round the point of a wooden skewer, and having dipt it in the caustic, apply it to the ulcer. One application usually suffices to destroy the morbid action, and it is to be followed up with metallic washes for the throat, and dilute citrine ointment for the nose."

Employment of Glycerine in diseases of the skin, etc.—(Journal de Médecine et de Chirurgie Pratiques, Feb.)—M. Cap read notice on glycerine, which he asserted was serviceable in most affections of the skin. It penetrates easily into the pores. softens

various medicaments. It unites in all proportions with water used for baths, injections, fomentations, and lotions of every kind. Applied to burns and other wounds, it preserves them from exposure to air, and keeps their edges in a souple and moist state. When it is added to cataplasms, it keeps them in a moist state, and, which is important, it prevents their borders adhering to the receiving surfaces.

Glycerine can be added to oqueus and alcoholic fluids, being also incorporated with lard, ointments, pomades, and soaps. It can serve as a basis for liniments and embrocations. It can be united to extracts, tinctures, medicated wines, and thus can serve in almost all medicinal compounds and those employed in surgery, adding to each substance its soothing, sedative, softening properties upon the tissues and disposing them to the absorption of the medicinal substances to which it is united. This substance should, therefore, form the basis of a new class of remedies, to which we may give the name glycerole.

Iodine in Phthisis—Vapours of.—(Condensed from Journal de Médecine et de Chirug. Pratiques.)—M. Piory, pursuing the researches of M. Chartroule upon the effects of the inspiration of the vapours of iodine in phthisis, unites his testimony to its favourable influence. M. Chartroule, who followed his service at La Charité, proposed the giving it in the shape of cigarettes, as is done in the case of stramonium. M. Piory, following a different plan, places 2 drachms in a vessel with a wide mouth—the fumes are disengaged. When the tincture of iodine is employed it is made to vaporise by heat. One full inspiration at a time is practiced, repeating it from one to two hundred times a day, notwithstanding the irritation it induces. Even during sleep the patient is made to respire it by placing near his head three or four saucers containing each one gramme of iodine. The vaporization is often such as to colour the curtains.

M. P. in furnishing a resume of 32 cases of phthisis, presented drawings indicating the changes which occurred. It does not appear that all were treated with iodine; but the following figures were given: 4 died with or without positive amelioration: In 21 there was marked amelioration; and in 7 the anatomical character of phthisis had entirely disappeared with the accidents of the disease.

These observations have induced M. P. to believe that iodine is destined to render great service in the treatment of phthisis, even when the tubercles are softened and the existence of caverns is plainly determined.

Dry Cupping to the Abdomen for Nervous Colic.—(From the Gazette Medica Italiana Toscana.)—The Tuscan Journal of Medicine for Feb., quotes from the *Moniteur des Hospitaux* the following: The cholics to which we refer stimulate intense peritonitis, the pain is intolerable, the abdomen distended, the pulse unequal, small, nervous, and sometimes there is vomiting. This intestinal pain, however, is distinguished from peritonitis, not less by the instantaneous invasion of the pain, the slight sensibility of the stomach upon pressure, than by the general nervous symptoms. Under these conditions Dr. Sandras finds nothing so effectual as two or three dry cups to the abdomen.

The use of dry cups has also been proposed in cases of attacks of hysteria. Tisson reports two cases; one referred to a young man, aged 20, of rather nervous temperament, who was a prey to the most violent hysterical paroxysms, which sometimes lasted two hours. His whole body was agitated by violent convulsions; his teeth clinched; his face of a red violet and the veins of the neck swollen. His hands were continually directed towards his larynx and the upper portion of the chest, as if to tear away some body which suffocated him. There was abolition of the senses, and a small pulse sufficiently regular. After trying for some time the inspiration of ether, cold applications to the head, synapsims to the feet, Tisson applied two dry cups to the epigastric region. Suddenly there was experienced a general relief, the convulsions ceased and the senses returned. The eyes of the sick man were turned towards the stomach, where, without doubt, the cups produced a painful feeling, and he made signs of raising himself. Scarcely, however, had five minutes elapsed before the spasms returned with renewed vehemence. The cups were replaced and kept on for fifteen minutes. This destroyed the attack. The second case, that of a young girl frequently taken at the period of menstruation with hysterical attacks, was characterized by a loss of sensibility, accompanied with weight about the epigastric region, difficulty of swallowing and shortness of breath, which continued during the whole of the attack; the application of cups caused to cease immediately those which had usually lasted from four to five hours.

On Lacto-varolœic inoculation as a means of supplying the defect or inefficiency of Vaccine. By CLAUDE BOSSU, Ancient Surgeon Interne of Hospital at Lyons. (From *Revue Medico-Chirurgicale de Paris*, April, 1854. Condensed for *Charleston Medical Journal and Review*.) Notwithstanding the advantages of vaccine, yet small-pox appears sporadically, and sometimes epidemic. Dr. Roy, of the commission of the department of the Rhone, reports that of 209 cases of varioloid observed by different physicians, 57 died; that is more than one-fourth. To this we may add the sad consequences which often afflict those who have suffered; hence, one will easily recognize the importance of a good vaccination, and moreover, one will not fail to comprehend also, that it is desirous to find out some means of supplying, in case of urgency, any deficiency of good vaccine. It is this which has induced the present attempt to experiment upon innoculation. The necessity for this is also added to after observing the numerous cases of doubtful or negative results of vaccination.

Other observers, M. Theile for example, had suggested the mixing the matter of small-pox itself, with milk, under the supposition that it was the presence of this in the udder of the cow which modified the virus.

From another quarter, M. Bonsquet said, in a report made to the Academy, that M. Robert took the virus of varioloid, mixed it with a drop of milk, and inserted it into six children. Three had nothing but a local eruption; the other two had a general eruption, but so little regular, that M. Robert could not explain the anomaly, but by the power of the modifier. He then took the virus of variola, mixed it with milk, and made three innocations, two had nothing but a local eruption, the third presented a general eruption, (de bontons) which did not declare itself before the 8th day. The pustules were accumulated, which distinguished them from those of variola. Lastly, twice M. Robert innoculated a mixture of varioloid virus, of vaccine virus and milk. The experiment gave rise to a number of pustules numbering more than a hundred over the body, but all offering the character of those developed during the second series of experiments cited above. On this subject, M. Brochet mentions that as far back as 1832, he was charged with the cure of fever patients among females in wards of Hotel Dieu. Three of these were severely attacked. Moreover, they had each a young child at the breast, the eldest one year. Two days afterwards a young person was brought in, attacked with confluent small-pox. Neither of the three children had been vaccinated, and there was no vaccine. Fearing for them the small-pox infection, he thought of the variolic innoculation. As soon as the pustules were matured he proceeded to the operation. The idea respecting the probable influence of milk upon them struck him, and he mingled with a drop of one a drop of the other; and the innoculation with this was practised upon each arm of the three children. At the end of three days the punctures presented the pustular redness of vaccine, each developed itself absolutely as if it had come from vaccine arms; there was neither infection nor general eruption. He asks the question, if the children innoculated had been preserved from varioloid, and if the pustules developed were those of variola or of vaccination. Other patients were brought in infected with small-pox, which was communicated to others, but the children escaped.

M. Claude Bossu has himself made a number of experiments with the mixture of milk and the virus of variola.

Nature and Condition of the Mixture Innoculated.—This consisted of a proportion almost equal of the variolic virus and of milk, generally giving the predominance to the latter. He gives the preference to milk because, like virus, it is an animal secretion, and, like it, is of easy absorption. It is recommended to try also saliva, tears, the serum of blood, etc. The virus was taken from good pustules, isolated, large, vesicular and distended by an opaline serum; never at the moment when the matter begins to be discoloured and to become lactescent and when the pustule becomes umbilicated and dries. Variolous infants were selected who had never been vaccinated, and in whom the disease appeared to have been generated spontaneously and without contact. In one, the variola was confluent. A bit was taken from these and mingled on glass with nearly an equal quantity of milk. With this, six punctures were made in each arm and six pustules always resulted. The infants innoculated were generally quite young; 8 were from two to

three days, 6 from five to seven months, 4 from a year to eighteen months, 2 from three to four years, and 1 eleven years old. Of the twenty-one two had been before vaccinated with ordinary vaccine matter some months before, without apparent results. The new mixture employed produced good pustules. All the inoculations were not made with the lacto-variolic mixture, but the most part by transmission.

Five were, at different epochs, inoculated directly with the melange of virus of variola and milk. Three were vaccinated from arm to arm with the liquid contained in the pustules obtained by the first experiment—first transmission. Three were vaccinated from arm to arm with the liquid obtained from these new essays—second transmission. Four were vaccinated from arm to arm by means of the virus coming from this second indirect inoculation—third transmission; same success.

Three were vaccinated by means of the virus collected from the pustules produced by the first transmission and preserved for eight days. Two were vaccinated by means of the mixture preserved eleven days. Finally, one was vaccinated from arm to arm with the liquid borrowed from pustules arising after this last experiment. In all the cases the results have been thus far favourable. Let us speak of the local and general phenomena which this species of inoculation determined. Of the former, only two or three of the infants suffered from symptoms of any gravity. As to the general symptoms, few worth noting made their appearance before the fourth day; after this time some of the usual symptoms of irritation, etc., manifested themselves and began to diminish about the twelfth or fourteenth day. "All of these symptoms, however, far from disquieting us, seemed rather indexes of a good and happy inoculation."

On two different occasions, from eight to ten days after the disappearance of all the phenomena produced by the lacto-variolic inoculation, we have, in two infants, inoculated from arm to arm vaccine virus. To be sure of our experiment we each time took the virus from pustules which united all the good physical appearances of good vaccine; then we have each time and at the same moment, with the same lancet, inoculated with virus taken from the same pustule, two infants of almost the same age not having had either variola or been vaccinated. Each time, at the commencement of the fourth or fifth day, many rosy and sufficiently pronounced buttons made their appearance in the arm of the infants not previously vaccinated and ran their usual course. On the contrary, those who had submitted to the lacto-variolic inoculation had not the slightest sign of a mark and not even the slightest trace of inflammation.

Thus showing the preservative influence of the lacto-variolic mixture against the vaccine matter.

negative results confirmed our anticipations. Thus, then, the vaccine virus and the lacto-variolic virus neutralize themselves reciprocally, and can, consequently, be substituted.

From the experiments of M. Brochet and his own, the author draws the following conclusions:

1st. The influence of the lacto-variolic inoculation completely localized itself eighteen times out of twenty-one cases. 2nd. The general consecutive phenomena, although quite sensible, did not induce any grave alteration of the health. 3rd. The local appearance and phases of this species of inoculation are in every respect similar to those of vaccine virus. 4th. The fluid enclosed in the pustules resulting from the primitive inoculation is susceptible of successive transmissions from arm to arm. 5th. The fluid collected from pustules sprang of the inoculation direct or indirect, and preserved in well-stopped tubes can be kept for some time without injury to its virulent properties. 6th. The lacto-variolic melange and the virus of variola can enjoy the same advantage. 7th. The lacto-variolic inoculation preserves, very probably, from variola, since it preserves from vaccine, and reciprocally the vaccine preserves from the inoculation lacto-variolic. 8th. It is advisable to get the virus to inoculate from a case of discrete small-pox occurring in an infant of good constitution and without affections of the skin. 9th. It is at the moment when the pustules of variola are still globular and semi-transparent that it is more suitable to take the liquid, which they enclose in abundance. 10th. The inoculation seems to take place most favourable in infants under one year, particularly in those under six months.

On the Connection of Rheumatism with Scarlatina. By J. H. HAYDON, Esq.) North Western Med. and Surg. Jour. from London Lancet.) I have been much struck with the abstract of a paper lately read by Dr. Willshire before the Medical Society of London, "On some Points in the Pathology of Rheumatism in Children." The part most interesting to myself is that on the connection of rheumatism with scarlatina. I can hardly coincide with the views of Betz, and declare for the actual identity of scarlatina and rheumatism; but I admit there is a most remarkable analogy between the phenomena, especially the secondary phenomena of these affections. In the year 1850 and 1851, I attended several hundred cases of scarlatina. From May to September, (1850,) in the village of Hennock, (containing, with two small contiguous hamlets, about 350 souls,) I attended 135 cases of the disorder. I had the opportunity of seeing every case, save one, of watching every case, and noting their symptoms. I had never heard of the views of Betz, or of the supposed identity of the two diseases. My own views on the matter were first directed to the close analogy of the two affections from the following circumstance:—Three children were ill in a family. On one of my visits the mother complained of great fatigue, as the whole care of attending these three children had fallen on her, from an elder daughter having been suddenly attacked with rheumatism. I saw the girl, and she had all the phenomena of acute rheumatism, together with fulness around the eyes, loss of colour and pain in the back; she had no rash, sore-throat, or other usual sign of

scarlatina; she had been well up to the night before; she now had albuminuria. I ordered for her a smart calomel and jalap purge, and an ounce and a half of lemon juice three times a day. In three days she was convalescent, and in three more every trace of albuminuria had subsided. I imagine this case to have been one of scarlatina, without eruption, and the first manifest symptoms to have been that arising from disease of the kidney, producing albuminuria. This state of the kidney was very general during the epidemic. I had under my own care more than forty cases of severe albuminuria during the two years.

Another remarkable analogy between rheumatism and albuminuria will be found in the great tendency of the blood to lose its colouring matter. In a fine boy of ten years old, suffering severely from albuminuria, I took blood from the arm; it was remarkably poor in colouring matter, as also in crassamentum, while the fluid part had more the appearance of milk whey than any thing else. This boy recovered very fast after the bleeding, only requiring an occasional purgative. I had occasion to bleed three others, all strong boys from ten to twelve years old. The same deficiency of colouring matter and crassamentum was found in each, though not so much of the whey-like appearance in the serum. Those three also recovered. I had previously, in the three first cases of albuminuria that presented themselves, used warm baths, leeches to the loins, blisters and saline purgatives, on the plan so strongly recommended by the late Dr. Miller; they died. Among the three children ill in the first house I have alluded to, two were afflicted with albuminuria. Seeing the good effect of the lemon juice with the elder sister, I gave it to these children, and it succeeded admirably. I have since given it to more than thirty cases of scarlatina albuminuria, and in only one has it failed, and this case failed more from general neglect than any thing else.

At the village I reside in there were about 1100 persons. In 1851 we had scarlet fever there as an epidemic, when I had fifteen cases of acute inflammatory rheumatism under my care. I could only trace the diseases as an open sequela to scarlet fever in one, and this case was very remarkable. A married woman, thirty-two years old, had a slight attack of scarlatina. On the rash disappearing she was seized with acute arthritis—I think the most acute attack I ever saw; there was albuminuria. She recovered. I am too old to trust to specifics, but I have very rarely seen an acute arthritis, or an acute attack of scarlatinal albuminuria, that has not been very speedily relieved by lemon juice and an active purgative; it was so with this woman.

If the views of Retz are correct, perhaps we may arrive at some

ten hours from the first attack, the usual rash being thickly spread over its skin before death. The child had never shown disease. I could not obtain a post mortem examination; but we may expect meningitis to have ensued. Now what was the cause of the latter? Was it meningeal rheumatism? It is said never to occur. I witnessed three cases of death at Hennock in the early stages of scarlatina, each within the fifth day of the eruption, from acute pleuro-pneumonia, with effusion; yet text-book writers say this state of things never happens, except as a symptom of scarlatina maligna. I have never seen meningitis as one of the sequelæ of the disease. There is one characteristic of scarlatina, viz: the extreme frequency of ulceration, particularly of the posterior nares, tonsils and fauces which so rapidly supervenes on the first inflammatory symptoms which serve to mark it as different from rheumatism—for rheumatism, however acute, is certainly not very prone to that state of things (at least in the country, which ends in ulceration; yet there is a strong analogy between the way the inflammatory action of the tonsils may be in many cases controlled, and the way in which we control rheumatism when it has fixed on some particular part, e. g., by the external application of stimulants. In the inflammatory sore-throat of scarlet fever, I have repeatedly applied a liniment (on hot flannel) of fourteen parts of turpentine, one and a half of spirit of camphor, and one and a half of tincture of cantharides; it produces the most intense irritation. I have seldom seen it fail of giving immediate relief, and checking the ulceration process, and rendering more certain the effect of the local application of the nitrate of silver. We have all seen a mustard plaster, or other stimulating applications, relieve a joint afflicted with rheumatic inflammation. We have seen, also, celebrate rheumatism quickly relieved by mustard to the feet and legs, especially if at the same time we have exhibited an opiate. I know we are told that an opiate would do alone, but I have found it fail more than once, though if not contra-indicated by other symptoms, I have always resorted to it in conjunction with counter-stimulants. A friend of my own, a young medical man, while a pupil in London, had scarlatina very slightly, but it was followed by a most acute attack of rheumatic fever, with pericarditis, and there was at the same time great functional disturbance of the brain. He is now well and in practice.

A young man whom I daily see had scarlet fever slightly, seventeen years since. It was followed by acute arthritis, from which he recovered, but with an ankylosed knee. So closely do I consider scarlatina and rheumatism to be related, that I make it a rule on the decline of the rash, to administer lemon juice; and since I have followed this plan I have lost one single case from the sequelæ of that disease. To Dr. Owen Rees I feel greatly obliged for having so prominently directed the attention of the profession to the use of lemon juice in certain forms of rheumatism, and I feel very sanguine that it will not fail in many of the affections that are considered as secondary to scarlatina, but which, in fact, I believe, will be found to depend on a diseased state of the blood, very analogous to the state of the blood in rheumatism. It appears to me to be a very uncertain point if the state of the kidneys is not reduced as a part and parcel of scarlatinal fever. I rather regard

it as a secondary affection, induced by the diseased condition of the blood.

Remarks on Croup and its Treatment. By Dr. HORACE GREEN. (Am. Med. Monthly.) In 1848, the writer published a small treatise, "On the Pathology of Croup and its Treatment by Topical Medications," in which the declaration was made that, "the practice of making topical application of medicinal agents into the larynxes of young children, for the treatment of membranous croup, is a plan entirely practicable, safe, and, when judiciously employed, *in the highest degree efficacious.*" This method of treating a disease hitherto so unmanageable, was founded upon the following propositions which were then advanced, with regard to the pathology of the disease, namely:—That the essential characteristics of true croup "consist in an inflammation of the secreting surfaces of the fauces, larynx and trachea, which is always productive of a membranaceous or an albuminous exudation.

2. That the membranaceous concretion, which is found coating the inflamed mucous surface of the parts in croup, is an exudation—not from the membrane itself, but is secreted by the muciferous glands, which so abundantly stud the larynx and trachea.

3. "That the exudative inflammation commences, invariably, in the superior portion of the respiratory passages, and extends from above downwards—never in the opposite direction."

Since the publication of the work in which this mode of treatment is advocated the author has had the opportunity of treating many cases of croup on the plan deduced from this view of its pathology, viz.: by means of topical medication—not only in his own practice, but in the practice of, and in conjunction with, other members of the medical profession; and with an amount of success that has afforded a high degree, of encouragement and satisfaction.

He has also received from medical men, in different parts of the United States, as well as from numbers in Europe, the history of many cases of membranous croup, wherein topical measures, in their hands, have proved effectual in arresting the disease. In view of the great fatality, on the one hand, which constantly attends this disease, as ordinarily treated, and on the other, of the prejudice against the local treatment, which is still entertained by many, especially of the older members of the profession, the author does not feel at liberty to withhold from his professional brethren this abundant and most conclusive testimony in favour of topical medication in the treatment of croup.

It will not avail, for the cavilling opposer of this method of treating

with the assertion, that the cases of croup which have recovered rapidly under local treatment "were not cases of true membranous croup, but those of a spasmodic or catarrhal nature, such as would have recovered under almost any treatment;" for, in many instances, the employment of nitrate of silver, in the treatment of croup, has been adopted with great success by eminent practitioners, who had previously had extensive experience in the management of the disease, but who, before the employment of topical medication, had treated, unsuccessfully, a large number of cases by the ordinary "depressing, depleting and disturbing remedies." This was the case, as with many others to whom we could refer, with the distinguished Dr. John Ware, of Boston, to whose experience, in the different modes of treating this disease, and the conclusions to which he has arrived, we shall now briefly allude.

It is well known to the reading members of the profession, that several years ago Dr. Ware published his "Contributions to the History and Diagnosis of Croup"—a work evincing more scientific research, and containing more information with regard to the true pathology of membranous croup, than all that had been previously written in America. In these papers, Dr. Ware refers to thirty-nine cases of what he denominates membranous croup, which were noticed in his own, or in the practice of his friends. Of these cases the state of the fauces was observed in thirty-three instances, and "in thirty-two a false membrane was present; most frequently, and sometimes only, on the tonsils, sometimes on other parts also, as the palate, uvula and pharynx. In one case no such membrane was present; but it was found to exist in the laryn after death. These thirty-three cases were treated by the ordinary therapeutic measures; and of the whole number, *three* only recovered—in thirty, the disease proved fatal. It is not at all surprising that, under these circumstances, Dr. Ware, eminent for his careful investigation and conscientious inquiry after truth, should have become "confirmed in the opinion," as he subsequently declares himself to have been, "that the methods of treating this disease, in common use, require a careful reconsideration; nor that he should have propounded the question—"If the mode of treating croup commonly adopted does no good, are we sure that it does no hurt?"

Having concluded, after the experience to which we have referred, to treat the disease "without the persevering use of the heroic remedies," Dr. Ware subsequently adopted a method in which the treatment consisted—

1. "In the absence of all reducing, depleting and disturbing remedies.
2. "Keeping the patient under the full influence of opium combined with calomel.
3. "Constant external application of warmth and moisture [to the neck], and of mercurial liniment, slightly stimulating.
4. "Constant inhalation of watery vapor."

measure. The physician however, persisted in his proposal, and the surgeon retired. Cauterization of the larynx was then promptly and perseveringly employed, and by this means the life of the child was saved.

In March, 1850, Dr. Ware read before the Suffolk District Medical Society "Additional Remarks on the Treatment of Croup," in which paper he refers to five cases of membranous croup, three of which were treated on the method indicated in the preceding propositions. The history of these five cases, as briefly narrated by Dr. Ware, with the conclusions to which he has arrived, we shall take the liberty of giving, in his own words.

"The first case was that of a male, four years old, who was taken with membranous sore-throat, accompanied by high constitutional irritation, Oct. 14, 1845. No croupy symptoms occurred till Oct. 18, when they were manifested in a perfectly distinct manner. On the 20th and 21st, patches of false membrane, with bloody sputa, were raised—and one piece of four inches in length. The raising of the latter was accompanied by a severe and suffocative paroxysm of coughing. On the 22d he died, eight days from the commencement of the disease and four from the access of croup. The suffering in this case was very considerable, but far less than I have been accustomed to witness in cases of croup treated according to the ordinary method."

The second was that of a female, four years of age, taken with croup on the 8th of Nov., 1845. No depleting or reducing remedies were employed. Patches of membrane, and one piece of considerable size, were brought up on the 10th, and a few following days. She never suffered much, improved steadily, and on the 15th seemed well in all respects except the voice, so that on the 16th I did not see her. On the 17th there was a return of all the croupy symptoms, including the appearance of lymph upon the tonsils, and she died on the night of the 19th, eleven days after her first seizure. During no part of the disease was the suffering from dyspnoea very intense for any continued period.

"On dissection, the usual appearances were found, and in one lung the false membrane extended for some distance into the bronchi in the substance of the organ."

"The third was a female, six years of age, who was seized with the disease, Oct. 31, 1847. The onset of the disease was gradual, yet quite distinct. Nov. 2d, the symptoms had become quite severe; and Nov. 3d, there was bloody expectoration, and pieces of membrane were spit up. Pieces of membrane continued to be found in the sputa for several days, and she was very comfortable and breathed with tolerable ease, yet never losing the distinct croupy sound of respiration and voice. She retained some appetite and sat up and amused herself as usual. On the 8th, she became rapidly worse, but without distress, and died on the 9th, quite easily, ten days from the first attack of the disease.

"It will be admitted, I think, that these cases, especially the two last, exhibited certain differences from the common course of this disease, which indicated a favourable influence from difference of treatment.

"In all of them the membrane was thrown up in considerable quantities.

"In all of them the disease was attended by very much less distress than is usual in croup, and, in two, there was so decided a mitigation of symptoms following the separation of the membrane, as to lead to considerable hope of a favourable termination.

"In two, at least, the disease was prolonged to at least twice its average duration under the usual treatment.

"In the two other cases, to which reference was made, the same general course of treatment was followed, with the addition of the introduction of a sponge, wet with a solution of the nitrate of silver, into the larynx. In each of these cases the application was made as early in the disease as I became satisfied of its distinct character. It was repeated morning and evening. It decidedly gave relief to the breathing, soon after each application, and both cases ultimately recovered perfectly. For the suggestion and adoption of this valuable addition to our means of treating this formidable disease, we are indebted, as is well known, to the enterprise of Dr. Horace Green, of New-York. The profession, I think, owe to him a large debt of gratitude, for the energy and perseverance manifested in the introduction of this remedy, and I am the more disposed to render this tribute to him, because so many attempts have been made to detract from his merit in relation to it.

"I am well satisfied from what I have now seen of this method of treating croup, as compared with that which has been followed for so many years, that it has the advantages which were pointed out in one of the preceding papers. It is a disease which I would treat without depletion—except, perhaps, by a few leeches—without vomiting, without purging, without blisters, without antimonials, ipecac., and all those other nauseous remedies which have been usually resorted to. I would trust to opiates, perhaps calomel, emollients, and the local application of the nitrate of silver.

"I ought to add that many of my friends in the profession have informed me of cases in their practice, treated on these principles, which have recovered in a favourable manner."*

Since the publication of Dr. Ware's papers, cauterization of the larynx, in the treatment of membranous croup, has been adopted by large numbers of medical men in New England, from many of whom we have received communications on this subject, expressing their full confidence in this therapeutic agent, when timely and appropriately employed in the management of croup.

Should we give the history of a tithe of these cases, which have been thus reported to us, they would occupy a much larger space than can be appropriated to this subject in the pages of the Monthly. In many instances—and this is the testimony of large numbers of practitioners, experienced in the disease—the morbid process has been promptly arrested, by topical medication to the surface of the tonsils and pharynx without the introduction of the sponge-probang into the larynx.

If the proposition, with regard to the pathology of the disease, be admitted, namely:—that the exudative inflammation in croup commences invariably, as a general rule, about the fauces and upper portion of the respiratory tubes, and extends from above downward, it must be apparent that no remedy can prove so effectual in arresting the morbid process as cauterization.

That this is the true pathology of the disease has been fully estab-

* *Boston Medical and Surgical Journal.* Vol. xlii., pp. 297, 268.

lished, not only by many impartial observers, but also by the success which has attended the practice founded upon this view of its nature : it is, moreover, so declared to be by some of the most eminent and experienced pathologists of the present day.

"In true croup," says Rokitsansky, "which is essentially a disease of childhood, the exudative process often affects the throat and pharynx, and it extends from the epiglottis through the larynx and trachea—in some instances to the minute ramifications of the bronchial tubes—but the points it most commonly attacks are the larynx and trachea."^{*}

Prof. Hasse, also, whose late work on Pathological Anatomy has been translated and published by the London Sydenham Society, observes, with regard to the extraordinary inflammation of croup, that its progress is invariably from above downwards, and that it never spreads in the opposite direction. "This law is so universal, that where plastic inflammation occurs in the bronchi of the adult, as the concomitant of pneumonia, it can only descend to the pulmonary cells, never mount to the larynx."[†]

Still farther to sustain these views, not only of the nature of the disease, but of the positive efficacy of topical treatment in every stage of membranous croup, we shall proceed to adduce further testimony on these most important points.

In a recent number of the "*Archives Générales de Médecine*," is an interesting memoir by M. Vouthier, on the history of croup, as it occurred in an epidemic form in *L'Hôpital des Enfants Malades de Paris*.

In this paper are the details of several well-marked cases of membranous croup, which were treated successfully by "emetics and cauterizations;" and although in these instances the argentine solution was not conveyed into the larynx, but was applied only to the fauces and pharynx, yet the patients recovered perfectly under the treatment.‡ Although the cases thus treated are characterized as having been very severe—"très-intense"—yet, as the treatment was early adopted, it is probable that the exudative process had not extended into the larynx; for, in the same paper is a history given, of five other cases of membranous croup, in which the disease, having reached the larynx, was not arrested by cauterizations. This measure was employed, as in the other cases, but no attempt was made to pass the instrument below the epiglottis. Tracheotomy, however, was resorted to in all these five cases, but every patient died. Efficient cauterization of the larynx, we maintain, would have saved three, if not more of these last cases.

A few weeks ago a physician, from the interior of one of our South-western States, called on us to state his experience in the treatment of croup by local measures. Two or three years before he had passed several weeks in our city, and had then seen, for the first time, the em-

^{*} A Manual of Pathological Anatomy. By Karl Rokitsansky, M. D. Sydenham Edition, vol. IV., pp. 20, 21.

[†] An Anatomical Description of the Diseases of the Organs of Circulation and Respiration. Sydenham So. Edition, p. 276.

[‡] Archives Générales de Médecine. Tome xix., art. 1st.

ployment of topical medication for the treatment of laryngeal and bronchial diseases. Returning to his home in the West—a region noted for the frequency with which croup occurs among children—he commenced at once to put into practice the new method of treating diseases, with which he had recently become acquainted. During his previous practice, he had treated many cases of croup in the ordinary method, and the proportion of deaths, in his experience as he stated, had been quite equal to one third of the whole number attacked. But since the adoption of topical medication, during the two or three years after his return, some fifteen cases, he affirmed, had come under his observation, and were treated by cauterization of the fauces and larynx, *every one of which recovered*. The plan adopted by this physician was the same as that to which we shall briefly refer, at the close of this paper.

As before asserted, since the publication of our work on croup, we have received similar verbal statements, in favor of topical medication, from physicians resident in almost every State in the Union; but we have also abundant *written* testimony on this subject.

The following cases from Prof. May, of Washington, who is one of the most distinguished of American surgeons, will be read with much interest.

WASHINGTON, April 30th.

My Dear Sir:

I herewith enclose you a hasty and brief account of the cases of cynanche trachealis, in which I have used the strong solution of nitrate of silver, as recommended by you in your work on croup. The statement of these cases is not as full and satisfactory as I could have desired, owing to the partial notes hastily taken by me at the time of their occurrence; but the result in several of them, fully sustains the great value of the local agent recommended by you, to arrest this formidable disease, and which you have been, I believe, the first to bring fully to the consideration of the profession.

I am very truly and respectfully yours,

JNO. FRED'K MAY.

Case 1st.—On the 2d Jan., I was called on to visit a son of Mr. C., of this city, aged about ten years, whom I found labouring under symptoms of croup. The child had been complaining of some uneasiness about the throat, with hoarseness and slight cough, a day or two before I saw him. At my visit, his respiration was difficult, and there was a good deal of tenderness about the tonsils and fauces, which were inflamed. There was considerable febrile action, and the little patient was very restless, and the voice quite hoarse. I directed an emetic, to be followed by an active mercurial cathartic, his bowels being constipated, and such local applications, both internal and external, as I thought most likely to give relief.

At my visit next day, I found my little patient no better, but, on the contrary, his symptoms were more unfavourable and alarming. His respiration was very difficult, and his countenance expressed great suffering and anxiety. He could not remain more than a few moments in a horizontal position, but was raising himself up continually, and stretch-

ing backwards the head to obtain relief. In a word, the difficulty of respiration was extreme. He was very hoarse, his pulse very hurried, and the throat and tonsils more inflamed than at my previous visit. The symptoms of diphtheritic croup, which had been epidemic in our city the year previous, were very decided. I at once resolved to try your method of introducing a strong solution of argent nitrat. into the larynx—everything else having been tried by me, in this disease the year previous, not excepting tracheotomy, with but little success. The symptoms were very urgent. I felt satisfied that, unless soon relieved, the case would probably end fatally. I had no instrument sufficiently small for the object I had in view, and I, therefore, bent a small piece of ash stick to the proper curvature, and secured to the end of it a small piece of sponge, which I saturated thoroughly with a solution of the crystalized nitrate, of the strength of forty grs. to the ounce, and introduced it fairly into the larynx. This I repeated once or twice, on the spot, the child being forcibly held by the father. There was considerable irritation produced in the throat for a few moments, but I believe it was caused as much by the effort on the part of the child to resist, as by the action of the caustic.

I remained some time after using this remedy, and in half an hour the improvement in his breathing was manifest to all present. I left him breathing decidedly better, and when I returned in the morning I found the child had passed a quiet and comfortable night, having slept a considerable portion of it. The caustic solution was again used by me in the same way, at this visit, his breathing having again become somewhat more difficult, and with the same improvement in his condition. My visits to him were soon after discontinued, the little patient having entirely recovered.

Case 2d.—I was requested in May, by my friend, Dr. Dawes, to visit, in consultation with him in the country, a little girl of two years of age, the daughter of R. S. Wood, Esq.

Dr. D. had been in attendance on the child for several days, and had administered the usual remedies in her disease, which was membranous croup. Having mentioned to him, some time before, the success of the argent-nitrat. solution, in the case which I have already stated, he was desirous of having it used in the present instance.

When I saw the child the breathing was extremely difficult, and the cough tight and ringing. The little patient was much exhausted, having slept but little during the twenty-four hours which had passed. It was constantly held by the mother in her lap, or by some of her friends, as the respiration became more laboured when she was placed in the bed. I at once used the solution of crystalized nitrate of silver, introducing it into the larynx, and cauterizing also the back part of the throat freely. The solution was of the strength of fifty grs. to the ounce.

the disease gradually yielded, and the child recovered. As I was not aware, or had forgotten the treatment which had been pursued in the commencement of her case, I applied to Mr. Wood, who had formerly pursued the study of medicine, for a statement of it. I received the following reply :

My daughter, about two years old, was attacked with croup in the month of May last. She had never suffered previously from any disease, and her constitution was good. After trying the usual domestic remedies, such as hot baths, emetics, local applications, and small doses of calomel and ipecac. frequently repeated, we were induced to send for our family physician, Dr. Dawes, who prescribed mercury to the full extent, warranted by her critical situation. On the third day, the doctor proposed consulting with you, in regard to the propriety of introducing a strong solution of the nitrate of silver into the larynx. I readily gave my consent, and the operation was performed twice within twelve hours. The character of the respiration was perceptibly changed on the first trial, but we felt doubly sure of success on its repetition ; the pulse rallied, and the colour of the skin became more natural.

"She is now enjoying perfect health."

Other cases have been treated by Dr. May with equal success, the history of which have been furnished by him ; but as our object has been to give the testimony of different members of the profession from various localities, these may with propriety be omitted.

Dr. A. M. Vedder, Lecturer on Anatomy and Physiology at Union College, has treated many cases of membranous croup by topical measures. The subjoined cases were communicated by Dr. V.

Even at the present day there are medical men who will not admit that the passage of an armed probang into the larynx of the adult can be accomplished. Much more emphatically do these men deny the possibility of cauterizing the larynx of the young child. Within the last year an astute professor in one of our medical colleges declared, before his class, his firm belief that the operation had not been, and never could be, performed ! To such "blind guides," we would commend the interesting fact recorded in the second case reported by Vedder.

Case 1st.—A little girl, aged six years, was under treatment by a neighbouring practitioner, who called me in consultation. She was first taken with what we supposed to be slight catarrhal symptoms ; after a few days the cough became stridulous, and was accompanied with some fever. These symptoms continued for three weeks, gradually increasing in severity. At this time I saw her, in the evening, and found her labouring under the characteristic symptoms of inflammatory croup ; pulse rapid and small ; voice reduced to a whisper ; respiration high and laboured ; tonsils red and inflamed, but not covered with false membrane. The attending physician had treated her with sinapisms, calomel, and tartar emetic, the latter in emetic and nauseating doses.

I suggested the topical application of nitrate of silver, which as readily acceded to. Two applications were made (℞ ii. to ℥j. aqua). The probang was found covered with a thick, tenacious secretion. Re-

aspiration became somewhat more free. On the following day there was a marked increase of all the symptoms, the silver was again applied three or four times during the day, entering the glottis each time. There was now a gradual improvement of all the symptoms, and no further applications were made. During this time, calomel and James' powder were also administered.

Case 2d.—Louisa —, aged six years, general health previously good, came home from school (Nov. 1st) complaining of sore throat and cough, which was followed by vomiting. A homœopathic doctor was sent for, who treated the case as "sore throat" for five days; during this time, she was not wholly confined to the bed, and was about the house a part of the time. On the afternoon of the fourth day she became very hoarse, with loss of voice and decided croupy cough. Nov. 6th, confined to bed, with considerable heat of skin and thirst. I saw the patient this day, for the first time, at 6 o'clock P. M. Expression of countenance anxious, skin pale, voice reduced to a whisper, respiration extremely difficult, high and characteristic, pulse frequent, skin above natural temperature, cough frequent; applied the nitrate of silver with the probang, which did not produce any unpleasant symptoms, her breathing became somewhat easier; during the night her respiration became more difficult, and an emetic was administered, which was followed by some relief. Nov. 7th, A. M., countenance still anxious, colour of skin inclining to blue, respiration not much improved, almost complete aphonia—prescribed the following powder, to be taken every three hours:

R. Tart. Antim., gr. $\frac{1}{8}$,
Hyd. Chlor., M. gr. $\frac{1}{4}$,
m.

Applied the silver three times during the day. Nov. 8th, no improvement, sweats now freely, and has done so all night—on coughing expectorates a little mucus, particularly after applying the sponge, continue powders and apply cold water to the neck by means of a towel. Nov. 9th, no improvement; applied the sponge, and on removing it the *false membrane* was found attached to the sponge; and on examination found it to be a membranous tube two and a half inches in length, and about one-half the thickness of milliner's pasteboard. Her respiration became immediately easier and she continued to improve from this time, the sponge was not again applied, her cough remained "croupy" for several days longer, her voice did not become natural for more than a week after, the cold water and expectorants were continued for several days. Her health has been good since: her voice becomes hoarse, occasionally, on taking cold. I should have remarked, that at my first visit, I saw patches of lymph on the tonsillary glands. On taking charge of the patient (which I did with great reluctance), I had very little hope of a favourable termination, on account of the advanced stage of the disease, and must attribute the cure to the application of the silver.

Since treating this case I have used the silver with varying success. In two cases, in which I was called early, and in which the lymph could be seen in the upper part of the fauces, the patients recovered. In one

case where I was called in consultation, the patient was in *articulo mortis*. We used the silver without any relief. In two other cases, I was called late in the disease; the nitrate of silver, and other remedies were employed, but both cases terminated fatally.

Believing, as we conscientiously do, that the reducing, perturbing plan of treating young children for croup, is, in a large proportion of cases, not only useless, but worse than useless; and that, on the other hand, we have, in the topical treatment, when judiciously combined with mild general measures, an entirely practical, and, in most cases, an effectual means of arresting the disease, we are solicitous to remove the objections which, for various reasons, exist in the minds of many practitioners, and hinder their adoption of this method. We have alluded to the dogmatic scepticism of one class, but fortunately this class of "unbelievers in the earth's rotation" is very small. Still there is a much larger class who, whilst they admit both the practicability and utility of the treatment, reject its employment because of the supposed difficulty of medicating the larynx in disease. This difficulty, we can assure the profession, does not exist. It is neither really difficult to accomplish, nor is it in any degree a hazardous operation. Any medical man who understands the relative anatomy of the parts, can, with very little practice, readily pass the sponge probang into the larynx; and, as benefit comes from the attempt (for the parts cauterized thereby are those first affected in croup), no time should be lost in putting the method into practice, in the onset of the disease.

During the prevalence of the disorder, last winter, when the writer was receiving calls, daily, to visit cases of croup, a request came from a physician residing in Morrisania, to meet him in consultation in a case of membranous croup. As the writer could not comply with the request, his assistant, Dr. Richards, attended the consultation; and he testifies to the fact that the case was one exhibiting all the characteristic signs of true croup, of a very severe grade. The physician, Dr. Mann, who is an accomplished and experienced practitioner, had had many opportunities of treating croup by the ordinary method; but in this case, which we shall give, he employed, for the first time, we believe, cauterization of the larynx; and it will be seen that in his hands the operation was both practicable and successful. So much pleased was Dr. M. with its effects that he has since placed his entire dependence, as will be seen, on topical medication alone in the treatment of the disease.

The following communication has just been received from Dr. Mann:
MORRISANIA, May 12th, 1854.

Doct. Horace Green:

Dear Sir—The enclosed list of cases of *cynanche trachealis* I send agreeably to your request. In none of them were any internal remedies used, not even a preparatory emetic, except—where circumstances required it—a mild cathartic. With this exception, they were all treated by topical applications exclusively.

Case 1st.—On the 7th of February last I was sent for, at 4 o'clock A. M., to attend Alfred —, aged four years. Circumstances pre-

vented my seeing him until 8 A. M. I then found him labouring under true membranous croup. The attack commenced on the evening of the 6th, preceded by hoarseness and slight cough. I had previously attended the patient on Jan. 8th, for pneumonia of the left lung, from which he had recovered perfectly, though still somewhat weakened by this illness. I found the little fellow struggling for breath, each effort at inspiration accompanied by a loud crowing sound, the muscles of the mouth contracting violently at the same time, the countenance and lips livid, a cold perspiration standing in large drops upon the face, and every symptom indicating rapidly-approaching dissolution. I immediately passed through the larynx and nearly down to the bifurcation of the trachea, a sponge-armed probang containing one drachm of a sol. argent. nit. xl. grs. to $\frac{3}{4}$ j. The first effect of this application was a severe fit of coughing and choking, followed by copious vomiting, which relieved the respiration temporarily. Between this time, viz., 8 A. M. and 7 P. M., five applications were made; at about the latter hour Dr. Richards saw the patient with me. We found him sitting up, perfectly conscious and able to speak, and had taken some beef tea. Respiration entirely free from any crowing sound, which could now only be heard during a fit of coughing. Skin moist. Feb. 8th. Continues still improving; respiration natural; slept at intervals during the night; appetite craving; cough troublesome; slight fever; face flushed. Feb. 9th. No symptoms of croup remain; slight bronchitis, which yielded in a few days to mild remedies.

Case 2nd.—On Feb. 11th I was called to see Sarah —, aged two years. The patient had been suffering for two days previously with hoarseness, and what the mother called a “sore throat,” accompanied by a loud ringing cough. On examination of the larynx and fauces, I found them considerably inflamed. The tonsils were enlarged: and, adhering to the posterior surface of these glands, I discovered a white substance lining them, and extending downward toward the larynx. The symptoms of true membranous croup were strongly marked. Skin hot and dry; face flushed; countenance expressive of anxiety and suffering; cough and respiration characteristic. I immediately made an application by means of the sponge probang to the fauces and larynx, of a sol. argent. nit. grs. lx. to $\frac{3}{4}$ j., then passed through the rima glottidis.

Feb. 12th. Patient playing about the room; respiration natural; cough troublesome, and accompanied by a free secretion of thick, ropy mucus, which the patient, on raising, immediately swallows. Detached the white substance from one tonsil, and found it to be a firm membranous exudation. This child had one other application of a weak solution, and was perfectly recovered on the third day after the first visit.

summoned to the sufferer. Applied a sixty-grain sol. at once, 7 A. M. At 4 P. M. found the respiration much relieved, the patient being better in every respect. Applied a thirty-grain sol.

Feb. 26th. The little fellow saw me tying my horse, and fearing, as he expressed it, that I was going to "run the poker down his throat again," scampered off and hid.

The above cases it gives me pleasure to submit to your perusal. I should have regarded any one of them as nearly hopeless under the old plan of treatment.

Very respectfully yours,

F. P. MANN.

Dr. HORACE GREEN, 12 Clinton Place.

With the history of one more case, voluntarily furnished by Dr. M., an intelligent physician of a neighbouring State, we shall close our selections from the mass of testimony that has been received, in favour of the plan of treatment advocated in these pages.

DEAR SIR:—

You will excuse the liberty which I assume in thus addressing you. I cannot forego the opportunity I have in expressing to you the gratitude I now feel for your kindness to me, and for the information I acquired in the short time I passed in your office, during my late visit to your city. So elated do I feel with the result of your mode of treating diseases of the air-passages, that I must trouble you to read an account of my first experience in the use of topical applications, in an interesting case of croup.

Feb. 18th, at 8 P. M., a gentleman entered my office, in some haste, and requested me to step across the street and see his child, which he feared was dying. Without ceremony or question, I accompanied him home, where I found two physicians, with some dozen, more or less, neighbours, surrounding the bed of a little fellow, six years of age, the sound of whose inspiration and expiration was to me a sure index of the nature of his disease. In the diagnosis we could not be mistaken. He was in the very last stage of membranous croup. The patient was of a bilious temperament, black hair and eyes, dark complexion, very large head, with a short and very thick neck, broad chest, and, for a boy of his age, very muscular. It was with the utmost difficulty that he could breathe, every muscle of the body seemed brought into action at each respiration; the countenance was flushed and anxious, the lips pale and swollen, the eyes protruded, nostrils dilated, and ever and anon that cough, the sound of which you know, but which I cannot describe; pulse beyond enumeration. All these were sufficient to cause the friends and physicians, as well as myself, to believe the child to be in *articulo mortis*. I, however, proposed cauterization with the probang; the physi-

results that might reasonably be anticipated; nor did I keep back the fact, that the child, notwithstanding, might die.

The friends immediately consented to my doing what I pleased, as the child would die without immediate relief. All the usual remedies, such as emetics, leeches, blisters, &c., had been used. I immediately made a sol. of argent. nitrat. cryst. fifty grs. to the ounce of aqua. distil., saturated the sponge, and made the attempt to pass it into the larynx; the child struggled and I did not succeed, but what was very good, I did succeed in sponging the fauces and epiglottis. My failure, however, to pass into the larynx, was a sort of triumph to the non-believers. However, the child strangled, coughed, and discharged considerable ropy mucus. After a delay of some fifteen or twenty minutes, I secured the head of the child, and made the attempt again; and what is *better*, I *succeeded well*. The little fellow strangled, coughed, and discharged a large quantity of thick ropy mucus, with patches or shreds of membrane, and in one minute was sensibly relieved. In ten minutes, the wheezing sound, in expiration, was gone. Then was my hour of triumph, and I made use of it. After waiting a half hour, I prescribed hyd. sub. mur., gr. i.; ipecac., gr. i.; opii., gr. $\frac{1}{2}$, every hour, and left for home.

It will be unnecessary to follow out the farther history of the treatment in this case, as detailed by Dr. M——. Topical, with the required general treatment, was continued for several days, and the patient recovered perfectly.

The history of these last cases have been given, not only as corroborative of the efficacy of the treatment, but to show what has proved true in many other instances, that where intelligent medical men have made the attempt, earnestly, to medicate the larynx, their efforts have, in all cases, been successful.

It was our intention at the close of this paper to have submitted a detailed statement of the plan, both local and general, that we would advise to be pursued in the treatment of membranous croup. But the pages allotted us in the Monthly, have been already fully occupied. Besides, these views have been stated at length, in our work—"Observations on the Pathology of Croup and its Treatment"—which has been several years before the medical public.* The accumulated experience

* With regard to the general treatment in the management of croup, we accord fully with Dr. Ware, in the propriety of avoiding, generally, "all reducing, depleting, and disturbing remedies." We would administer, as required, mild emetics, of ipecacuanha, or ipecac. and sulphate of zinc. Calomel alone, or in combination with Dover's powder, or opium, is frequently required, but never in *scruple doses* as it has been recommended to be given to young children, by some practitioners in this city. These remedies, together with the inhalation of the vapor of warm water, may be employed in the treatment of croup. But our main depend-

furnished from the treatment of a large number of cases of croup, since the publication of that work, on the plan therein advocated, *without the loss of a single patient in our own practice*, has confirmed us in the opinion of its efficacy over all other methods hitherto advanced for the treatment of this terrible malady; and it has impressed us with the full belief that had the prejudice against topical medication not existed with the profession in this city—a prejudice which, we regret to add, has been excited and fostered by some of our prominent medical men—and this method conjoined with *mild* constitutional remedies, has been generally adopted, in the treatment of those *six hundred and eight* fatal cases of croup, which occurred during the last year, not one half of that number would have perished, at least from that disease of which they died.

to the tonsils, and to the faucial region generally, but the applications must be extended into the laryngeal cavity.

If the exudations are not already formed into adventitious membrane, the employment of a few successive applications below the epiglottis may be sufficient to arrest the plastic inflammation altogether. But even in a more advanced stage of the disease, when, from its continuance and the severity of the disease, we have reason to apprehend the formation of a false membrane, or a "tubular mould" throughout the larynx and trachea, we should not despair of removing the obstruction, or of arresting the inflammation.

When called, therefore, to a case of croup in this its second or developed stage of the disease—and fortunately, it is not until this period of the affection that medical aid is resorted to, in a large proportion of the cases of croup—the local employment of the nitrate of silver, conjoined with other appropriate measures should be entered upon at once.

An application may first be made to the tonsils, and about the opening of the glottis. After a delay of from fifteen minutes to an hour, the operation may be repeated, and the sponge wet with the solution should then be passed into the larynx. The cauterizations may be repeated once in two, four, or six hours, according to the effect produced and the intensity of the disease.

OBSTETRICS.

Mammary Abscess prevented by Iodine Ointment. By Dr. R. H. BAYLOR. (Stethoscope.) The subject of this paper is a coloured woman, aged thirty years, with six children. Owing to an obstruction of the ducts, she was unable to nurse her last child, caused from an inversion of the nipples. On my first visit I found the breasts very much enlarged, quick and full pulse, hurried respiration, hot and dry skin, tongue coated with a whitish fur in the centre, but red around the *edges*—bowels had not been moved since the birth of the child, which was some five or six days previous—great pain in the abdomen—the patient could not bear the slightest pressure. Blood was freely taken, and then a full dose of mercury in combination with opium was administered.

R. Hydrarg. chlor. mit. grs. xv; pulv. ipecac. et opii, grs. x. M. To be taken at one dose.

After the first operation from the bowels, all symptoms of puerperal peritonitis ceased. My attention was then directed to the breasts. They

were very painful and hard. The first indication suggested to my mind was to relieve the tention by drawing the milk, but failed in the attempt. Violent inflammation had already commenced, and spread with great rapidity over the whole chest. I then went to work to prevent, if possible, suppuration. Having seen an article on "iodine as a preventive of mammary abscess," in a number of the Medical Examiner, from the pen of Dr. H. C. Stewart, Somerset county, Penn., on the fourth day, finding them (breasts) much larger and intensely painful, I determined at once to make an application of iodine ointment to the breasts, spread upon cloth; and after the third application, all signs of inflammation and suppuration had disappeared—the breasts entirely relieved of pain and perfectly flaccid. From the success of the iodine ointment in this and other cases reported, I hope the profession may be induced to give this valuable remedy a trial.

In conclusion, I submit these suggestions to the readers of your journal, hoping that they may not be disappointed in its remedial powers.

On a mode of removing the Placenta, not generally resorted to. By Dr. SCHNEEDER. (Peninsular Jour. Med.) If we fix upon a table with the hand or a weight, a fresh placenta, and seizing it with a pair of wide bladed forceps, either by its margin or at the attachment of the cord, turn them once or twice round, we observe that the bulk of the placenta is diminished, and its strength increased so that we have to use more force to tear it apart.

We find also that we are able to push the placenta through a hole made in a board, by twisting it through which no force could make it pass in any other manner, without laceration.

I think that we may, with advantage, make use of these two facts in cases where other means of removing the retained placenta have failed, or where severe hemorrhage requires its immediate removal. For instance, when the placenta is retained by spasm of the uterus, two fingers are to be introduced which seize a portion of the placenta. The forceps are to be applied to this portion, turned once or twice round, and moderate traction used.

Should we not succeed in overcoming the spasm, the two fingers hold the twisted portion of the placenta in its position, and the forceps are applied higher up. Thus the placenta diminished in size, and rounded, dilates the os uteri more equally than the fingers, and its ability to endure traction is much increased.

Should the uterine surface of the placenta present itself, we should, if possible, change its position so as to be able to apply the forceps to the margin. The same process may be used in an hour glass contraction as well as in adherent placenta, naturally without using any traction in this last case.

As the periphery of the adherent placenta by twisting it is slightly moved toward the center, the adhesions will easier yield to the separating hand, and the danger of laceration or inversion is less.

If we had to remove a very fragile placenta, we might surely and successfully have recourse to this twisting; and finally, we must apply

this method when the placenta is retained after abortion, other means of removing it having been successful.

Though I have no certain proof for my idea in regard to the first mentioned cases, I remember a case of abortion where, by this proceeding, I evidently saved a life, which case I will in short relate.

Having been called to a woman that had an abortion in the fourth month, was told by the midwife, that three days previous, the abortion had taken place, that the placenta had not been naturally expelled, nor had she herself been able to remove it; and that some hours before my arrival, a severe flooding had taken place, which had reappeared whenever the patient recovered from fainting. The funis was torn off, and the midwife had removed as much of the placenta as she had been able to reach, so that I could only just touch some of the remaining part.

I ordered applications of cold water both internally and externally, and a strong dose of ergotine, and went home for a proper pair of forceps; but the husband came running after me, and informed me that I would probably find his wife dead, as the hemorrhage had become worse than ever before.

When I came back, without trying to revive the patient, I immediately introduced my hand, and seized with the forceps as much of the placenta as I could reach. For three several times, however, I could only remove small portions, but was at last able, by applying the above mentioned method, to remove the whole.

It is not in slight cases that I would advise this method of twisting the placenta, though even then it might be used successfully; but where danger is urging, various other means having failed, it seems to me a method not only theoretically true, but practically useful.

On the different modes of delivery, I have read Naegele, Siebald, Scantzony, Cazeau, Lievee, Ramsbotham and Churchill, without finding mentioned the twisting of the placenta; therefore, I call this idea a new one. Whether it will prove a good one, experience will show.

Difficult Labour Incision of Neck of Womb. By M. PAUL DUBOIS, Clinique of Faculty of Medicine. (Condensed from *Journal de Médecine et de Chirurgie Pratiques*.) In a woman who was brought to bed on the 13th February, in her third labour, there was complete dilation at 2, P. M., of the 16th. At 10 o'clock, of the morning of the 15th, she still continued to suffer. M. Dubois found the neck of the womb extended, forming a circular ring, the resistance of which evidently caused the obstacle to the spontaneous termination of the delivery. Two incisions at points opposite to this tightened portion were practised by a button pointed bistoury, and a little while after she was delivered naturally.

M. Dubois had practised the same operation but a short time previously, for the purpose of enabling him to introduce the forceps in a case where the life of the child seemed threatened by a vicious conformation of the pelvis, and by the transverse position of the head of the child. In this instance, he did not hesitate to incise the neck to the right and left.

EDITORIAL AND MISCELLANEOUS.

American Medical Association.

[WE condense the following account of the late meeting of this Body, from the St. Louis Medical and Surgical Journal,]

THE seventh annual meeting of the American Medical Association, was held in Veranda Hall in this city, on Tuesday, May 2d. In the absence of the President Dr. Knight of New Haven, at 11 o'clock, A. M., the Association was called to order by Dr. Usher Parsons, of Rhode Island, the Vice-President.

Dr. Jas. R. Washington on behalf the committee of arrangements welcomed the delegates and tendered to them the hospitalities of our citizens, in a neat and appropriate address, which was responded to on the part of the Association by Dr. Parsons.

The roll being called, the President announced that the Association was organized. He congratulated the Association on the return of their anniversary, and expressed himself much pleased to see so many new delegates.

The report of the Committee on Publications was read, and laid on the table. The Treasurer's report was also read, and referred.

On motion of Dr. White, of New-York, a recess of fifteen minutes was now had, to allow the delegates of each State to meet and nominate of their number to form a nominating committee to report permanent officers for the Society.

On re-assembling the following gentlemen were reported as composing the nominating committee: *Maine*, Dr. Charles Millett; *Massachusetts*, Dr. D. H. Storer; *Connecticut*, Dr. P. G. Rockwell; *New-York*, Dr. L. P. White; *New-Jersey*, Dr. George R. Chitwood; *Pennsylvania*, Dr. Rene La Roche; *Virginia*, Dr. Adam Spittler; *Minnesota*, Dr. J. H. Murphy; *South-Carolina*, Dr. Thomas G. Prioleau; *Illinois*, Dr. W. B. Herrick; *Alabama*, Dr. S. W. Clanton; *Louisiana*, Dr. E. D. Fenner; *Missouri*, Dr. Thomas Reyburn; *Michigan*, Dr. William Dro-

from Dr. Knight, explaining his absence—from E. L. Beadle, of New-York, tendering his resignation as one of the Secretaries—and others.

On motion, adjourned to 3 o'clock, P. M.

On taking the chair, Dr. Parsons, the senior Vice-President, in the absence of the President, delivered an able and interesting address, which was listened to with interest, and which, on motion, was referred to the committee on Publication, to form a part of the Transactions.

Dr. Gross, of Louisville, Kentucky, offered the following resolution :

Resolved, That hereafter it shall be considered disorderly for committees of arrangements of this Association to give costly entertainments.

This resolution gave rise to some discussion. Dr. McPheeters, of St. Louis, moved the following as an amendment :

Resolved, That, in future, committees of arrangements be requested to dispense with all costly and extravagant entertainments. The amendment was accepted by the mover and carried by a large majority.

The committee on nominations now made their appearance, and, through their chairman, Dr. White, of Buffalo, N. Y., recommended the following for permanent officers :

FOR PRESIDENT,

CHARLES A. POPE, M. D., of Missouri.

VICE-PRESIDENTS,

E. D. FENNER, M. D., of Louisiana,

N. S. DAVIS, M. D., of Illinois,

WM. T. WRAGG, M. D., of South-Carolina,

JOHN GREEN, M. D., of Massachusetts.

SECRETARIES,

E. S. LEMOINE, M. D., of Missouri,

FRANK WEST, M. D., of Pennsylvania.

TREASURER,

D. F. CONDIE, M. D., of Pennsylvania.

The report of Dr. Condie, chairman of the committee of publication, .

ness before their names were stricken from the list, they were adopted without opposition.

Dr. Atlee, on behalf of the committee to procure a stone with a suitable inscription for the monument of Washington, reported that he had adopted, at the suggestion of the lamented Dr. Pierson, of Salem, the design for the stone, representing *Hippocrates* refusing the presents of King *Artaxerxes*, who invited him to go to Persia and succor the enemies of Greece. The sculpture was on beautiful marble, by Samuel Beck, a young artist of Lancaster county, Pennsylvania, from a daguerreotype copy of Viardot's celebrated picture, presented to him by Miss Abby L. Pierson. The execution of the work is in the highest style of art, and evinces extraordinary talents in the artist. He also stated that, in order to complete the work, \$400 in addition to what had already been received would be required, and those favourable to the undertaking were invited to come forward and contribute.

Dr. Charles Hooker, of Conn., was appointed Treasurer *pro tem.*, Dr. Condie, the Treasurer, being absent.

Dr. Pope, the President elect, appearing in the Association, was conducted to the chair, and on taking his seat returned thanks for the honour conferred on him in the following address :

Gentlemen :—There are occasions when the mouth is dumb, because the heart is full. I am unequal to the task of thanks. Honour, which is valuable according to its source, is in the present case great indeed ; for I behold around me the members of a noble profession, the cultivators of a yet nobler science, gathered from the wide extent of our beloved country.

I would that this undeserved and unexpected honour had fallen on an older and worthier than I, for many such I see before me: In one thing only will I claim equality ; for, however, unworthy in other respects, I yield to none in ardent devotion and ceaseless love to our time-honoured art. But I would not claim more than I ought. Nor do I misconstrue the honour which your partiality has conferred upon me ; for well I feel that it was intended less as an individual compliment than one to the West in general, and to Missouri especially. In this view, gentlemen, for them and for myself I return you the warm unfeigned thanks of a grateful heart.

Dr. Pope, Chairman of the Committee on Prize Essays, reported that nine essays had been submitted to their consideration, of which, one was presented as a voluntary contribution. Many of these essays possessed undoubted merit both in matter and style, but the committee preferred to be governed in their choice by considerations of originality and practical import, and had, therefore, concluded to award but one prize. The essay selected is entitled, "An Essay on a New Method of Treating ununited Fractures and certain Deformities of the Osseous System."

On breaking the seal of the packet, Daniel Brainard, of Chicago, was announced as the successful competitor.

Dr. Guthrie, of Tennessee, offered the following resolutions, which were unanimously carried :

Resolved, That in the Secretary of the Treasury's recommendation to Congress to abolish or materially modify the duty on such crude drugs not produceable in this country, as are used in the laboratories of the country in the manufacture of chemicals, we recognize a wise provision for the further protection of the profession and the community at large, from impure and sophisticated medicines.

Resolved, That a copy of this resolution be signed by the proper officers of this Association, and transmit the same to the Secretary of the Treasury and to the Committee on Ways and Means.

Dr. J. B. Johnson read a letter from Dr. Stephen W. Williams, asking that certain resolutions which he had offered at the last meeting in New-York, in reference to the preparation and publication of memorials of the distinguished dead of the profession in this country, be taken up and acted on. On motion, the resolutions were called up and adopted, and the President authorized to appoint a committee to carry them out.

In conformity with the recommendation of the Committee on Nominations, Philadelphia was fixed on as the place for holding the next meeting of the Association.

Dr. French submitted the following resolution, which was carried :

Resolved, That a Committee be appointed to enquire what State or other Society, represented in this Association, are in fellowship with irregular practitioners.

Dr. Blachford, of Troy, read a letter from Dr. A. D. Spore, stating that he (Dr. Spore) had been for some time investigating the subject of Hydrophobia, to ascertain what influence the weather had upon the disease, and in the letter he requested that communications on the subject might be sent to him by members of the Faculty who had opportunities of making observations. Dr. Spore not being a delegate, it was moved

Dr. Blachford be appointed Chairman of a Committee for the investigation of this subject.

The following was offered by Dr. S. M. Smith, of Columbus, Ohio, which was carried :

Resolved, That a Standing Committee of ——— be appointed by the Association on the subject of Insanity as it prevails in this country, including its *causation*, hereditary transmission, educational influences, physical and moral, social and political institutions, &c. *Its forms and complications, curability* and means of cure and prevention.

The committee appointed by the American Medical Association to devise or consider some comprehensive plan for the more general, systematic and thorough investigation of subjects connected with medical science, made a report, to which was appended the following resolution :

Resolved, That the American Medical Association hereby recommends all Medical Societies to establish, in accordance with the plan detailed in the foregoing report, special committees for the selection of all subjects of interest connected with medical science.

The resolution carried, and the report and resolution was referred to the Committee on Publication.

Dr. Atlee offered the following resolution, which was carried :

Resolved, That it shall be the duty of the Publication Committee to append to each volume the Transactions hereafter published, a copy of the Constitution of the Association.

The following resolution, offered by Dr. Gross, was also carried, and Dr. Gross was appointed by the Chair the committee designated :

Resolved, That a committee of one be appointed by the Chair to enquire into the causes which obstruct the formation and establishment of our National Medical Literature, and to report the subject at the next annual meeting of this Association, or as soon thereafter as practicable.

Dr. J. Berrien Lindsey offered the following resolution, which, on motion, was referred to the Committee on Medical Education, with instructions to report at the next annual meeting of the Association :

Resolved, That this Association earnestly recommend to the few Western schools which still retain the rule of making four years' practice equivalent to one term at college, the abrogation of said rule, as holding out a strong inducement and temptation to young men to enter upon the practice of medicine with little or no preparation.

Dr. Paul F. Eve, of Nashville, Tenn., submitted a resolution, which, after amendment, was carried :

Resolved, That a committee of three be appointed by the Chair, to report at the next meeting of the Association, the best means for preventing the introduction of diseases by emigrants into our country.

The Chair appointed Drs. Dickson, Griscom and E. D. Fenner, said committee.

Dr. Linton, of St. Louis, offered the following, was also referred to the above named committee :

Resolved, That, in the opinion of this Association, quarantine establishments afford no protection to States and cities against the invasion of epidemics, such as cholera and yellow fever.

The following report of the Committee on Nominations was submitted by the chairman, Dr. White.

The Committee on Nominations, in fulfilling the duty imposed upon them, recommend the continuance of several of the special committees previously created, and the appointment of some new ones. They, therefore, submit the following list of Chairmen of special committees, with the subjects to them committed :

Dr. Worthington Hooper, of New-Haven, Connecticut, on epidemics of New-England and New-York.

Dr. John L. Atlee, of Lancaster, Pa., on epidemics of New-Jersey, Pennsylvania, Delaware and Maryland.

Dr. D. J. Cain, of Charleston, S. C., on epidemics of South-Carolina, Florida, Georgia and Alabama.

Dr. W. L. Sutton, of Georgetown, Ky., on epidemics of Tennessee and Kentucky.

Dr. Thos. Reyburn, of St. Louis, Mo., on epidemics of Missouri, Illinois, Iowa and Wisconsin.

Dr. Geo. Mendonhall, of Cincinnati, Ohio, on epidemics of Ohio, Indiana and Michigan.

Dr. E. D. Fenner, of New-Orleans, La., on epidemics of Mississippi, Louisiana, Arkansas and Texas.

Dr. James Jones, of New-Orleans, La., on the mutual relation of yellow and bilious remittent fever.

Dr. D. V. Condie, of Philadelphia, Pennsylvania, on the causes of Tuberculous Disease.

Dr. Jos. Leidy, of Philadelphia, Pa., on diseases of the Parasitic Origin.

Dr. A. P. Merrill, of Memphis, Tenn., on the Physiological Peculiarities and Diseases of Negroes.

Dr. Jos. N. McDowell, of St. Louis, Mo., on Statistics of the operation for the Removal of Stone in the Bladder.

Dr. F. P. Porcher, of Charleston, S. C., on the Toxicological and Medicinal Properties of our Cryptogamic Plants.

Dr. Daniel Brainard, of Chicago, Illinois, on the Constitutional and Local Treatment of Carcinoma.

Dr. Geo. Englemann, of St. Louis, Mo., on the Influence of Geological Formations on the Character of Diseases.

Dr. Henry Taylor, of Mount Clemens, Mich., on Dysentery.

Dr. Horace Green, of New-York, on the use and effect of applications of Nitrate of Silver to the Throat in Local or General Disease.

Dr. P. C. Gooch, of Richmond, Va., on the administration of Anæsthetic Agents during Parturition.

Dr. Chas. Hooker, of New-Haven, Conn., on the Diet of the Sick.

Dr. E. R. Dabney, of Clarksville, Tenn., on certain forms of Eruptive Fevers, prevalent in Middle Tennessee.

Dr. Sanford B. Hunt, of New-York, on the Hygrometrical State of the Atmosphere in various localities, and its influence on health.

Dr. Frank H. Hamilton, of Buffalo, New-York, on the Frequency of Deformities in Fractures.

Dr. G. S. Walker, of St. Louis, Mo., on diseases of the Prostrate Gland.

Dr. H. A. Johnson, of Chicago, Ills., on the Excretions as an index to the Organic Changes going on in the System.

Dr. Leroy H. Anderson, of Sumpterville, Ala., on the Typhoid Fever and its complications as it prevails in Alabama.

Dr. W. H. Byford, of Evansville, Ia., on the Pathology and Treatment of Scrofula.

Dr. N. S. Davis, of Chicago, Illinois, on the Nutritive Qualities of Milk, and the influence produced thereon by pregnancy and menstruation in the human female, and by pregnancy in the cow; and also on the question whether there is not some mode by which the nutritive constituents of milk can be preserved in their purity and sweetness, and furnished to the inhabitants of cities in such quantities as to supercede the present defective and often unwholesome method of supply.

Dr. M. M. Pallen, of St. Louis, Mo., on Puerperal Convulsions.

Dr. E. B. Haskens, of Clarksville, Tenn., on the Microscopical Investigations of Malignant Tumors.

Dr. Geo. K. Grant, of Memphis, Tenn., on the Sulphate of Quinia as

Dr. Charles Q. Chandler, of Rocheport, Mo., on Malignant Periodic Fevers.

Dr. S. B. Chase, of Portland, Maine, on Typhoid Fever in Maine.

Committee on Plans of Organization for State and County Societies.—A. B. Palmer, M. D., Michigan; R. R. McIlvain, M. D., Ohio; D. L. McGugin, M. D., Iowa; E. R. Peaslee, M. D., New-Hampshire; Thos. Lipscomb, M. D., Tennessee.

Committee on Medical Literature.—Robert J. Breckenridge, M. D., Kentucky, Chairman; A. A. Gould, M. D., Mass.; D. L. McGugin, M. D., Iowa; J. B. Flint, M. D., Ky.; O. M. Langdon, M. D., Ohio.

Committee on Medical Education.—Wm. H. Anderson, M. D., Alabama, A. Lopez, M. D., do.; Andrew Murray, M. D., Michigan; A. Ramsey, M. D., Tennessee; R. D. Ross, M. D.

Committee on Prize Essays.—Rene La Roche, M. D., Pennsylvania; Isaac Hays, M. D., do.; Alfred Stille, M. D., do.; J. B. Biddle, M. D., do.; Geo. W. Norris, M. D., do.; Joseph Carson, M. D., do.; Joseph Leidy, M. D., do.

Committee of Arrangements.—Isaac Hays, M. D., Pennsylvania; G. Emerson, M. D., do.; Wilsen Jewell, M. D., do.; Alfred Stille, M. D., do.; Francis West, M. D., do.; Wm. V. Keating, M. D., do.

Committee on Publication.—Pliney Earle, M. D., New-York; D. Francis Condie, M. D., Pennsylvania; E. S. Lemoine, M. D., Missouri; A. March, M. D., New-York; E. A. Davis, M. D., do.; C. R. Gilman, M. D., do.

Dr. Atlee offered the following resolution, which was carried :

Resolved, That this Association earnestly recommend to their medical brethren in those States in which Societies do not exist, the immediate organization of State and County Medical Societies.

Dr. Phelps, of New-York, read a long and able paper on the connection between Religion and Medicine, which, on motion, was referred to a special committee, consisting of Drs. Atlee, Sayre and Marsh.

Dr. Breckenridge, of Kentucky, offered the following resolution, which was carried :

Resolved, That hereafter the majority of the Committee on Publications shall be selected from the Physicians of that city in which the Association may annually meet.

A vote of thanks was then unanimously returned to Dr. Condie, for the able, zealous and impartial manner with which he had discharged his duties as Treasurer.

A resolution was reported to amend the constitution, which provides that its annual meetings shall be held on the first Monday in May, and substitute the second Monday. The resolution, under the rule, lies over for a year.

On the recommendation of the Nominating Committee, Dr. Isaac Wood, of New-York, was elected Treasurer, to supply the vacancy occasioned by the resignation of Dr. Condie.

On motion of Dr. Atlee, thanks of the Association were tendered to the Committee of Arrangements and to the profession and citizens of St. Louis, for the hospitality with which they have been received and entertained.

Dr. Mussey, of Cincinnati, was appointed to report at the next meeting, on the Effects of Alcoholic drinks in Health and Disease.

Notice to Subscribers.

SUBSCRIBERS are earnestly requested to remit the amount of their indebtedness without delay. The Journal will not be sent, after the present issue, to those who do not comply with our reasonable and just demand.

TO CONTRIBUTORS, PUBLISHERS, ETC.

Drs. Nott & Gliddon's Work on the Races will be reviewed in our next.

The following books, pamphlets, etc., have been received:

The Science and Art of Surgery; being a treatise on Surgical injuries, diseases and operations. By John Erichsen, Prof. of Surgery in Univ. College and Surgeon to Univ. College. By John H. Brinton, M.D. Illustrated by 311 engravings. Philadelphia: Blanchard & Lea. 1854.

Hand-book of Chemistry, theoretical, practical and technical. By F. A. Abel, Prof. Chemistry at Royal Military Acad., Woolwich, etc., and C. L. Bloxam, formerly past Assistant to Royal Col. of Chem. With a preface by Dr. Hofman and numerous illustrations on wood. Philadelphia: Blanchard & Lea. 1854. (From publishers, through Mr. Russell.)

Woman: her Diseases and Remedies. A series of letters to his class, by Chas. D. Meigs, M.D., Prof. Midwifery, etc. Third edition, revised and enlarged. Philadelphia: Blanchard & Lea. 1854. (From publishers, through Mr. Russell.)

On Rheumatism, Rheumatic Gout and Sciatica; their pathol. symptoms and treatment. By H. W. Fuller, M.D., Contab. Assist. Phys. to St. George's Hosp., etc. New-York: Samuel & William Wood, 261 Pearl-street. 1854. (From publishers.)

Archives de Physiologie de Therapeutique et d'Hygiene sous la direction de M. Bouchardat, Prof. d' Hyg. a la Faculté de Méd. de Paris. No. 1, Janvier, 1854. Memoire sur la Digitaline et la Digitale, par E. Homolle et F. A. Quevenne Baillière, Libraire-éditeur Rue de L'Ecole de Med. 17. Paris. 1854. And Chez. Baillière. New-York. (From publishers.)

Clinical Report on Pleurisy, based on the Analysis of forty-seven cases. By Austin Flint, M.D. Buffalo: Jewett, Thomas & Co. 1853. Also, by same author, Clinical Report on Dysentery. (To be noticed.)

 Dr. J. Lawrence Smith, formerly of Charleston, is appointed Professor of Medical Chemistry and Toxicology, University of Louisville.

*Meteorological Observations made at Aiken, in South-Carolina,
for the month of May, 1854, by H. W. RAVENEL.*

Days.	Rain and Melted Snow.			Psychrometer.						Daily Remarks on the Weather.
	Hour began.	Hour ended.	Am't inches.	Dry bulb.			Wet bulb.			
				7 A. M.	9 P. M.	9 P. M.	7 A. M.	9 P. M.	9 P. M.	
1				47	66	63	46	65	62	clear and cool.
2				58	75	69	53	58	60	clear; weather moderating.
3				63	73	67	58	58	56	cloudy morning; clears off.
4				60	75	66	53	61	59	clear.
5				61	73	71	51	63	64	cloudy morning; clears off.
6				65	74	68	61	67	65	cloudy and variable.
7				65	77	71	63	68	68	clear.
8				63	80		61	69		clear.
9				68	83	77	65	70	68	clear.
10	A. M.	A. M.		68	81	76	65	68	68	clear; windy at night.
11	9.00	10.30	1.150	72	75	70	68	70	68	heavy shower in the morning.
12				70	77	72	68	71	68	morn'g overcast & heavy; clears off; va'ble.
13				71	77	73	67	68	67	variable and unsettled.
14				69	79	74	66	72	71	variable and unsettled.
15	P. M.	P. M.		74	83	78	70	73	70	va'ble morn'g; clears off. [der,light'g & wind
16	6.30	7.30	2.210	72	87	73	69	73	70	clear morn'g; h'vy fall rain after'n,with thun-
17			0.410	74	75	74	71	72	70	morn'g overcast, with light rain; clears off.
18				66	76	69	60	64	63	clear.
19				63	77	73	58	64	67	clear.
20				69	83	78	64	68	69	clear.
21				73	84	89	69	71	73	clear. [slight rain in afternoon.
22				73	83	75	70	72	70	clear morning; thunder and lightning with
23	6.30	7.00	0.066	70	81	76	67	71	73	clear morning; light shower in evening.
24				72	81	75	69	71	70	clear.
25				72	83	79	69	70	71	clear.
26				72	85	81	70	72	73	clear. [distant thunder.
27				75	85	75	70	76	71	clear morning; cloudy in afternoon, with
28	3.50	4.20	0.472	73	85	76	69	73	73	thunder stm. in afternoon with rain & wind.
29			1.210	75	75	71	72	72	69	showery all the morning; afternoon variable.
30				72	80	69	71	73	67	variable; cloudy, with distant thunder.
31			0.610	68	84	79	67	71	73	foggy morning; clears off; rain* in the night.
6.128										

SAVANNAH MEDICAL COLLEGE

SESSION 1854-'5.

R. D. ARNOLD, M. D., Prof. Theory and Practice of Medicine.

P. M. KOLLOCK, M. D., Professor of Obstetrics.

W. G. BULLOCK, M. D., Professor of Surgery.

C. W. WEST, M. D., Professor of Medical Chemistry.

H. L. BYRD, M. D., Professor Materia Medica and Therapeutics.

E. H. MARTIN, M. D., Professor of Institutes of Medicine.

J. G. WAVERD, M. D., Professor of Anatomy.

J. B. READ, M. D., Professor of Pathological Anatomy and Demonstrator of Anatomy.

J. J. WEST, M. D., Assistant Demonstrator of Anatomy.

The regular course of LECTURES in this Institution, will commence on the *first Monday in November*, and terminate in the following March. Preliminary Lectures will be delivered from Monday, the 16th day of October, until the commencement of the regular course, for which no additional fee will required. Students have free access to the Medical and Surgical Wards of the Savannah Hospital. This extensive charity is under the medical control of two members of the Faculty, by whom clinical lectures are delivered. In addition to this, they have ample opportunity for learning *practical* medicine at the College Clinic, which is open daily to patients.

During the past session, upwards of two hundred cases were prescribed for, and over thirty surgical operations performed in the presence of the students at this clinique. *Material* for anatomical investigations will be abundantly supplied.

FEES—Matriculation, \$5; Full Course of Lectures, \$105; Demonstration, \$10; Graduation, \$30. For further information address (post-paid),

H. L. BYRD, M. D., DEAN.

UNIVERSITY OF LOUISVILLE

MEDICAL DEPARTMENT.

The Eighteenth annual course of Lectures in this Department, will commence on the 30th of October next, and terminate on the last of February, under the following arrangement:

BENJAMIN R. PALMER, M. D., Professor of Descriptive and Surgical Anatomy.

LUNSFORD P. YANDELL, M. D., Professor of Physiology and Pathological Anatomy.

SAMUEL D. GROSS, M. D., Professor of the Principles and Practice of Surgery.

HENRY MILLER, M. D., Professor of Obstetric Medicine.

LEWIS ROGERS, M. D., Professor of Materia Medica and Therapeutics.

J. LAWRENCE SMITH, M. D., Professor of Medical Chemistry and Toxicology.

AUSTIN FLINT, M. D., Professor of the Theory and Practice of Medicine.

T. G. RICHARDSON, M. D., Demonstrator of Anatomy and Dissector in Pathological Anatomy.

The fee for admittance to the Lectures of each Professor is \$15, (\$105 in all,) payable invariably in advance. Matriculation and Library fee together, \$5. Graduation fee, \$25. Practical Anatomy and Dissection, \$10—ticket to be taken at least once before graduation. Rooms open from 1st October.

A preliminary course of Lectures will be delivered, without additional charge, during the month of October.

Clinical instruction is given twice a week at the Louisville Marine Hospital. Ticket \$5, to be taken once before graduation.

CHARLESTON MEDICAL JOURNAL AND REVIEW.

Vol. IX.] CHARLESTON, S. C., SEPTEMBER, 1854. [No. 6.

ART. I.—*Position and character of the abnormal sounds, with other diagnostic symptoms, in twenty-three cases of Disease of the Heart, observed in the Hospital La Charité, Paris, particularly in M. Bouillaud's wards.* By F. PEYRE PORCHER, M.D. Paris: 1852 and '3.

WE are uncertain whether the publication of this paper will assist any one in his investigation of this difficult subject, who has not previously paid some attention to the normal, as well as to the deranged, sounds of the heart.

Whatever value it has consists in this—that it presents accurate statements of conditions and symptoms which existed, and which were interpreted by some of the highest authority living. We had the opportunity of selecting and examining, under the guidance of M. Le Maire, for many years M. Bouillaud's Chef de Clinique, all the cases of disease of the heart and chest in the Hospital, and again of seeing the examinations and hearing the comments of the latter at his morning visits. By this means being enabled to collate and verify the opinions given by each.

We should say that M. Le Maire has the entire confidence of M. Bouillaud, for it was under his special sanction that the examinations were made, and many of the cases brought under review were commented on, and the phenomena they presented often discussed by them before us.

We alone, however, are answerable for notes taken at the bed-side, as faithfully as we could, from their observation, and from the confirmation afforded on each occasion by our own subsequent and repeated examinations.

Many of the cases which, however, we have not desired to give in full, are very imperfectly related from meagre details, but what are presented furnish positive examples of states and conditions which

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existed. The diagnoses being made by those especially competent to pronounce, they may serve to indicate why certain conclusions are to be drawn from certain symptoms.

There has been no attempt in all cases to give the treatment, nor have post mortems been furnished to confirm or invalidate the prognoses; whenever possible however, they are, of course, included.

French technical terms, descriptive of the character of the deranged sounds in diseases of the chest and heart, are so consecrated by the genius of Lænnec, Bouillaud and others, that we offer no excuse for their frequent substitution.

Musical sounds.

CASE MARKED No. 5.—Female æt. 27. First sound of the heart, a prolonged musical murmur, very distinct, over entire precordial region, particularly at apex under breast. The second sound seemed masked by this. The pulse not very full nor much disturbed.

Intermittent and prolonged sounds.

I heard several resulting from anæmia and chlorosis in hysterical patients, and in those suffering from hemorrhages and after typhoid fever. These, however, it is not necessary to designate more particularly.

Absence of first sound—auriculo-ventricular alterations.

No. 9.—Male. Andral's wards—absence of first sound. The second is replaced by a *bruit* (*fremissement*), audible at the upper portions of the thorax above the fourth rib, and extending to the cervical region, dependent upon hypertrophy.

The abnormal sound is not heard at the point of the heart, though the first sound is not distinguished anywhere—no *claquement* at all.

The pulse was full and the thrill quite perceptible in it. A *souffle* under the knee could be plainly recognized.

Hypertrophy of left heart—not extending materially to the aorta—dependent upon Rheumatism.

No. 1.—New patient in M. Bouillaud's wards.

Boot-maker, æt. 33—full habit. Father had not suffered from dis-

Beneath the third rib, externally, the first sound not audible, more evident above, with scarcely any continuation of sound in aorta—none in carotids.

Deep under the left precordial region under the arm, the first sound indicating enormous alteration of orifice of mitral valve.

A diagram would represent the positions.

1. Alteration of orifice of mitral valve.
2. Souffle of the first sound.
3. Enormous first sound audible.
4. Here not so evident.

At each movement of carotids a series of oscillations observed in neck.

Pulse not strong.

M. Bouillaud thinks that the left ventricle does not contain much blood, there being no alteration of the valves.

Narrowing (retrecisement) of auriculo-ventricular opening.

There is no swelling of the legs.

Treatment.—Blister to chest; lemon juice and digitalis; it produced irregular pulse, thus:—1, 2, 3; 1, 2, 3; 1, 2; 1; 1, 2; 1, 2, 3, 4.

Several days subsequently under the treatment sounds have become almost normal—still, however, there is a souffle of the first sound at the point of the heart, with a slight hardness of the second sound.

The patient immediately opposite has a double second sound at the point of the heart very audible, the first being natural. No abnormal sounds at base.

Right Heart affected.

No. 17.—Bouillaud's wards. Female æt. 43, has what is unusual viz: the right heart affected with narrowing of the auriculo-ventricular openings on each side, with a souffle of the first sound and a venous pulse.

Dilatation of the right side of the heart and narrowing of the tricuspid induced venous pulse. Another case in Bouillaud's wards.

Endocarditis from Acute Pleurisy.

Musical second sound at base of the heart—prolongation of the first sound at point.

CASE No. 1.—Bouillaud's wards at La Charité. In a young man who had suffered from acute rheumatism three months previously, important changes had already taken place; quinine in small quantities had been administered, but the nurse refused to give it.

In this case there is general change, including a prolongation of the first sound at the point.

At base musical sound of second, which is prolonged into the aorta, hence not pericardial in cause.

Diagnosis.—Disease alteration of sigmoid valves of the aorta with slight retrecisement—alteration of the valves possibly by incrustation, but causing no pouring back of the blood, because that would give rise to a souffle which does not exist. Hence, a rugosity of valve; and also a lesion of the bicuspid.

The pulse being regular, there is no great amount of retrecisement which would be attended by diminished force of the pulse.

Hypertrophy.

CASE MARKED No. 7.—Boullaud's wards.

M. Boullaud thinks that when the point of the heart beats (impinges against thorax) as low as the sixth rib, there is hypertrophy, particularly when it strikes without a vertical line extending from the nipple.

The souffle of the first sound, becoming more distinct and defined (net) at base, accompanies the shock of the heart, covering the first stroke, (claquement) and lessening as it approaches the base—hence indicating the mitral valve as the chief point of lesion (cartilaginification!) The cause of the rubbing (frottement) is the insufficiency of the valvule, as the pulse is weak and the patient suffers from a sense of suffocation when he walks. He has never had irregular pulse or swelling of the limbs.

But the slight souffle of the second sound shows, also, a slight retrecisement or irregularity of structure in the other orifice.

So the souffle of the second indicates that the blood in passing the orifice receives a check or change in its current.

You have no swelling of the veins, and the absence of swelling of the limbs indicates, also, the absence of much retrecisement or alteration of sigmoid valves.

The pulse is slightly intermittent every five or six beats.

Patient had not had rheumatism.

Bruit musical.

1st ward at La Charité.

There is a bruit musical, first sound being a, probably, lesion of bicuspid with

Endocardite—prolongation of first sound masking second, from rheumatism.

CASE MARKED No. 1.—Had had rheumatism, which was not treated, three months since.

There is a prolongation of the first sound at the apex masking completely the second. Patient had had a musical sound at the base, but now at the base the second has become perfectly evident and with a soufflé. At this point they are both heard.

This case resulting from acute rheumatism, M. Bouillaud bled for three days consecutively, at the end of which time the sounds became perfectly normal. To-day, March 8, there is still a distinct buffy coat upon the blood extracted.

Bruit musical at base which absorbs second sound.

CASE MARKED No. 10.—There is a bruit musical at base which absorbs the second sound; diagnosis: cartilaginous condition of bicuspid; retrecisement of left auriculo-ventricular orifice.

Only the first sound at point and at base is evident masking the second at the base; diag.: bicuspid and sigmoid both diseased.

The pulse irregular—a condition which sustains the opinion respecting these alterations.

Bruit de scie of first sound at point with thickening or change in bicuspid valves; patient taking digitalis.

CASE No. 1.—La Charité, Bouillaud's wards. Boy æt. 20. At point the bruit de scie is perfectly distinct; pulse rather prolonged, full but vibrating with an occasional jerk; the first sound synchronous with pulse; (patient taking digitalis.)

At the base there is a claquement or bruit of first sound.

The second sound at base is altered in timbre, which shows change in mitral valve.

Both sounds are audible here; the pulse not indicating insufficiency, but all the signs showing thickening or induration of the bicuspid. The soufflé of first sound at base is not connected with that at the point.

At the right side of chest there is also a bruit musical with the first sound.

Diagnosis: it is not a retrecisement of aortic orifice, though the second sound is rough at the base; for various reasons, viz: absence of swelling or dropsical accumulation, intermittence of pulse, etc. M. Bouillaud stated that he could hear the first sound at base with the ear at some distance from the chest, but not the second. (So Corrigan is wrong!)

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But what is the cause of *souffle* of second sound at base! It is not the result of insufficiency, because there is no propagation to the point, so the cause must be searched for elsewhere. You have a bruit musical in the aorta, hence the increased noise of the second sound is probably owing to pericardial influence.

The claquement or rude sound of second at base shows a deposit upon, or change of the valve, but not insufficiency, because then the blood may flow back without any harshness (*rudesse*), though there may be insufficiency.

The alteration of the second sound at base pertains to a rubbing of false membrane of the pericardium, for, not being in the aorta, this conclusion is forced upon us.

No. 7.—M. Boullaud's wards. The prolongation of first and second sound in this case at the apex indicates narrowing of left auriculo ventricular opening and derangement of the bicuspid valve.

There being also the thrill like the purring of a cat (*fremissement cataire*) signifying the passage of the blood against the rugous valve, it is either pathognomonic of derangement of the valves or pleura.

The pulse being irregular, but not large, indicates that the narrowing is not considerable, the blood passing with a certain degree of freedom.

At base the first sound is also prolonged, the second natural.

The patient had taken digitalis.

Second sound at base doubled.

No. 8.—This case presents symptoms precisely the reverse of those furnished by No. 7.

The pulse is small but not filiform or irregular.

In this instance the digitalis is the cause of the slowness of the pulse. Before taking it the pulse had been extremely irregular and fine.

Both first and second sounds at apex are prolonged, particularly the second at the apex.

There is narrowing or retrecisement of the bicuspid orifice and valves.

The second sound at base is doubled, announcing almost constantly a

in chlorotic and nervous females you may have altered sound, but not a regular double sound as in this example.

In retrecisement you can give digitalis, for it regulates the movements and renders them slower, thus allowing the blood to pass from the auricle. Digitalis does not diminish the force of the heart so much as it makes it more regular. When the patient entered he had morbid symptoms very decided and well marked.

There was swelling of the jugular veins, a souffle of the first sound and a slight one of the second. After the lapse of fifteen days there supervened a doubling of the first, the pulse becoming more tumultuous.

The indications of emphysema of the lungs are also quite evident.

Prolongation of first sound at the apex.

No. —. Case of female in wards up stairs. Prolongation of first sound at the apex, the second scarcely distinguishable.

The pulse being well developed and regular, there is no narrowing (retrecisement).

So it is an alteration in the bicuspid and to a certain extent in the sigmoid valves.

The second sound at the base being also extended to the carotid region, indicates its prolongation there and that this does not proceed from the first sound at the apex, which is, as we have just said, disordered.

Exocarditis, Pleuritic claquement.

No. 15.—M. Le Maire. Pleuritic claquement or click just above the heart, existing in pericardium, not in valves. Joints enlarged.

There is, however, a souffle at base prolonged to the carotids, but no prolongation from the point; though, rigorously speaking, it must come from the point, and if it is the second sound, it is necessarily of the sigmoid valves of the aorta.

Digitalis, influence of, in modifying sounds of the heart.

CASE No. 1.—(May be referred to before.) Boullaud's wards. Patient has now only slight souffle of first sound, the abnormal sound at base having completely disappeared.

The second sound has become quite distinct, with no rugosity or induration of the sigmoid.

At the beginning, two weeks previous, he had had a double bruit at base and a musical sound.

There had also been a souffle of first sounds at base and point.

As it has disappeared it was in the pericardium. Had it been insufficiency or lesion of the valule this would never have occurred.

So the bruit musical was not owing to rugosity of the valves of aorta, for as it has disappeared it necessarily was external.

Hence the bruit musical had its seat in the pericardium.

At the point there is even now some thickening of the bicuspid indicated by the soufflé.

The patient was treated by infusion of digitalis; pulse strong and good, therefore not insufficiency but thickening; the patient had not been bled, the digitalis rendering the pulse very regular.

Exocarditis—influence of depletion.

CASE MARKED No. 6.—Man *æt* 30. Bouillaud's wards. Patient has been sick eight days; all sounds of the heart regular. He was bled twice; not relieved of pain; chef de clinique ordered again two bleedings; pulse very rapid. The sounds perfectly normal.

M. Bouillaud discovered, upon examination, slight deep-seated pleurisy. The blood drawn was firm, containing plenty of serum and clot, but no buffy coat.

There may be a very slight carditis existing, for the regularity and persistence of the symptoms forbids our supposing that they depend upon a nervous condition. Vesicatory to chest ordered.

Second days observation.—Bleeding has been repeated, and pulse has fallen to 96; it had been 200. Patient actually seems better.

The noise of first sound at point has become more distinct than ordinary; pulse much more regular; a slight feeling of doubling.

The vesicatory has certainly acted well.

Anæmia, characteristic sounds in.

No. 5.—Woman *æt* 25, who had bled a good deal. At base variable sounds of anæmia.

First sound slightly prolonged at point, more so at base; second natural. The sounds, as would be anticipated, not rough like the bruit de scie, but flowing, from hydræmia of the blood. You also hear the murmur in the carotids. The headache and general appearance also indicate loss of blood. There is not gastralgia enough to make it a case purely hysterical.

Now compare this case with that of the old woman, No. —, who suffers with an organic solidification of bicuspid valve. We find that the sounds, instead of being flowing, are rude, expressing cartilagification, especially at the point. It lifts the hands up when pressed against it, the soufflé being excessively strong at the aortic orifice, indicating also some derangement of the sigmoid.

This case, seen four days after, has continued to improve, notwithstanding the number of bleedings.

At base, first and second sounds are both of the same timbre. At

point, both exist, the second finer and smaller; pulse more regular and natural.

Arterial Souffle.

No. 13.—In second room. New patient; man æt. 37; entered 8th March. Worker in ivory.

Heart when marked out not very large.

He has had very strong palpitations, but never Rheumatism; pain in the head, and dizziness.

Souffle in arteries of the neck; arterial souffle a very distinct roar.

At base first sound more distinct than natural; second a little duller than ordinary; pulse regular, normal but rather slow; both sounds at base quite distinct; at point a very slight prolongation of the first sound.

From these symptoms diagnosis simply a nervous disorder of the heart (*nervose du coeur*) there being no souffle indicating an organic disease.

Lesion in right cavity—venous pulse.

No. 17.—Woman, æt. 60; Bouillaud's wards.

There is a souffle in right cavity of the heart, with a very distinct venous pulse; pulse 40.

At point souffle of first sound; second distinct. At base sounds almost natural. The souffle of first sound at point disappearing here.

The irregular first sound of the bicuspid extends even to the right side for some distance. (The patient is taking digitalis.)

Diagnosis: bicuspid indurated and cartilaginous and from the extent of the sounds and from the persisting venous pulse, you may suppose the tricuspid also diseased; being in fact a lesion of right side of heart.

At first the patient had had a very small pulse and both then and now lower limbs and abdomen infiltrated and swollen.

The digitalis has made the pulse much more regular. There is probably insufficiency of tricuspid and possibly some adhesions. The right orifice is perhaps too large, for the souffle extends there and yet you do not hear the first sound of the tricuspid.

Rheumatic carditis—bruit cataire.

CASE MARKED No. 12.—Becherel's wards.—Entered 25th Feb. Man æt. 70. With Rheumatism.

Pulse full, irregular; impulsion under the carotids very evident.

He has a lesion of bicuspid, though there being very little souffle of second at point, there is not apt to be insufficiency.

The arteries are a little ossified; hence more natural that the incrustations exist in the aorta. But, strange to say, he has a double bruit at base which indicates insufficiency.

Autopsy of case of thinning of the walls of the heart—pleural effusion, and inflammation of the coats of the stomach. Case illustrative of disordered cardiac sounds dependent, for the most part, on sympathetic derangement.—This patient, whom I was invited to see by Dr. Happoldt, since my return to Charleston, was a man *æt.* 54, who had had intermittent fever some twelve or fourteen times in his life. He had also suffered from one attack of cerebral apoplexy.

He had great tenderness and irritability of the stomach, and symptoms of effusion in the chest was diagnosed by one of the physicians (Dr. Wragg) who saw the case. The attending physician anticipated disorganized condition of the stomach, which the post mortem revealed. My opinion was that the valves of the heart were not diseased, notwithstanding the very much altered sounds, and that there was thinness of the walls of the heart. This case is quite instructive from the complication as well as for the peculiarity of sounds of the heart which were of the following character, as derived from notes taken during the course of the examination:

Signs physical.—Second sound most distinct at base; at middle both nearly similar and inverse phenomena at point, where the first sound is exaggerated with the second scarcely perceptible. No souffle, however, or prolongation.

No hypertrophy; pulse extremely intermittent and variable—1, 2, 3; 1, 2, 3, 4; 1; 1, 2; 1, 2, 3, 4; 1, 2.

Second day.—Took infusion of wild cherry bark; had been treated with iron and quinine, but even these were finally rejected; sounds at base scarcely distinguishable; those at point very similar the one with the other. The first, however, still more distinct; great variation as to regularity and force. Dr. Happoldt informs me that he has met the patient on two occasions with a natural pulse; this, however, was never the case during the visits I paid him previous to his death.

with the exception of unusual thickening of the columnæ carnes of the bicuspid valves. There was thinning of the walls of the left heart.

The mucous coat of the stomach was infiltrated with blood, very red in color, and seemed to have been much inflamed, the irritability being thus easily accounted for.

ART. II.—*Medical News in Paris. Letter to the Editors from R. A. KINLOCH, M.D., of Charleston, S. C.*

PARIS, July 2nd, 1854.

Drs. Cain and Porcher :

Messrs. Editors—I have sojourned now but two short months in Paris, and, of course, must yet consider myself only an “*enfant de famille*,” yet I have already seen a great deal, so it may not be altogether improper for me to send you some scattering reflections, connected with my hospital experience. It is as well for me to premise the remarks which are to follow, by stating that the opinions which I may here advance are the results of only a small experience, and that perhaps a longer sojourn in Paris may change many of the views that I now entertain.

My time here has been occupied almost exclusively in a professional way. With the exception of a week of indisposition, which I suffered shortly after my arrival here, a part of every day has been spent in hospitals or with medical men, seeing continually something interesting or instructive. Though knowing little of the language upon my arrival, I was nevertheless determined to take in whatever information I could through my eyes, notwithstanding that but little could be forced through my ears.

The services that I have attended more especially are those of Mons. Nelaton, at the Clinique de l'Ecole de Medecine; Jobert, at Hôtel-Dieu; Maisonneuve, at the Hospital Cochin; Velpeau (I beg pardon for having mentioned the veteran last), at la Charité; at the same time I have cultivated slightly the acquaintance of Mons. Ricord, at the Hospital du Midi, and Guersant, at the “Hospital des Enfants Malades.” I may as well, at first, give you some idea of my impressions of these several worthies in the profession.

Of Velpeau I shall say nothing; the world already knows him and has bowed to his greatness, and whatever I might say, whether to elevate or depreciate him, would be of little moment. I thank him

most heartily for what he has done, and notwithstanding one so poor as I have dared to criticise his practice at the hospital on one or two occasions, I am yet willing to lend my feeble voice in according to him the hearty "well done, good and faithful servant." With M. Nelaton I am well pleased. He is decidedly the favourite among the surgeons of Paris, and especially liked by all Americans. In his deportment, both in and out of the hospital, the gentleman is conspicuous. In diagnosis he is pre-eminently correct, and as a pathologist, sound. He is the type of a clinical lecturer, and it is admitted by all that in this respect he has no equal in Paris. His manner of bringing a case before you, his logical exactness as he analyses the points and symptoms before coming to his diagnosis and prognosis, proclaim him possessed of a mind made, as it were, for the noble calling which, it is said, he follows only for love. As an operator he is, however, not brilliant; indeed, I think, decidedly poor. I am sorry I cannot say as much for M. Jobert as I have said for M. Nelaton. Notwithstanding that he is now the surgeon of the Emperor, permit me to say that he has none of the requisites of a good surgeon. His haughty behaviour in the wards of his hospital, his almost brutal conduct to those poor devils called "internes," must create, with one of proper feeling, the most profound disgust. I was, however, even before leaving home, led to believe that at all events he was a good operator. In this respect even, I was woefully deceived. His style of operating is scarcely better than his general deportment; at least, such is my opinion from seeing him perform several operations of minor importance. As to operations of any gravity, it is said, by those who have attended his service for a length of time, that he invariably shrinks from their performance. In corroboration of this, I may state that a patient with an osteo-sarcomatous tumor of the lower jaw, has been awaiting his knife for some time; he has long since decided upon extirpation and the woman has long since left it for him to appoint the day. I have visited the operating room for some time, anxious to see his effort, but as yet in vain. Another poor creature—with bad compound fracture of the leg, which, if amputated, would have afforded him a good chance for life—I witnessed sinking day by day from profuse purulent discharge; the conservative element in M. Jobert's nature permitting him to die without making an effort to save him. In the preceding remarks I have had no reference to M. Jobert's operations upon the uterus and its appendages. In reference to these operations I must accord to him some credit; they are very well done, and I most heartily thank him for the opportunity I have had of witnessing several of his interesting operations for the relief of "vesico-vaginal fistula." M. Maisonneuve has, in many re-

spects, a nature opposite to that of M. Jobert. He is *par excellence* the desperate surgeon of Paris, ready to undertake any operation which may afford his patient a chance of life, or, as some are cruel enough to say, which will afford him a pretty good chance for death. Although the service of M. Maisonneuve is a very small one, I have there witnessed more operative surgery than in all the other hospitals combined. M. Maisonneuve is undoubtedly rather too much carried away by ambition, too fond of doing what others will not undertake, too fond of working out novelties with his knife; yet he possesses many of the requisites of an excellent surgeon. He is undoubtedly the best operator I have seen in Paris. I have often, too, had occasion to admire his expertness in diagnosis, and his promptness and decision in forming an opinion in difficult cases. Indeed, he possesses, in a remarkable degree for a Frenchman, what I would call good practical sense. I was particularly struck with this soon after my arrival in Paris, when seeing presented to him a case of obscure injury of the hip joint. I saw him at the bed side for the first time, when his intelligent "interne" informed him that the patient, besides several other fractures, had one of the neck of the left femur. So soon as I heard this I was somewhat astonished, as I had noticed that the leg was turned inwards. M. Maisonneuve immediately alluded to the same circumstance, but the "interne" persisted and said that "crepitus" was evident. "Donnez moi du chloroform," shouted Mons. M. The patient was soon brought under its influence (the chloroform here being a very different article from what we generally have) and the injury quickly examined. Mons. M. very soon worked out his diagnosis by exclusion, and pronounced the case one of fracture of the cotyloid cavity with displacement of the femur. The individual died in a few days from the effect of other severe injuries which he had sustained, when the diagnosis was fully confirmed. I had heard Mons. M. spoken of by many Americans as a mere butcher, and I visited his hospital considerably prejudiced against him, but I very soon discovered that he possessed a great deal of ability. Though I have often had occasion to dissent from his procedures, I have nevertheless gained from him many useful hints. I admired, early, one feature in his surgery, which I had noticed was wanting with most of the surgeons in Paris, viz.: his desperate efforts to secure union by the first intention, after his operation. On one occasion, after an operation for a fearful cancerous disease of the face, I saw him make use of about twenty pins for securing the edges of the extensive wound made by the dissection, and his procedure is the same after most of his operations. I went with him here with my whole heart. I have often thought,

when at home, that this was a matter too much neglected; too often do surgeons trust to those miserable things called "straps," instead of taking a firm hold upon the tissues with the bloody suture. But you must not think that the enthusiasm of youth has caused me to approve of Mons. M. simply from his being an energetic surgeon and somewhat of an expert with the knife. I have studied him somewhat carefully. and as I seek only truth, you will see that I shall be quite as ready to censure when the occasion seems to me to call for this. I will relate you one or two cases of special interest, which I select out of the many I have seen at the Hospital Cochin. Apart from the interesting questions called up by these, there will be afforded me the opportunity of completing for you the sketch of the surgeon himself.

The first, and not the least interesting case, is one upon which an operation for obliteration, or rather closure, of the vagina was practiced by Mons. M. The patient, aged about forty, suffered from a very bad *recto vaginal*, and also from a *vesico-vaginal*, fistula. The fistulæ were too extensive to hope for cure by the common operative procedures, and so it was thought best to attempt the operation of M. Vidal de Cassia, viz: to close the vagina and compel the urine, as well as the feces, to pass by the anus. This plan suggested itself to the originator from the accidental closure of the vagina in a case under his charge, following upon excoriation of the mucous membrane produced by the nitrate of silver; at least I am under that impression, but have not the book of M. Vidal with me at present to enable me to speak positively. The operative procedure was this: the mucous membrane of the labia, from the clitoris above to the fourchette below, was dissected carefully from the deeper tissues to the depth of about three-quarters of an inch. This gave, of course, two flaps, as it were, while the whole of the inner portion of the labia, on either side, presented a raw surface capable of being united the one with the other, by adhesive inflammation. By this careful dissection the first step of the operation was completed, and it now remained but to arrange the different parts and retain them in position by suture, so as to secure complete adhesion. The first object now was to unite the flaps of mucous membrane on either side so as to cover in the orifice of the urethra, as well as the vesico-vaginal fistula,

stout thread had been passed through the recto-vaginal opening and one end of it made to appear outside of the anus; the other end was now entangled and fixed to the suture of Gély, so that so soon as the arrangement of this latter was completed, traction made on the thread passing through the anus would, of course, more completely invert the mucous flaps, now affixed to each other by their raw surfaces, and draw them back towards the rectum. The thread passed through the anus was also to serve another purpose, viz.: that of assisting the suture of Gély, in proper time, to find its way out through the anus.

The next step was to bring the raw surfaces of the labiæ, occasioned by the displacement of the mucous membrane, together, and, if possible secure their perfect union. For this purpose the quilled suture was made use of, the threads being passed very deep, and brought out some distance from the free margin, and secured in the usual way over pieces of elastic bougie. Then, thirdly, the free margins of the labiæ, where the integument and raw surface, resulting from the dissection, joined each other, were closely connected by the continued suture. Thus you perceive there were three kinds of suture brought to bear at different depths; first, the suture of Gély, for uniting the inverted margins of the dissected mucous membrane; next, the quilled suture, for retaining in perfect apposition the bloody inner surfaces of the labiæ; and thirdly, the continued suture, to aid the quilled by operating upon the free margins simply, upon which the quilled suture could act only imperfectly. As you may imagine, the most perfect apposition was then obtained, and the result was all that could have been expected. It is now some three weeks since the operation, and the patient's condition is apparently good. Complete union has been obtained, the urine passes by the anus, and of course the fæces also. At first the urine came away per anus, guttatim, without being able apparently to retain it, but now she has full power over the rectal sphincter and does not even suffer, as I can learn, from excoriation produced by the irritant fluid thus compelled to take an abnormal direction. It remains yet to see how materially benefitted will be the patient's condition. I will have to leave the case here at present, but will advise you of the further developements.

The next case I would remark upon, is one of "*fistula in ano*," made to possess a peculiar interest from the mode of operation adopted by Mons. Maissonneuve. The case in point was one in which there were

presented by one who has suffered for a long time from this common and distressing affection. Now I had always entertained the notions of Sir Benj. Brodie, in reference to this affection, and you are aware, Messrs-Editors, how much the doctrines taught by him have accomplished towards simplifying the operative procedure necessary in these cases. Instead of the free and extensive incisions practiced previously by surgeons who insisted upon following up the fistula throughout its extent, no matter how far the external orifices was from the anus, or how far the internal orifice was up the gut, the operation was reduced in most cases to a simple division of the sphincter. I had, nevertheless, observed how common it was for these operations to fail, and more than once it had struck me that Sir B. had not made his ideas of treatment conform altogether with his pathology of the disease. For instance, he recognizes two causes as operating in preventing the healing or closure of fistulae in the neighbourhood of the anus—the one the contractions of the sphincter muscle; the other, the entrance of fecal matter, or other contents of the bowels, into the internal orifice of the fistulous track; and yet he contends that in a majority of cases a simple division of the muscle will cure the disease, and recommends that in those cases where it would require an extensive incision to lay open the whole fistulous track, that the sphincter should simply be divided. Now I say that though these thoughts had previously passed through my mind, I, nevertheless, still regarded Brodie as the best authority upon the subject of fistula in ano, and relied much upon his experience. I was thus somewhat startled, or even terrified, in seeing Mon. Maissonneuve adopt the most energetic operation that can be imagined upon the case in point. He not only laid freely open the whole of the fistulous tracks, but he practiced the most extensive *excision* of parts around the contour of the anus; every portion of the callus tissue was removed, and even much of the healthy integument intervening between the different fistulous openings. In short, one could not imagine a greater deviation from the principles of Brodie. I have never yet seen a poor creature so horribly

mild operations of Brodie fail, it is as well to know how much cutting in the neighbourhood of the anus a patient can stand; and this one certainly will learn who witnessed many of these operations after the manner of Roux. The patient alluded to is doing well; that is to say the extensive wound is granulating finely; he suffers from no bad symptom; and I can not believe when the parts shall have entirely healed, there will be found any fistulous track.

CASE 3.—*Operation for the radical cure of inguinal hernia.*—An operation of this kind is, of course, never without interest; but the case in point is especially interesting from the success claimed to have attended the procedure. M. M., read an account of the operation before the Academy, not long since, and presented the case as a perfectly successful one. The operation, which I had the pleasure of witnessing, was that of invagination. The surgeon, with the fore finger of his left hand, pushed a portion of the scrotum up through the external ring fairly into the inguinal canal, anterior to the chord. All the tissues exterior to the point of the finger were now carefully divided by an incision of about three quarters of an inch in length, until the knife reached the finger in the inguinal canal. This wound thus had traversed all the tissues which cover a hernial tumor that has not yet made its exit though the external ring (indeed the point of the finger in the canal was a sort of representative of such a tumor) and also the invaginated portion of the scrotum retained in the canal by the finger. The surgeon then, with the aid of assistants, passed several sutures through the entire thickness of the lips of the wound on either side, including the invaginated portion of the scrotum. The double ends of these sutures instead of being cut off, were now gathered together and passed through the wound, into the canal, and from thence, as the finger—which up to this time had not been moved from its original position—was withdrawn, were drawn through the entire invaginated part, when the ends rested upon the lower portion of the scrotum, or were pendant between the patient's thighs. This disposition of the threads appeared to me (M. M. did not remark upon this in his clinic) to serve several useful purposes. In the first place a conduite was thus kept open for the inflammatory secretions; then the threads acting as an irritant, helped to develop the requisite amount of plastic inflammation, and thirdly, filling up the invaginated tissue, somewhat as the finger of the surgeon did during the operation, this was made to plug more effectually the external ring and the inguinal canal, so that during the progress of the plastic inflammation this (the invaginated portion of the scrotum) was more likely to become united generally around the contour of these

structures. At the expiration of four or five days the sutures were cut away and removed, and then a crayon of the "nitras argenti" was passed up into the invaginated portion of the scrotum, now fixed in its abnormal position by the adhesion which its apex had acquired with the ring and crura, and freely turned about so as to excoriate the entire surface; after this, a compress and bandage was applied to keep the excoriated surfaces in contact, and thus procure their union. The result was as desired, and in a few days more the surgeon completed his operative procedures by a slight incision which partially disconnected the invaginated from the other portion of the scrotum, and thus allowed the parts to assume a more natural appearance. As I have previously intimated, the case was considered radically cured, the hernial tumor cannot be made to descend by the coughing or other movements of the patient; but whether this fortuitous state will continue, is a question yet to be determined. I should have said before that this operation was undertaken on account of the particular request of the patient, and not from the recommendation of the surgeon. Permit me, also, to say, in conclusion, that I have not reported the case with the view of advocating operations for the radical cure of hernia, as I believe, from the nature of things, that these operations must continue, in almost all cases, to be failures, and consequently ought only to be attempted in very exceptional cases.

CASE 4.—The fourth case, upon which I would remark, was one of compound dislocation of the ankle joint, in a very robust, athletic Frenchman, in the prime of life. The tibia projected through the soft parts for about two inches and a half, the fibula being, of course, fractured. M. Maisonneuve attempted to save the limb. The patient was fully chloroformed, and reduction effected without resection, a thing which could not possibly have been accomplished previous to the introduction of anæsthetic agents. The wound in the soft parts was closely brought together by straps, and Dupuytren's apparatus, for the treatment of fractures near the ankle was tightly applied, the splint being placed upon the inner side of the limb. On the morning of the third day I visited the hospital, and found, as I had previously predicted, that the foot and leg were in a gangrenous state. Of course, amputation was

evident in the hospital. He, furthermore, observed that death from "*purulent absorption*" after amputation had been so ominous in Paris, that he intended to modify his operative procedure in the present case, to guard, as he believed, against such a fearful result. In the circular operation usually practised upon the leg, he remarked that the vessels were cut upon a level with the bones, and, of course, higher up than the section of the soft parts or flap; he thought that this favored purulent absorption, (how, I cannot understand, unless he believed that pus forms about the flap and gravitates into the open mouths of the veins cut upon a level with the bone,) and so he modified his operation to suit this *ingenious theory*, as follows:—A longitudinal incision was made over the fibula, beginning at the point where he intended to saw the bone, and continued downwards for about five inches; the soft parts were now closely dissected from the bones, and several inches of this resected with the chain saw. The same was done for the tibia by means of a second incision over that bone, the vessels in both instances being carefully avoided. The whole of the soft tissues, integument, muscles and all, was now divided by a circular sweep of the knife, and the limb at once removed. This case furnished me with ample material for reflection, and I hesitate not to say that I condemned the proceedings of the surgeon in toto. In the first place, it lowered M. M. very much in my estimation to hear him talk of the bad weather, and the previous disposition to gangrene manifest in the hospital, *after* he had adopted a particular course of practice, and lost by the venture. This was a matter to have been noted before hand, and should, in my own humble opinion, have decided him to adopt the other alternative in the first instance, and have amputated the limb. Again, was not the mode of adjustment made use of in the first place enough of itself to develop gangrene? Would such have been the practice in an English or an American Hospital? I think, or at least I hope not. Still further, I can see very little sense in the theory which M. M. says induced him to modify the usual operative procedure in amputating the leg. And what was the consequence of such modification? The patient, already in a very critical condition, was kept upon the table for twenty minutes or more instead of for five, all that would have been necessary in the common method of operating, and the soft tissues were pulled at and bruised, in the efforts of the surgeon to complete the resection of the bones, and consequently the stump was more liable to take on gangrene than if it had been formed simply by a clear division with the knife. But the saddest picture is yet to be painted. Upon visiting the hospi-

tal the second day after the amputation, I found the poor unfortunate firmly lashed to the bed, in consequence of the violent traumatic delirium from which he had suffered; a cold perspiration bedewed his skin, and his pulse flickered at the wrist. When the dressing was removed from the stump, I saw that this had been partially attacked with gangrene, although it seemed not to be progressing. And now came another novelty to my American eyes. An India-rubber bag, made to fit like a cap, was applied to the stump, and then by means of a spring, connected with a tube that communicated with the bottom of the bag, the air was exhausted from its interior, so as to make it press closely upon all parts of the stump. What was the result of such pressure upon a stump possessing as feeble a circulation as this did, I can not report, as I saw the patient no more; was it not reasonable though to anticipate an extension of gangrene, provided the patient lived long enough to admit of this result. From his appearance when last seen, however, it was evident to all that he would not survive many hours. I am not able to speak particularly as to the termination, as the hospital was closed that day to undergo some necessary repairs, and will not be open again for some weeks.

And now, Messrs. Editors, I must draw to a close. I have much more that is interesting that I might communicate, but I fear that my letter has already been drawn out to a tiresome length. I have written hastily, and, as you see, have ventured to express my opinions freely; of course these must pass for what they are worth. If I have been somewhat harsh in my criticisms upon men and their actions, it is not because I am ungrateful, or insensible to the advantages I am permitted to enjoy amongst the liberal people with whom I now live, but simply from a desire to follow truth. I see much to admire, yet I must be permitted to say that I can never become altogether a Frenchman, and shall probably return home as much an American, in many matters connected with my profession, as I was before leaving my native shores.

Respectfully and truly yours,

ART. III.—*Compound fracture of Os Frontis, with depression of Spicula, successfully treated by Trephining.* By J. J. CHISOLM, M.D.,
Lecturer on Surgery, etc.

I HAVE been induced to put the following case upon record, although it has no very great intrinsic interest, except as connected with operations upon the head, which are always considered serious accidents from the immediate proximity of the brain and its sensitive envelopes. Besides, such cases might be useful to others, in presenting crude elements for statistical tables, which, if carefully collected, are of great importance in establishing the prognosis and proper treatment of surgical diseases.

Trephining is a very serious operation, and is now only called into requisition in very serious injuries, therefore we would expect to find the results not always favorable. Nélaton says that there has not been a *single* successful case of trephining in the Parisian hospitals for the last fifteen years. They have all terminated fatally. Erichsen reports six cases operated on in the University College Hospital, by Cooper, Liston and himself, only one of which was successful. Dr Lenter has collected forty-five cases operated on in the New-York Hospital, thirty-four of which died. In a report made by Drs. Laurie and King of fifty-one cases, forty were fatal. These reports show us the danger of the operation, which should never be made to complicate injuries of the head unless imperiously demanded.

Frank, a negro boy, aged 13, of a good healthy constitution, chimney-sweeper by trade, was kicked in the head by a horse on the evening of the 16th May, 1854. He was picked up insensible and conveyed home, where he was found by his physician, Dr. Pritchard, in a coma, toxic condition. He prescribed stimulating enemata, administered a dose of sulph. magnesia, and applied a large blister between the shoulders. When I saw him on the morning of the 17th he was perfectly sensible, complaining of no pain, pulse good, skin pleasant, tongue clean and had had one operation during the night. The only morbid symptom which we discovered was incessant thirst, crying for more water whilst he was emptying a tumbler which he held in his hand. He had urinated two or three times. On examining the head, I found a gaping wound of three inches in extent an inch above the right orbit, parallel to the superciliary ridge. The skin and soft parts had been driven up for some distance by the shoe of the horse forming a large flap, covering an extensive surface of denuded bone. When the finger was passed

under this flap to the extent of one inch, an extensive crushing of the bone with considerable depression was distinctly felt. We determined that the depressed fragments required elevation, and after prescribing cold cloths to his head, and ice instead of water to quench his inordinate thirst, we left him to return in a couple of hours to perform the necessary operation.

By uniting the wound by a perpendicular incision of two inches and dissecting up the two flaps formed, the entire denuded bone was exposed. The fracture in the external tablet was stellated, the longest ray was over two inches in length and was parallel to the orbital ridge, most of the radii running from this as a centre downwards towards the eye. The depression existed from this long fissure. When a segment of bone had been taken out, the dura mater was concealed by a clot of blood in which five sharp spiculæ of bone were found embedded. These were mostly triangular, the base being attached in some instances to the inner tablet, the apex driven downwards into the dura mater. Some of these were three-quarters of an inch long. After the removal of the spiculæ, the dura mater being detached for a considerable distance round the wound, the depressed portions were elevated, a good deal of force being necessary. The wound was freed of clotted blood, the incision was united by sutures, and the patient left quietly sleeping under the effects of chloroform which had been administered. At our afternoon visit the patient was found perfectly sensible and comfortable. Pulse one hundred, skin cool, complains of no pain, but still has incessant thirst, drinking every five minutes and constantly crying for water. The wound has been dressed with compresses dipped in cold water.

May 18th. Morning visit, skin chilly, pulse small and feeble, ninety-two, complains of head. He has not slept since yesterday afternoon; bowels opened during the night; tongue clean; still incessant thirst.

Rx. Brandy and arrow-root to be given to him instead of ice water when he calls for drink; cold cloths to be continued on the wound.

Afternoon visit—patients condition slightly improved; he complains of his head, which is very hot, and the soft parts are so much swollen that his eyes are closed from cedema. The wound is healing kindly with no discharge; pulse much fuller, one hundred and four, skin pleasant. He is quite rational and has just awoke from a long sleep. His thirst is still great.

Rx. Continue cold cloths to head, ice and arrow-root without brandy, 3i sweet spr. nitre at bed time.

May 19th, 7 A. M. The patient has had a good night's rest; slept well; skin cool; pulse one hundred; tongue clean and quite moist; he

complains of his head and eyes, which are still closed. The incision has healed by the first intention; the first dressing remains firm, and as the straps encircle half of the head, they afford excellent support to the soft parts. The wound is suppurating and granulating freely; he still has inordinate thirst, which we cannot account for except from habit, as he has no febrile symptoms and mouth very moist.

R. Cold water dressings continued, also arrow-root; ordered a warm bath to open the pores of his skin, which, from his professional occupation, have been long closed with soot.

In the afternoon we found a progressive improvement; pulse eighty-eight, normal; skin clean and pleasant; swelling of face subsiding, eyes partially opened. His bowels have not been moved for two days; prescribed an enema of salt and water.

May 20th. Patient quite bright and makes no complaints; his thirst is at last quenched; the enema gave him one large evacuation. All swelling disappeared from face, eyes widely opened. He has rested well during the night; wound granulating very freely.

May 21st. Continues improving, suffers only a slight soreness in wound, which is contracting very rapidly. He eats hominy and arrow-root.

May 22d. Patient sleeps well and eats heartily; has no pain; pulse seventy-six; bowels costive.

R. Cold water dressings changed for poultices over the wound; a compress and bandage protects the incision from the poultice.

May 24th. Patient considers himself well; he gets up, walks about and even makes up his own bed. Wound cicatrizing rapidly under the application of poultices. He has been costive for three days.

R. Castor oil ʒss.

May 26th. The oil operated upon him three times; continues improving; he is most of the day out of bed. In drawing the compress and roll of bandage firmly over the trephine hole, laudible pus escapes from the uncicatrized portion of the wound indicating a fistulous passage.

June 6th. The wound has completely healed; pus still escapes, on pressure, from a small fistulous orifice. He has a considerable projection of the head on the injured side from deposited lymph and hypertrophy of soft parts. He is no longer confined to the house.

June 10th. The patient being well enough to go into the street, pays us daily visits at our office. The lower fistulous passage has closed up, the side of face is somewhat swollen.

June 24th. A second fistulous opening has formed higher up in the cicatrix of the incision, which discharges pus in small quantity on

pressure. A probe can be passed an inch in extent towards the trephine hole, around which was a portion of bone denuded of periosteum by the kick, and from which the dura mater had been separated by the internal hemorrhage. This portion feels rough under the probe, and will, no doubt, exfoliate. Sometimes the fistulous orifice closes, pus collects under the flap, and swelling of the upper portion of the face is brought on; the skin becomes thinned at the old opening, and, if touched with a lancet, a large collection of pus will be emptied, and all the swelling rapidly subsides.

For some weeks I have lost sight of the case, but was told to-day, July 21st, that the boy is so well that he has resumed his occupation of chimney-sweeper. No fragment of bone has as yet escaped from the fistula.

The most striking feature in the case, is the small amount of febrile excitement during the treatment. The greatest degree of excitement of the pulse being one hundred and four, which lasted only a few hours, accompanied throughout by a cool skin and clear tongue; and, also, the entire absence of meningeal inflammation, which was so much feared, as the dura mater must have been more or less injured by the sharp apiculæ of bone driven down upon it.

NOTE.—Aug. 1st, I have just seen the boy, who is quite well. His head gives him no trouble; most of the deformity has disappeared, and there is no longer any discharge, the orifice having closed. He continues his occupation of chimney sweeper.

ART. IV.—*Case of Tetanus.* By M. GREENLAND, M.D., of Charleston, So. Ca.

JUNE 1st. I was requested by Mrs. C——, to call and attend her son, a youth, aged 13 —, she said he had a very severe cold. On paying the visit, I found him in a kneeling posture, and complaining of soreness in the lumbar and sacral regions; there was no pain upon pressing them, or any extreme tenderness; no febrile action; intellect clear; tongue foul, and pulse indicative of no inflammatory action. Ordering him to stand, I found that he was unable to put his left foot to the floor, its appearance being that of a dislocation of the femur on the ramus of the pubis; the toes just barely touched the floor—the heel being strongly drawn up—the sole of the left foot was turned inwards and upwards, the toes pointing towards the tarsus of the right side; the thigh projecting from the median line of the body, and there was considerable projection of the adductors and iliacus muscles; there was

likewise rigidity of the abdominal muscles. On examining the foot there was some tumefaction over the scaphoid and cuboid bones, with considerable projection of the cuboid, as if partially dislocated, the superior facet, near the scaphoid being very much raised above the level of the other bones composing the arch of the foot, and this spot when pressed, produced general spasm of the limb. I immediately endeavoured to gain the history of the case, which, with much trouble, was so far elicited, that six weeks ago he had received an injury by falling while playing on some bales of cotton. It was impossible to learn any thing of the injury received, but that he was unable to walk, and complained of pain in the left leg. From such a history it was impossible to draw any inferences of a correct nature, as regarded the primary lesion. Whenever the paroxysm recurs, he says that it begins in the painful spot, over the scaphoid and cuboid, and extends upwards towards the left lumbar region, and there, and then, the pain is excruciating. There was no dislocation of the hip, or injury to the knee, nor could there be detected any fracture of the bones of the tarsus. I perceived that a disease of the nervous system was before me, and its cause most obscure, ordered—

Proto. Chlorid. Hydr. grs. x.

P. Rhei. grs. xii. in. ch. No. 8—one every 2 hours.

℞ Chloroform.

Ammon Fort.

Tr. Cantharidis.

Oleum Olive a a 3 i.—rub the back.

Leeches to foot, followed by warm fomentations.

Evening visit, 10 o'clock. Suffering severely; general spasms of left leg; pulse frequent; leeches drawn well; tr. opii. gtts. xxv. to be repeated every hour until relieved.

June 2. Rested better towards morning; bowels opened; tongue cleared; had but one paroxysm of pain since last evening; the foot still being in same position; no increase in symptoms; tr. opii. to be given every hour, as first evening, if required.

June 3. Slept better; pulse fuller; paroxysm not so painful, occurring about every 12 hours: tr. opii. continued.

June 4. The same; tongue slightly furred; dose of oil.

June 5. Slept well; pulse natural; tongue cleaner; all swelling of the tarsus gone; the pain on pressure over the scaphoid and cuboid bones endurable, put under chloroform; the foot was placed in position, a pad and bandage applied; tr. opii. to be continued if required.

June 6. Remained easy until middle of last night; paroxysm coming

on, the bandage was removed; tr. opii. freely given; the foot has its old position; pulse more frequent; tr. opii. continued, as also the friction with liniment.

June 7. Had two exacerbations during the last night; his general symptoms good.

June 8. No better; Dr. Bellinger requested to see the case; considered very obscure by him; ordered blister to sacrum and lumbar vertebræ; tr. opii. continued.

June 9. Blister drawn well; no increase in spasms; no change in contracted muscles; pulse small and frequent; R^x Opium half gr. each hour.

June 10. Has not slept so well; some rigidity of the abdominal muscles, with difficulty of evacuating the bladder; solution of morphia, a tea spoonful every hour until relieved.

June 10. Evening visit. Bladder has been freely emptied; feels considerably better; has slept some through the day.

June 11. Pulse weak and frequent; no increase in spasms; ordered weak brandy and water; morphia continued.

June 12. Same as day before.

June 13. This morning had a very long and painful paroxysm, during which there was convulsions of the limb and great rigidity of the abdominal muscles; pulse very small and frequent, about 110. Brandy to be repeated; morphia continued; diet as usual.

June 14. Slept better; no recurrence of spasm; complains of intense pain in inner part of left thigh, it is very painful on pressure; pulse 90 and weak; morphia repeated—emp epis—to left groin, extending down thigh.

June 15. Blister drawn well; not so painful; pulse fuller; slept better last night; had one paroxysm during the night; position of foot the same; the leg cannot be extended on the thigh, the biceps and semimembrinosus and tendinosus being strongly contracted; ordered to produce ptyalism; calomel gr. ii.; p. opium gr. 1, every two hours; morphia to be given in intervals, if required.

June 16. Slept well; some soreness of limb; continued powders.

Evening visit. Mouth sore; no mercurial fœtor perceptible; tongue furred white; no return of spasm.

there is perfect relaxation of all the muscles; the foot is in its natural position; there is no pain complained on pressing the part over the cuboid, which is still prominent, and can be replaced with ease; foster of breath; tongue furred white; calomel and opium continued.

June 18. Free from all pain and spasm; slept well; appetite good; usual diet; brandy allowed; calomel and opium powders continued.

June 18. Half past 11 P. M. Spasms returned involving superior extremities, with partial closure of the jaws; difficult deglutition; body bedewed with perspiration; pulse very frequent and contracted; tr. opii. in tea spoon doses freely given.

June 19. Spasm had returned, but did not remain on long; soreness along the cervical spine; difficult deglutition; mercurial foster strong; stiffness in the jaws; pulse weak and frequent; the powders continued; Emp. Epis. to extend from occiput to fourth dorsal vertebra; brandy and water; morphia if required.

June 20. Spasm has not returned; blister drawn well; complains of soreness in muscles of arms and legs; pulse frequent; weak; tongue dark furred; some nervousness; slight wandering; powders of calomel, quin. and opium.

Evening visit. No worse; no return of spasm.

June 21. Had in the late hours of the night a very severe attack; he is now completely convulsed, the form being that of opisthotonos; pulse scarcely perceptible; chloroform inhaled; in a few moments there was complete relaxation; breathed more freely; pulse increased in volume, less frequent; surface covered with clammy sweat; chloroform to be inhaled should the attack return.

About 1 p. m., he was seized with another paroxysm like the one of the morning, and died during the attack. No post mortem was allowed.

In the history of the above case there are a few points on which I would make a few remarks. The first is, what was the cause of the disease? Was it of centric or eccentric origin? There was no apparent lesion, nor was there elicited from the boy, or parent, any thing which could prove that there was a lesion to the spine. I am inclined to think that there was a partial laxation of the bones of the tarsus, of the left extremity, which, being unnoticed, and not treated, produced irritation, this producing contraction, by exciting the motor nerves. The contraction of the flexor and tibial muscles was permanent, there never was, from the time I first saw him, until the 17th day, with the exception of the 5th, when, under chloroform, I applied the bandage and pad, any relaxation,

even in the slightest degree. The bandage was applied under the above idea of the exciting cause—it relieved the pain and abbreviated the spasms, but it did not prevent their return. The appearance of dislocation, it is true, might have been produced by the constant action of the muscles, stretching the ligaments, and thus giving to the cuboid a prominence which it would not have had under other circumstances. If this had been an original lesion of the spinal cord, or membranes we would have had, in all likelihood, high febrile action, general convulsions, or paralysis. The second point is the peculiarity of its course. Tetanus is a disease, the termination of which is generally of a fatal nature, and that, too, in a few days, extending most generally as far as the ninth; but here we have a case lasting for eight weeks, before there is an increase or diffusion of the spasm; and for six of those weeks without treatment, and just as soon as the disease seemed to be yielding, and all are buoyed with hope, the disease returns with redoubled force, and proceeds in its fatal career. When it assumes a chronic form, such as this would be considered, there is every chance of recovery, independent of any form of treatment; but this is one of the many exceptions to our medical generalizations. As regards treatment, opium in its various forms was freely given. To the mercury I am inclined to attribute the effects of relaxation, produced on the 17th day, although the recurrence of the spasm would tend to render negative the idea. Cannabis Indica was not used, for having observed its effects in several cases, when it appeared to be worth as much as simple cold water. Chloroform was used by me, and in the last stage freely. I have no hopes that it will cure a disease, so incurable, but thrice happy we, if by it we can assuage the most intense pain, and render the pathway to the grave serene.

July 28th, 1864.

ART. V.—*Case of Abdominal Wound.* By THOMAS P. BAILEY, M.D.,
North Santee, S. C.

THE following case, although not presenting anything very peculiar. I do not think would be devoid of interest to the readers of your excellent Journal. I have, therefore, transcribed it from my note-book for publication, should you deem it worthy of an insertion.

I was summoned in haste, in the afternoon of the 19th of May, 1868, to see Mr. G. H., an individual of large family and scanty means.

On my entrance I found my patient in a state of syncope; some stimulants were administered and on resuscitation I received the following statement: It appears that while engaged in hunting cattle in the swamp, he was dragged from his horse by the limb of a tree, and fell upon a dry cane, which penetrated his abdomen. On examination, I found a contused and penetrating wound in the left iliac region, near the abdominal ring, which was dilated by means of the bistoury and thoroughly probed, the probe following a direction obliquely downwards and inwards, coursing between the deep layers of abdominal muscles, but I could discover no portion of foreign body, and the patient informed me that he had extracted the principal piece of cane, which projected externally, before coming home. A simple compress and bandage was applied, and I left him comparatively comfortable.

May 20th. Wound somewhat swollen and inflamed around edges, with slight ecchymosis along its track; slept very well during the night; bowels open; continue dressings; visit in the evening, had hot fever and somewhat delirious since morning; abdomen tender on pressure, and tympanitic. Wound again probed, when a small piece of cane was discharged with a large quantity of purulent matter; discontinued compress and bandage; to have the following—

R. Hydrarg protochlor, grs. vi., pulv. opii. grs. iii. M.—dividend. in pulv. No. 6. One every two hours.

21st. Abdomen not so tender on pressure; wound not so painful; suppuration still continues, but can detect no further extraneous substance; bowels open; had some fever last night; continue powders; apply poultice to wound.

22nd. Much the same as he was yesterday; wound somewhat contracted; purulent discharge still continues profuse; discontinue powders; to have nourishment in diem.

24th. Much the same; wound presenting a fistulous appearance; complains of pain about spermatic chord and testis; continue as before.

28th. Has fever towards evening; wound much the same; pus more sanious in appearance.

Injected fistulous cavity with solution argent. nit. 10 grs. to ℥i.; to have the following mixture: quinine sulph. grs. x., acid sulph. gt. lx., aqua pura 3 viii, sumatur cocthearia magna ter die.

From this date until July 1st the same treatment, with some slight variation, was pursued, being cauterized on several occasions, when he consented for me to lay it open. By means of a groove director and bistoury the whole track, as far as the probe terminated, about two inches, was laid open freely.

July 3rd. Wound more healthy; pus seems to be discharged in the direction of a line drawn from orifice of the wound to the pubis; to have poultice applied; continue quinine, etc.

5th. From direction and continuance of purulent discharge, was led to explore wound by dilating it with bistoury towards the pubis, and on probing discovered that there was a foreign body present; on application of forceps I extracted three pieces of cane, two of them one and a half inches long and one about half an inch, with a piece of shirt and pants. From this date my patient began to mend slowly, his recovery being retarded by an attack of intermittent fever, but by frequent cauterizations and the use of tonics by the 25th July he was able to walk about.

Remarks.—This case has presented several features worthy of note. First, the fallacy attending the examination of a wound of the abdominal parietes. It will be seen, on reference to the above, that the wound was probed carefully on several occasions. The reasons to be assigned for not discovering the portions of cane before, is the elasticity of the abdominal parietes, which causes any body impinging thereon to assume a direction contrary to the force applied, as for instance a musket ball. From the statement of the patient and the appearance of the pieces extracted, I was induced to believe that the parent cane was split at the end, so that on his falling one portion took a direction obliquely downwards and outwards a little below the anterior superior spinous process of the ilium, whilst the other took a direction downwards towards the pubis, forming an angle, the acute part of the angle being the point of entrance, and the two limbs extending, one towards the ilium, the other towards the pubis. Secondly, it is rather surprising that there was not a greater tendency to peritoneal inflammation. Thirdly, it

NO. II.

ART. VI.—*Medical Heads and Medical Life in Paris.—Unpolished Sketches from personal observation.—The Chirurgien-in-Chief of Hotel-Dieu!*

Faulx. "For he is but a bastard to the time
Who doth not smack of observation."
King John.

"Princeps in prælium ibat."

l'arrose laral Savarès!
(The physician that slays is Death!)

THERE is an old Surgeon and septuagenarian just dead at Paris, who always in life 'went first into battle,' who next claims my attention in sketching a very few of the most prominent of the medical celebrities of France. And when I shall have described M. Roux, having finished with VELPEAU, we shall have disposed of the two Ajaces; the one chief of La Charité, and the other of its rival Hotel-Dieu.

The Surgeonships of these great Institutions are pitted against each other, for it is now *sub lite* whether Velpeau, whose friends like Agamemnon's would have him "as amply titled as Achilles is," should be transferred to Roux's place, for fear some may regard it as a promotion rather than a removal, and hence imply that the Coryphæus of La Charité had heretofore been occupying a subordinate position.

But certainly the Hotel-Dieu, built on the Isle and in the ancient *Cité* where Constantine and Julian walked, is more classic than her sister on the verge of the Quartier Saint Germain. She has more of antiquity, proximity to the venerable Abbey Notre Dame, and many associations connected with Saint Louis, and the old bishops and reverend prelates of early times. Kings and pontiffs patronized and endowed her, and she was taken into the bosom of the Church as one of the most important and sacred of her institutions for ministering to suffering humanity, and to her own influence over the minds and bodies of men, at a season when they are most prone to entertain humble thoughts, and to yield obedience to holy and ecclesiastical ordinances.

M. Roux was a large, blunt, free spoken gentleman, though never unamiable, with rather a *brusque* air, who lorded it over his juniors and all the younglings of Hotel-Dieu, and who even sometimes did not confine the theatre of his priorities to this spot, but in all the medical and scientific gatherings of the French capital, to the attendance upon which

in his later days he was notoriously addicted, appeared *primus inter pares*!

This, however, resulted from no assumption on his part. Roux had never the presuming 'greater part of which I was' air of an ignorant upstart or novice, whose only passport was assurance, but he stepped to the first seat, which all readily and willingly yielded to, and left vacant for him.

He, skilled in all the lore of the 'antiquary times,' was veritably the Nestor of surgery, and had been recognized as such for more than a generation past. He was, indeed, the connecting link between the age of Dupuytren, Bichat and Lisfranc, and that of Velpeau, Andral, Lennec and Louis. Himself the friend of Bichat, he had been bold enough to compete with the first mentioned on more than one occasion, and though defeated, even in retiring he bore off fresh laurels. The Baron Dupuytren, his senior by two years, had to guard his place against this daring young aspirant, who had gained experience and some fame in the camp, and who came to Paris in the midst of all opposition, determined to succeed. Though not winning it by *concour*, he has for near thirty years held the post of highest honour in one of the oldest and most renowned Hospitals of Europe. But, what constitutes an important distinction, he has not held it simply by right of priority or occupancy, like some old drone, whom a tide of successful fortune has thrown up for once, and who keeps it for aid; or worse, if that were possible, like some unlicked and pampered cub, full of *niaiserie*, and utterly incompetent to manly duties, who is fed and supported by pap, or bolstered up by wealth-blown friends, until he actually has had time to grow into something like competency. Roux has every day defended his right in demonstrating his fitness and ability. He has quailed before no surgical operation within the reach of human interference. He has taught that there was never a necessity to remove an unfortunate from *within* the surgical wards of the Hotel-Dieu to seek aid from *without*. If the operation was declined, there was a sufficient

of advised age." He was lively and jocose, and bore himself, for one of his time of life, bravely. In his wards he was free and familiar with his assistants and students. But, withal, he had the slightest shade of mannerism. He was given to all kinds of motions, both of face and hands; I have seen him when M. Gosselin was lecturing in his place making every variety of wry faces, twirling a pair of surgical scissors about, performing imaginary operations with it against the air, and actually attracting more attention from the lookers on, in his temporary capacity as second, than did his junior who all the while was speaking so much better than Roux ever could.

M. Roux had the most execrable delivery possible to be conceived of. I have not yet made up my mind as to whether the Frenchman understood his language. A young man, just come to Paris, who thinks he knows something of French, should never have gone to hear him lecture for years after his arrival. Abundantly fluent, he yet poured never forth any of the "rills of oily eloquence." What he said was good, no doubt, but he mumbled his words so—they came out so disguised from his tongue—his voice did not, like the Trojan chieftain's, when he found himself in the realms of Pluto, stick to the roof of his mouth, but it kept pouring out a stream of gutturals, inextricably interwoven and merged together, and to you neither conveying parts of speech, nor signs nor symbols of them. It was so different from the clear, distinct, emphatic enunciation of his youthful substitute, that you did not believe it was the same French. All that he said was a confused, inextricable mass of sounds united together, and utterly without significance; the last time that I heard him, at the end of near two years sojourn, it was, to me be it understood, the same almost incomprehensible *vox et preterea nihil!*

In running through the main points of M. Roux's career, and tracing his most distinguishing features, I find myself arrested at his 22nd year, when Bichat dying at 32, he succeeded his famous master, whose collaborator he had been during life—writing entirely for him the fifth volume of the *Anatomie Descriptive*, and actually filling his chair as lecturer to a numerous audience.

He has since often taken up the pen as author, and whilst his works do not occupy the very foremost rank, yet their author always did.

M. Roux has done bold things in surgery; he has very much improved, amidst numerous other contributions and suggestions offered, the mode of remedying and correcting deficiencies of the palate.

But it is in cataract that Roux has immortalized himself. His personal and comparative operations for elevation, depression and extraction of

the cataractous lens would almost furnish the bulk of the statistics of this disease, however MM. Sichel and Desmarres may see their hundreds of patients a day in the Rue Antoine DuBois and the Rue Christine.

M. Roux has been eminently hardy in his characteristics as a surgeon. It was he who loved surgery so much and who exclaimed, when Le Roy d'Etiolles and Civiale were crushing stone and lithotritry was fast taking the place of lithotomy, "Alas! after all the labour and all the nice dissections, shall there be no more Lithotomists?"

The only man in Paris, my observation teaches me, as bold as he, and now I speak simply of surgical audacity, is Maissonneuve. Like him, no conjuncture of untoward circumstances intimidates; in the midst of the most trying crisis of the operation, when dangers thicken, disaster stares in the face, when arteries are spouting, pulses ebbing and life itself on the wane, and whilst dismay is depicted on all around, he alone—cool, calm and dispassionate—seems by some master stroke or unseen resource to snatch victory from defeat; he seems actually to rejoice in the combinations against him, ever

"Most master of himself and least encumbered
When over-matched, entangled and outnumbered!"

He preserved his steadiness and nerve to the end, and he attempted any of the capital operations to the last days of last spring, when his fine portly figure, at 74, was seen radiant with good humour at the clinics of the Hotel-Dieu.

M. Roux was always in good odour with all his professional brethren of the proper stamp and he was a favourite every where. He was a person of the most unbounded *bon homie*, which extended even to his internes and students.

Completely embraced by some one from behind, as happened to the writer whilst standing by the bed side, you would find yourself locked in the arms of the portly and urbane Roux, who took this affectionate way of arousing you from some possibly profound reflections or "brown study," and of reminding you that you were in his way.

The funeral orations over his tomb, and the eulogies at his death, which only occurred in March last, were delivered, we perceive, (in the *Revue Médico-Chirurgicale de Paris*, edited by M. Malgaigne,) by the best men of every rank in Paris—almost as much honour as gained M. Arago, who, like Chomel, from his staunch and unyielding republicanism, had the additional weight of political disfavour.

Hear what M. Velpeau said, as organ of, and speaking in the name

of the Academy of Sciences, when strong emotion seized upon orator and auditory, as he recalled the ancient friendship which had long bound him to M. Roux, and when he addressed to him these last adieus, which are so eminently in the French style that we can not spoil it by a translation. The manner is indeed so truly French that were not the speaker Valpeau, whose intimacy with, and friendship for Roux is known, and who evidently speaks from the "lowest chest," we should be at a loss to know whether the tears were Crocodilian or not. Here it is :

"Maître vénéré, ami fidèle, tu n'as rien à craindre de la vie céleste : tu as bien servi tes semblables, ta carrière a été noblement, largement remplie ! Ta mémoire ne périra pas ; ton nom à jamais honoré occupera une belle, une grande page dans l'histoire des savants, des hommes utiles.

"Du séjour des élus, Dieu permettra à ta grande ombre de jeter par moment un regard indulgent sur nous ; tu sauras du moins que parmi tes anciens élèves il en est un qui chérira ton souvenir et te sera reconnaissant jusqu'à sa dernière heure, qui, en te disant un éternel adieu, ne s'est éloigné de ton linceul que la cœur gonflé de larmes et de sincères regrets !"

M. Malgaigne also took the rostrum, in behalf of the Faculty of Medicine, and gave quite an eloquent cital of some of the most striking incidents in M. Roux's career. We cannot, however, reproduce here more than the following fluent and just tribute :

"Ce sera là, Messieurs, pour la postérité attentive, l'un des beaux côtés de la vie de M. Roux : c'est ainsi qu'il a agrandi la domaine de la science et élevé l'édifice de sa propre gloire. Mais ses travaux, son enseignement, sa pratique étaient marqués d'un autre caractère que je dois rappeler ici à l'immortel honneur de sa mémoire : c'était un esprit de droiture, de sincérité, d'honnêteté qu'il avait poussé à ce point de s'en faire, si j'osais parler ainsi, une vertu spéciale. L'antiquité a loué Hippocrate pour s'être accusé d'une erreur ; A. Paré et J.-L. Petit se sont honorés par des confessions du même genre ; mais personne peut-être n'a porté aussi loin que M. Roux la loyauté dans l'aveu de ses revers. Convaincu à bon droit que le récit d'une faute apporte un enseignement salutaire, jamais il ne chercha à dérober les siennes ; il les étalait en quelque sorte avec le même empressement que d'autres eussent mis à les cacher. Il regardait la chirurgie comme un sacerdoce, et le mensonge ou même seulement la dissimulation prenait à ses yeux les proportions d'un sacrilège. Noble et fière politique, et que avait sa récompense ;

car nulle parole n'inspirait plus de confiance que la sienne, et en toutes choses son témoignage faisait foi.

"Si maintenant nous passons du savant à l'homme privé, combien toutes ces belles qualités de l'esprit étaient rehaussées encore par les qualités du cœur ! Bon et compatissant pour ses malades, affable avec ses élèves, affectueux pour ses confrères, prompt à reconnaître les jeunes talents et à leur prêter son appui, son mérite incontesté put lui créer des rivaux, ses succès exciter l'envie ; son caractère ne lui avait fait que des amis. Quelquefois il aimait à rappeler les obstacles qu'il avait surmontés, les chagrins qui prennent une part inévitable des plus belles vies ; ce qu'il regrettait par-dessus toutes choses, c'était de n'avoir pas toujours rencontré des affections égales à ses affections. Dès à présent et dans l'avenir, Messieurs, le nom de M. Roux restera comme celui d'une des plus belles et des plus pures illustrations de la chirurgie française, et c'est ainsi déjà que le monde entier l'avait connu ; mais pour nous qui l'avons approché de plus près, qui l'avons aimé et respecté, nous lui devons, au bord de sa tombe, ce témoignage dont il eût été plus flatté que de tout autre, qu'il fut en même temps un homme de cœur et un homme de bien."

M. Roux was born at Auxerre, department of the Yonne, France ; studying the elements of medicine under the direction of his father, a surgeon of some reputation of the town. He must be distinguished, however, from Gaspard Roux, who wrote the well-known treatise on measles.

In 1801 he obtained the first prize at the *Ecole Pratique* and was nominated, in 1806, second surgeon of the Hospital Beaujon. In 1810 he obtained the place of surgeon-in-chief attached to La Charité and taught external clinique in concert with the Professor Boyer. In 1820 he was nominated titular member of the section of Surgery of the Royal Academy of Medicine.

Among the works written by M. Roux we may cite the following :

Essai sur les Secretions. Paris, en 8.

• *Mélanges de Chirurgie et de Physiologie.* Paris, 1809. 3v. en 8.

Résections des portions d'os Malades soit dans les Articulations, soit hors des Articulations. Paris, 1812, en 4.

Nouveaux Elements de Médecine Operatoire. Paris, 1813, 2 vols.

Memoire et observations sur la réunion immédiate de la plaie après l'amputation des membres dans leur continuité. Paris, 1814, en 8.

Observations sur un strabisme divergent de l'œil droit, guéri sur un suget adulte qui en était affecté depuis son enfance. Paris, 1814.

Relation d'un voyage fait a Londres en 1814, ou parallèle de la Chirurgie Anglaise avec la Chirurgie Française, précédé de considérations sur les Hopitaux de Londres. Paris, 1815.

Memoire sur la staphyloraphie, ou suture du voile du palais. Paris, 1825, en 8.

Previous to his death, which occurred from apoplexy, in the latter part of last March, he had been engaged on a work somewhat of the character of memoirs of his experience. It is to be regretted that he was not spared to complete it, but as it had been, however, somewhat advanced, we may yet be enabled to see a portion of it. No reminiscences may be made more rich and interesting.

IATROS.

ART. VII.—*Case of total Inversion of the Uterus, in which extirpation of the entire organ was successfully practised.* By E. GEDDINGS, M. D., Professor of Surgery, &c. Medical College of the State of South Carolina.

On the 16th of May, 1854, I was requested, by Dr. A. P. Pelzer, to meet him in consultation, in the case of a negro woman belonging to Mr. Robt. White, in King-street. On my arrival, Dr. Pelzer called my attention to a large pyriform tumor, equal in magnitude to a foetal head at the full term, which, proceeding from within the vagina, hung pendulant between the thighs. This tumor was large and rounded below, but contracted into a rather thick pedicle above, which could be traced about three fourths of an inch within the vulva, at which point its contour was surrounded by a kind of *cul de sac*, beyond which the finger could not be passed. Its whole surface was covered by a rough, thickened mucous membrane, abraded and ulcerated on many points, considerably inflamed, and disposed to bleed when roughly handled. In the general aspect, it bore a strong resemblance to a case of prolapsus of the uterus, of long standing, but the uniform roundness of the most dependant part, together with the absence of the os tincæ, served at once to convince us that it was of a totally different nature.

The first supposition that presented itself to my mind was, that it might be a case of prolapsus of the bladder, of such long duration, that the walls of the organ had become very much thickened, and otherwise altered in texture. But on introducing the catheter, and passing my index finger around the neck of the tumor within the vulva, I was

enabled readily to discover that it was a case of complete inversion, with extensive hypertrophy of the uterus, of ancient date. The orifice of the urethra was but little removed from its normal position, and in passing my finger up, on the posterior and lateral aspects of the neck of the tumor, as far as the reflected walls of the vagina would allow it to reach, I could distinctly discover the elastic feel imparted by the convolutions of the small intestines, which rested on the partially inverted walls of the vagina.

How long the inversion had existed could not be satisfactorily ascertained, but as there is reason to suspect that the accident must have occurred at the period of her last delivery, an approximative conclusion may be drawn from the fact, that her youngest child, a daughter, was present, and had the appearance of a person of from eighteen to twenty years of age. The report of the woman herself was, that she had been greatly annoyed by the tumor for many years, but had generally been enabled, by partially forcing it up into the vagina, and sustaining it there by means of a T bandage, to pursue her ordinary avocations. Latterly it had increased so much in size as to render this impracticable, and at the period of our visit, any attempt at replacement, however partial, was productive of excruciating pain. She was, besides, suffering so much from engorgement and inflammation of the inverted organ, that, considering this, together with the partial and uncertain benefit likely to accrue from any merely palliative treatment, it became a serious question how we could most readily and efficiently relieve our patient.

Reflecting on all the circumstances of the case, it occurred to me that excision of the entire inverted organ presented a rational prospect of relieving, not only the present sufferings, but also the cause, of much future annoyance. The vagina being also partially inverted, the danger of such an operation was materially diminished, inasmuch as we would in consequence of that condition, be enabled to excise the entire mass by cutting through the vaginal walls, thus leaving the substance of the uterus untouched.

Dr. Pelzer concurring with me, I seized the neck of the tumor as high up as possible, between the thumb and index finger, and manipulating in such manner as to satisfy myself that it contained none of the convolutions of the intestines, I proceeded to include it in a strong ligature, for the two-fold purpose of preventing the protrusion of the int-

The operation was exceedingly simple and easy; was attended with no great pain; and, as may be supposed, was executed in a few seconds.

The after treatment presented no features of particular interest, and the case progressed so favorably, that after a few days, I was enabled to discontinue my visits, leaving the patient in the hands of Dr. Pelzer, who, in a short time, transferred her to Prof. Frost, the family physician, who, at the period of our attendance, was absent from the city. She speedily recovered, and, as I understand, has since done well.

On making a section of the tumor, it was found to present a solid homogeneous mass, of a greyish white texture, and fibrous appearance. The whole cavity formed by the inversion of the walls had become obliterated by adhesions between the opposing peritoneal surfaces; but the point of junction between the vagina and the contour of the cervix could be distinctly recognized, the incision, as stated above, having passed through the walls of the vagina.

Partial and complete extirpation of the uterus, for various objects—inversion, prolapsus, carcinomatus and other degenerations of its structure, has been so often practised, that the simple operation and the description of which I have detailed, possess no claims to interest in point of novelty; yet it has some value as affording an additional instance to prove that, under similar circumstances, the unfortunate victims of a displacement so deplorable, may often be relieved of much suffering and inconvenience. It might be interesting to collect full references to the numerous cases in which extirpation has been practised on account of inversion, but as I have not time to execute the task, I must content myself with this brief and imperfect exposition of a single case.

ART. VIII.—*An exposition of a New Function of the Liver considered with regard to its production of saccharine matter in man and animals.* By M. CLAUDE BERNARD. Translated from the French, by WILLIAM H. FORD, M.D., of Charleston, S. C.

located in the liver. The unexpected results to which I have arrived with respect to this curious production, will be seen, I hope, to be based upon incontestable proofs. Before, however, entering upon the description of facts peculiar to the question, I have thought it useful to explain by what series of ideas I have been directed in my physiological researches. Such an explanation will show how I have, step by step, advanced in my experiments, and will, moreover, prove that the discovery which constitutes the object of this memoir, is due to experimental physiology, that is to say, that it would never have been possible to attain it without direct investigation upon living animals.

How, indeed, could we ever have been led to know by anatomy alone, that saccharine matter is incessantly formed in the liver and poured into the blood to minister to the phenomena of nutrition? No deduction from the formation or structure of that organ could have suggested a proper direction to our studies, and the minutest microscopic examination of the hepatic cells and vessels, would most certainly never have brought us to the knowledge of a similar function.

The rapid progress of organic chemistry, and the fertile physiological impulse imparted to that science by modern chemists, and in particular by the labours of M. M. Dumas in France, Liébig in Germany, &c., had placed, in bold relief divers questions relating to the nutrition of animals. The radiant torch of chemistry, nevertheless, would have illuminated but the surface of the vital phenomena, had not physiology stepped in to carry experiment to the very midst of our organs, and among those internal functions, of which so large a number still remain shrouded in mystery. By this puissant instrumentality, I have been able to demonstrate that there exists in us as in all other living animals, an unceasing fabrication of sugar, whose profoundly hidden phenomena betray themselves externally by no perceptible manifestation whatsoever. A comparative observation, on the contrary, of the nutritive acts in plants and animals, apparently induce us to imagine that the animal organism is incapable of producing saccharine matter; in fact, the sugar and starch formed in considerable quantities in the vegetable kingdom, are incessantly consumed by animals for their own nourishment: whence result two phenomena apparently correlative, and continually accom-

origin, viz : starch. It was therefore reasonable to believe that *saccharine or feculent matters ought to be the exclusive origin of the saccharine principles encountered in animal fluids.*

There were, however, in that singular disease known as *diabetes mellitus*, certain circumstances totally inexplicable. This curious affection is characterized, as is well known, by the inordinate presence of sugar in the organism, to such a point that the blood is overcharged, all the tissues impregnated with it, and that the urine contains it in enormous proportions. Again, habitually in these cases, and particularly when the disease is intense, the quantity of sugar expelled by the patient is far above that which can possibly be furnished him from his aliment, and the presence of saccharine matter in the blood, and its expulsion by the urine, are never completely suspended, from the moment we effect the absolute suppression of feculent or saccharine ingesta.

It was after an attentive examination of these peculiarities as presented by diabetics, and which moreover were well known among physicians, that I was led to suspect that there might exist in the animal organism, phenomena unknown either to chemists or physiologists, capable of effecting the production of sugar through other instrumentality than feculent substances. And from the year 1843 these facts became to me motives for physiological investigation.* As will easily be understood, it was impossible for me to experiment directly upon the production of sugar in the animal organism. There existed no indication of that function in the normal condition; it was observable only as a pathological phenomenon of diabetes, and vanished from my view in those animals upon which I was able to institute experiments.

This, however, could not be said of the *destruction* of the sugar contained in alimentary matters; this was, upon the contrary, a physiological fact evident in itself and perfectly accessible to experiment. It was sufficient, in order to investigate the mechanism of this phenomenon and to determine its cause, to introduce into the circulation of a healthy animal a certain quantity of saccharine matter and thence to

* My first researches upon the assimilation or destruction of sugar in the living organism date from 1843, and it is in my inaugural thesis, (Paris, 7th December, 1843,) that my earliest experiments have been disclosed. I had succeeded in proving a fact, from then unknown, viz: that cane sugar or sugar of the first class,

pursue it in the blood up to the point where it was destroyed or disappeared, that is to say, transformed into other substances. By determining in this manner, the tissue or organ in which this disappearance took place, we ought to have been enabled at length to localise and limit to a precise point *its assimilative agent or organ* in living animals. Afterwards, this agent or organ being known, I was desirous of studying it comparatively in carnivorous and herbivorous animals, and finally to suppress and disable it, if possible, in order to realize and produce artificially diabetes mellitus, and to succeed thus in establishing the existence of a formative agent of sugar of some kind, in the animal organism, &c. Such was, according to my acquaintance with the subject at that time, the indirect and irregular series of experiments I had invented to triumph over the problem which I wished to solve, viz: to determine whether or not *there takes place any production of sugar in animals without the intervention of saccharine or feculent alimentary substances*. But, when we are engaged in the investigation of phenomena as complex as those which occur within living beings, a host of elements must always escape us in our experimental combinations, for the essential reason that they are unknown to us. These elements presenting themselves unexpectedly during the course of our experiments, may doubtless easily disconcert and lead astray an experimentalist of restricted and preconceived opinions. To a docile and attentive observer, however, they are in reality new ideas, which he grasps at once, and after being associated with others, induce him to modify profoundly the direction which he had at first given to his experimentation. This is exactly what happened to myself in this particular case. I was soon obliged to abandon my first point of observation, because the question of an *originating organ of sugar*, which I had considered as the most difficult to attain, physiologically, had, so to speak, unveiled itself to me at once, and at the very commencement of my researches, as will be perceived from the following experiment:

I submitted an adult and healthy dog to a regimen of which sugar formed a large proportion. He was fed daily with two menses of milk-broth, to which was added bread and common sugar. It is evident that nourished in this manner, the animal absorbed by his abdominal venous system, saccharine matter furnished from three sources. 1st, From the sugar contained in the milk. 2d, From the sugar resulting from the digestion of the bread. 3d, From the cane sugar added to the soup.

My object in that experiment was to follow in a measure step by step through the circulatory passages, the saccharine constituents of the aliment, after it had been once absorbed, and then conveyed into the blood

passed through the liver, into the lungs and finally into every tissue of the system.

It remained then to be determined whether the sugar was destroyed during its passage through the liver, the first organ which it must traverse after its absorption by the ramifications of the vena portae. With this object, the dog which had for seven days been subjected to the saccharine alimentation above mentioned, was killed by section of the rachidian bulb during the digestive period. I penetrated as soon as possible both the thorax and abdomen, in order to search for sugar in the blood of the hepatic veins, i. e. after having traversed the liver. Upon examination, it was easy for me to determine most decisively, that there was present in the blood of the hepatic veins, at their inoculation with the inferior vena cava, a large quantity of sugar (*glucose*). There was nothing, however, in this experiment very surprising at first sight. The presence of sugar was apparently explicable by the mode of alimentation, and the existence of this principle in blood which had passed through the liver seemed only to indicate that that organ was not the assimilative or destructive agent for this substance, and that it must be further on, either in the lungs or in some other organ of the body, that the assimilation of this body was effected.

It was, nevertheless, still necessary to verify the exactitude of these results by a *counter proof*. To establish peremptorily that the sugar found in this animal was, in point of fact, that derived from its peculiar nutriment, and which had traversed the liver without being destroyed, it was indispensable to prove the *non existence* of it in the blood of the hepatic veins of another dog, nourished exclusively with meat, or other substances not containing saccharine matter. Such a method of experimentation is the very best safeguard we can have against error, in a study so difficult, as the investigation of the complex phenomena of living beings, and in this instance it was undoubtedly the actual source of the new facts which form the subject of this paper.

To establish my *counter proof*, a second adult and healthy dog was fed for seven days, exclusively upon meat (boiled sheep's head), of which he ate at perfect liberty, and at the expiration of this period he was sacrificed like the first, by section of the rachidian bulb during the digestive period. I collected immediately a portion of the blood of the

upon meat, and had for a week been deprived of all substances capable of furnishing saccharine material, and though the first had during the same lapse of time partaken of aliments strongly impregnated with sugar, and of a nature to furnish it of themselves, there still existed in both large quantities of the same substance.

Such a result was quite too unexpected and too important to be unscrupulously accepted. The only way to remove my doubts was to repeat my experiments; and this I did upon two other dogs subjected to the same differential regimen. From this second comparative trial, I obtained precisely the same results; that is, I determined that there existed equally saccharine matter in the form of glucose, in the hepatic venous blood newly taken from both dogs, as well in that nourished solely upon flesh as in the one limited to the saccharine diet of the first experiment. Finally, to complete and isolate the demonstration of this astonishing fact, I verified by autopsy of both animals the exact and irreproachable character of the conditions of the experiment. I found in the stomach and intestines of the animal fed upon the sugared soup, considerable proportions of saccharine matter; I also established the total absence of any trace of that substance in the matters contained in the stomach and intestines of the animal submitted to the meat regimen; and yet, I repeat it, the blood of the hepatic veins of both these dogs was equally strongly impregnated with sugar.

It will, of course, be easily understood that I now instantly abandoned my original plan of experiment, for the exclusive pursuit of this fact containing in itself the sole and entire solution of my problem. Thus, the determination of the presence of sugar in animals which had eaten nothing but meat, had become an indication of a functional production of saccharine matter in the organism in its physiological state, and such was, it will be remembered, the final object of my experiments. The question of the destruction of this sugar became likewise entirely secondary, and the point of most immediate importance was, to know whence proceeded the sugar encountered in the blood of the hepatic veins of the *carnivorous* dog; that is the animal fed exclusively upon meat.

My attention was naturally directed towards the abdominal organs, in searching for the source of this sugar, because that substance arrived in the inferior vena cava by the hepatic veins, a *continuation* of the abdominal venous system, or system of the vena portae.

The point to be determined was, what organ of the abdomen furnished this sugar?

It was not the intestinal canal, because I had proved that the aliments

(meat) contained within it were devoid of it, and I moreover assured myself that the blood of the* mesenteric veins, as they left the intestine, were likewise devoid of this principle.

Neither was it the spleen, for the blood of the splenic vein contained no sugar; nor was it the pancreas, nor the mesenteric ganglia; the blood which had traversed these organs was in each case deprived of saccharine matter.

Lastly, after many experiments, many illusions which I was obliged to rectify by gropings, as it were, in the dark, I arrived at this demonstration: that, in dogs nourished with meat, the blood of the abdominal vena portae contains *no sugar* before its entrance into the liver, while at its emergence from that organ, and at its arrival at the vena cava inferior, by the hepatic veins, the same liquid contains considerable quantities of the saccharine principle.

The conclusion was inevitable; I was forced to acknowledge that it was in traversing the *hepatic tissue* that the blood acquired its saccharine character, and thus to admit the existence, in the liver, of a peculiar function, in virtue of which sugar was produced.

Examination of the hepatic tissue gave, moreover, the crowning proof of my deductions. Having boiled with a little water a portion of the liver of a dog, fed for fourteen days upon animal diet, I obtained a decoction, in which it was easy to discover the presence of the saccharine principle (glucose). No other tissue or organ of the body treated in the same manner, by boiling in water, gave a saccharine decoction.

It was, therefore, satisfactorily demonstrated, in accordance with all these experiments sufficiently repeated, 1st, *that sugar may exist in the organism independently of feculent alimentation*, and 2dly, *that the liver is an organ capable of producing it*.

Since 1848, when I published my first results, I have considerably extended my observations and researches upon† that new function of the liver consisting in a real generation or secretion of sugar in the animal organism, at the expense of the blood which traverses the hepatic tissue. I have demonstrated that that function, which no one, I presume, had recognised or even suspected before myself, is universal and exists in all animals. I have shewn, moreover, that, if it be necessary to consider the formation of sugar in the liver as essentially a chemical

* See further on the experimental process.

† Claude Bernard. De l'origine du sucre dans l'économie animale (Archives générales de médecine, Octobre, 1848, et Mémoires de la Société de biologie, 1849, t. I, p. 221).

phenomenon, resulting from the metamorphosis of certain elements of the blood in the liver to produce this glucose, it was not the less substantiated, that like the other secretions derived directly from the blood, it was a chemical phenomenon special to *living animals*.

It cannot, in fact, be accomplished without the participation of the nervous influence, and in this respect there exists perhaps no function whatever so directly controlled by nervous action, as the formation of sugar in the liver. I have shewn that, by operating upon certain parts of the nervous system, we may, at pleasure, either suppress the secretion of sugar in the liver or increase it to such an extent as to render animals artificially diabetic.

I have had the honour of demonstrating all these experiments before a commission of the *Academie des Sciences*,* which has accorded to my essay the prize of experimental philosophy for the year 1850. I can also bring to witness the same facts a large number of the learned of all countries. Finally, I may add that at this time—my experiments have been reproduced and confirmed by many experimentalists, among whom I may mention Vanden Broeck,† Frerichs,‡ Lehmann,§ Baumert,|| Gibb,** A. Mitchell,†† etc., etc.

After having established that there exists in animals an internal production of sugar peculiar to themselves and independent of their alimentation, the question how is this sugar disposed of, presents itself naturally. Neither the saccharine matter poured into the blood by the liver, nor that of alimentary origin, are destined in the normal condition to be accumulated in the body, or expelled from the system. This sugar, on the contrary, is to be incessantly assimilated and changed into other products.

All these various questions have been the object of my studies for

* Cette comission etait composée de MM. Flourens, Rayer, Duméril, Pelouze, Serres, et Magendie.

† Vanden Broeck. *Ou der zokingen over der vorming suiker in het organisme der dieren.* *Nederlandsch lancet*, 1849; t. VI. p. 108-110.

‡ Article. *Verdaung* in Rud: *Wagner's Handwörterbuch*, p. 831, n. 2.

§ Lehmann. *Berichte über die Verhandlungen der kön sächs. Gesellsch. der Wissenschaft zu Leipzig.* 30 Nov. 1851.

|| Baumert. *Ueber das Vorkommen des Zuckers im thierischen Organismus* (38 *Jahresbricht der schles. Gesellsch. Vaterland cultur.* Breslau: 1851. 4. s. 22, 25.

** Gibb. *Experiments on the livers of birds in relation to the presence of sugar.* (*Stethoscope*, *Virginia Medical Gazette*, October, 1852.)

†† Arthur Mitchell. *On the occurrence of sugar in the animal economy.* (Glasgow, *Fevrier*, 1850.

years, and I think that the moment has arrived to unite in a more collected form the results of all the physiological researches that I have made upon this subject. Each particular fact, which I have published individually as soon as I had discovered it, will find here its appropriate place, and each experiment, described in detail, will draw its correctives, and its answers to certain incautious critics, from the recital itself of the circumstances from which it has been established.

The functional production of sugar in man and animals, as I understand it at this time, should be examined under three different heads, inducing three different orders of demonstration, viz :—

1st. The experimental demonstration and physiological history of the production of sugar in man and animals, considered in itself, and as a special and normal function of the liver.

2dly. The demonstration of the mechanism by which the sugar produced is destroyed and disappears, and its uses in the animal organism.

3dly. Lastly, the demonstration of the direct influence of the nervous power upon this production of saccharine matter in the organism. This question is, most undoubtedly, one of the most interesting in the domain of the physiology of the nutritive functions, since it teaches how certain phenomena, entirely chemical in their nature, may, however, be completely subjected to the influence of innervation. These three points ought to be treated of separately and in distinct monographs, not only on account of the extent of the subjects which they embrace, but also from the special ideas and considerations embraced in their exposition.

In this memoir I will engage in the discussion of the first question alone, viz : the function by means of which man and animals produce incessantly certain proportions of sugar in the hepatic tissue. All my demonstrations will be specially physiological. Nevertheless, in order to enable persons desirous of so doing to repeat my experiments, I will add in terminating these preliminary observations, a few words concerning the nature of this hepatic sugar, and of the means of research and the dosage which I have employed.*

Like diabetic sugar, to which it is very analogous, hepatic sugar belongs to sugars of the second class ; it deviates polarized light to the right, is not modified by acids, whilst its solution becomes coloured by the caustic alkalis, and reduces the tartrate of copper dissolved in potassa. I will mention summarily the methods for determining these various characters of the sugar of the liver.

1st. *The saccharine decoction of the liver ferments when in contact with yeast, giving rise to alcohol and carbonic acid.*—Alcoholic fermentation is the best of all tests to determine the presence of sugar, and I have always considered it indispensable in my experiments. For this purpose a portion of the fresh liver of an animal killed in perfect health, is reduced to a pulpy consistence in a mortar, or still better, cut up into very small particles with a sharp knife, and to this is added about twice its weight of water. Ebullition is then produced and maintained for a few moments, care being taken to stir the mixture, in order to avoid its adherence to the sides of the capsule. This cooking process effects the contraction of the vessels, and coagulation of the albumoid elements of the liver. The liquid is then passed through a bit of muslin or a fine sieve, and a decoction is thus obtained often opaline or yellowish white in appearance, containing in solution the saccharine matter of the liver, also a little caseine united to fatty matter, various other proteine materials, several chlorides, some phosphates and sulphates in small quantity ; in a word, the various substances commonly found in the hepatic tissue.* It is directly with this decoction, concentrated if deemed necessary,† that I mix the yeast, in order to effectuate the transformation of the hepatic sugar into alcohol and carbonic acid gas. During the process of fermentation the carbonic acid is collected by means of an appropriate apparatus for examination. When the temperature is unequal or not sufficiently elevated, there is almost always formed a small quantity of nitrogen, indicating that along with the alcoholic, a little lactic or butyric fermentation has taken place. In these cases the liquid acquires a decided acid reaction. When the fermentation is terminated, which condition is recognized by the cessation of the gaseous

* See F. E. von Bibra. *Chemische Fragmente über die Leber und die Galle.* Braunschweig. 1849.

† Instead of concentrating the saccharine solution by evaporation, I follow the process which consists in introducing into the first decoction a second quantity of water, and then evaporating to dryness.

disengagement, and the refusal of the solution to decompose the tartrate of copper dissolved in potassa, it is necessary to collect the alcohol produced. For this purpose the solution is submitted to distillation, and the first third of the liquid which passes over is reserved. This product is then mixed with quick lime or caustic potassa, in order to obtain by a second distillation the alcohol in anhydrous condition, or at least sufficiently so to allow of its certain recognition. The alcohol thus obtained by fermentation of an hepatic solution, is a limpid, colourless fluid, of a spirituous taste often masked by a disagreeable odour not belonging to itself, and which appears to possess special character in different species of animals. This alcohol, if it be sufficiently concentrated, takes fire and burns with a yellowish flame. When the alcoholic solution is too weak, in order to determine its presence more easily, the liquid should be heated over a lamp in a tube, closed at one end and of moderate length. During the ebullition, a lighted match is held at the open extremity of the tube, and as soon as the apparatus becomes sufficiently heated, the first portions of alcohol are seen to burn with a bluish-yellow flame, which descends and is extinguished in the tube, several times in succession, so long as there remains any alcohol to be volatilized. Mixed with a solution of chromate of potassa, to which a small portion of hydrochloric acid is subsequently added, upon the application of heat the liquid resulting from the first distillation exhibits phenomena of reduction, and assumes a green colour; this is occasioned by the formation of green chloride of chromium, and at the same time the peculiar odour of aldehyde becomes perceptible.

In experimenting upon very small animals, whose livers are also diminutive, it is important, if alcohol cannot be obtained, to collect at least all the carbonic acid evolved during the fermentation. I have in these cases made use of an apparatus of a simple kind, consisting of a glass tube (T) of capacity proportionate to the quantity of liquid to be examined, and closed at one end (i. e. an ordinary test tube.) Into this is introduced the hepatic decoction and the yeast; it is then closed by a cork (C) through which passes a slender tube (E) open at both ends. In corking the tube (T) it is necessary to expel with care all atmospheric air. This little apparatus is maintained at a temperature of about 72° fahrenheit, and fermentation being established, the carbonic acid produced is imprisoned at the superior part of the tube (T), and by its pressure forces out the liquid through the tube (E). As the quantity

better under mercury, and a small fragment of caustic potassa allowed to come in contact with the gas, the whole contents of the tube being retained by the extremity of the finger, and slightly agitated. The carbonic acid being absorbed by the potassa, the atmospheric pressure is felt upon the finger, and if this be removed, the water or mercury mounts within the tube and completely fills it, thus proving that the gaseous substance experimented upon was in reality carbonic acid.

By conducting the fermentatory process in a similar manner, the presence of sugar in the blood, or other animal fluids, may be readily demonstrated. In the serum of the blood of the hepatic veins especially, it may be recognized by direct admixture of yeast, the fermentation generally manifesting itself with great rapidity.

I will here mention that I have always employed fresh and well washed yeast, and in all my experiments I have instituted corrective processes, with the yeast alone, in order to be well assured that the carbonic acid had in fact no other origin than the hepatic sugar. Finally, the liver is the only organ of the body capable of furnishing alcohol by the agency of fermentation with yeast; so that this characteristic becomes perfectly distinctive.

When the saccharine hepatic decoction is left to itself, no globules of ferment are developed within it, as takes place in diabetic urine and some other animal liquids, and alcoholic fermentation does not occur spontaneously. The sugar usually disappears rapidly in these hepatic liquids, but always under the influence of the lactic or butyric fermentation, giving to the fluid an acid reaction easily affecting litmus paper. When the decoction of the liver on the other hand is not saccharine, it becomes decomposed, exhibiting an alkaline ammoniacal reaction.

(To be continued.)

REVIEWS.

ARTICLE IX.—*Types of Mankind*—Dedicated to the memory of Samuel George Morton, M.D., (late President of the Academy of Natural Science, at Philadelphia,) and illustrated by contributions from Prof. Agassiz, LL.D., W. Usher, M.D., and Prof. H. S. Patterson, M.D. By J. C. Nott, M.D., Mobile, Ala., and Geo. R. Gliddon, formerly U. S. Consul at Cairo.

[The discussion of the questions under consideration, having been originally conducted in the Journal, by Drs. Morton and Bachman, and much if not all the material from Dr. Morton's pen incorporated in Drs. Nott and Gliddon's book having been written and intended for our pages, the lamented death of the author alone preventing its insertion, we feel constrained again to offer its sheets for its continuation, by either of the parties concerned.—*Eds. Char. Med. Jour. and Rev.*]

We have been induced to take some notice of the above work, not so much from its intrinsic merits, as from the parade with which it has been ushered before the public, and the names of distinguished scientific men who have contributed papers to swell its importance. We were surprised at the very slender contributions they have afforded Dr. Nott, in a scientific point of view, and not a little gratified to find that they have not aided Mr. Gliddon in carrying on his grand design of discrediting the Christian religion, and heaping on the Holy Scriptures all manner of epithets of derision and contempt. We have only room at present for one or two extracts from Mr. Gliddon. He sums up his opinions on the Book of Genesis, in these words: "viewed as a narrative inspired by the Most High, its conceits would be pitiful and its revelations false, because telescopic astronomy has ruined its celestial structure, physics have negatived its cosmic organism, and geology has stultified the fabulous terrestrial mechanism upon which its assumptions are based. How, then, are its crude and juvenile hypotheses about human creation to be received?" (p. 565.)

Being determined not to be diverted from our first position, in examining this subject solely on the principles of science, we will leave Mr. Gliddon in his 271 pages, (90 of which are consumed by enlightening the benighted clergy on the true reading of the 10th chapter of Genesis,) to be dealt with by the Theologians. We counsel them however, not to be unmindful of the threat that hangs over their heads if they dare venture on the perilous experiment of defending their good old Bible which has been fairly demolished by Gliddon. He informs them that he had kept "300 pages" in reserve, and that he is the prudent soldier who "keeps his powder dry." (p. 627.) If, however, these "Telogastri" as he terms them, (p. 716,) are fearless enough to undertake to question, "the damning evidence producible that these worthies (the translators of the scriptures) could not construe a simple line of the Hebrew text," (p. 614,) and venture, on the peril of receiving the appellations heaped on the American clergy, such as "biblical dunces," "snobs of universal humanity," &c., to remove the odium he has cast on their Bibles, we advise them to consult the books he has so frequently quoted and which are as fa-

miliar to the theological student of German rationalism, as household words. Strauss, De Wette Gesenius, of Germany, and Burke, of England, &c., will afford them the evidences of the origin of this ponderous mass of oriental lore, which he informs us "to extract their works is merely mechanical," and therefore requiring no extraordinary amount of intellect or judgment. They may perhaps find some difficulty in fully understanding some of his transcendental terms, such as "tachygraph of a bull's head," "monumental evidence coetaneous in registration," "hamitic affiliation," and many other hard words not found in their dictionaries. It would require a good deal of scrutiny to understand his meaning when he says the "truth will revile to our view through the archaeological deductions with the force of an euclidian demonstration." (p. 580.) Besides all this, they may refer to many of their commentaries in common use, and they will find these and other difficult passages in scripture satisfactorily explained, without encountering this pedantic flood of Hebrew lore, or stoop to the necessity of abandoning the doctrine of the inspiration of the scriptures, or unite with him in hurling contemptuous epithets against the learned and good men, who gave us our English translation.

We have seen this book of "Types," which reviles the common version of the scriptures which we read in our family devotions, in the possession of some of its subscribers, and heard several of them complain that through the cunning devices of Nott and Gliddon who led them to suppose that they were to receive a scientific work on the doctrine of the diversity of the human races, supported by the matured views of Morton, Agassiz and others, they had been completely entrapped and deceived.

Now it appears that the contributions from Agassiz amounted to 17 pages, the substance of which he admits he had published nine years before, and which may be found in much fuller detail in one of his school books (*Principles of Zoology*, pp. 154-181,) nor did he add that in both these works, he insisted that "man alone was a cosmopolite. His domain is the whole earth. For him and with a view to him it was created." Twenty-seven pages are inserted from Morton's inedited manuscripts for which, if they were subscribers to his former works, they have already paid. The article on geology and palæontology, in connection with human origins—of the fossil man dug out of the delta of the Mississippi, "57,000 years old," and of Agassiz' man found in Florida at least "10,000 years old," as well as Morton's prophecy, (p. 326,) that "man will yet be found in the fossil state, and that he walked the earth with the *Megalonix* and *Paleotherium*," might as well have been omitted for their own credit sake as well as that of Morton. A premium however, has now been offered for the discovery of ancient human bones; they will no doubt soon be forthcoming. Fortunately we have a sure method of detection. When a cunningly stuffed specimen of a pretended new bird, in which the types of several genera were presented, was sent to the Zoological Society, of London, the members were for a time perplexed. Swainson, however, applied his scalpel to the specimen, and the hoax was detected in the patch work of the artificially constructed bird. Now, our friend Agassiz knows full well that every species in these lower strata or eocene beds differs from those that at present inhabit the earth. He even goes much farther than we are disposed to follow him, viz: that even the very shells in the lower formations differ from those of the present. He has, we

believe, described a fish from a scale, restored another from the vertebrae and several species of animals from a tooth, and arranged them under their proper genera and species. Let him now describe these new species of man, *Homo Floridians*, *Homo-Mississippians*, and each specimen will tell its own story. In the case of the cranium dug out of the excavation at New Orleans for the Gas Works, Dr. Usher candidly admits "the type of the cranium was that of the Aboriginal American Indian," (p. 338.) Agassiz' specimen of "jaws with perfect teeth, and portions of a foot," was, as he informs us, dug out from "a bluff on the shores of Lake Munroe, in Florida, with a mass of shells mostly *ampularias* of the same species now found in the St. John's river," etc. These portions of a skeleton were examined in Charleston, and as we are informed. Count Portalis spoke of it as that of a modern Indian. The Indians in Florida must have always had burial grounds.

If Dr. Usher had quoted Hamilton Smith fairly and fully in his account of the fossilized human body found at Gibraltar, in 1784, there would be no need of any further remarks. (Read Smith's *Nat. Hist. of the Human Species*, p. 953, and also as an explanation, Bachman's *Unity*, etc. p. 298.)

But it may be inquired, why these desperate and thus far futile attempts at making man older than the present animals of our earth? This can in no wise affect the species. The characteristics by which the species are designated are stamped on them by the hand of the Creator. It is true, the Bible tells us that man was the last creature on our earth, formed by the hand of his Maker, and if it is now attempted to prove that he was not the last, but "walked the earth with the *Megalonyx* and *Paleotherium*," (p. 326,) then the argument, if it is of any value, would hold good, against the veracity of the scriptures, but would in no wise have any bearing on the species or the unity of the human race. In that case, however, these arguments instead of being smuggled into a work whose object is to prove the diversity of the human species, on scientific principles, ought to be presented in a separate work, and might be headed "*Types of Infidelity*."

There is yet another fact which these gentlemen seem to have overlooked. Hitherto they have insisted that the short period of 6,000 years, to which various authors have limited the creation of the existing species of animals and man, would not admit of the production of those endless varieties which are found in the human race. They have therefore anxiously watched the labours of Lepsius and others, among the monuments of Egypt, to trace up the negro as well as the Caucasian to a remote antiquity. The investigations of Lepsius, Bunsen and others, have met with the approbation of many christian ministers, especially those of the Protestant Churches in Germany, England and America. Many learned writers have been at work on Chronology, from Julius Africanus, Eusebius down to Hales, and recently to Bunsen and Lepsius. It has not disturbed the faith of enlightened Christians. So far as the Hebrew chronology is clearly laid down in the Scriptures, we receive it as truth. Beyond this we gladly avail ourselves of all the aids that scientific investigations can afford. But Messrs. Nott and Gliddon seem to have overlooked the fact, that as far as the doctrine of the unity of the human species is concerned, which in a scientific point

of view, separates itself both from theology and chronology, (these being fully able to take care of themselves,) the longer the period is extended, the more time will have been allowed for the production of varieties among men. If, therefore, they can prove (what they never will accomplish) that the present creation of animals and men are as old as the modern Indian, dug up on the banks of the turbulent ever varying soil of the delta of the Mississippi, to whom they have assigned the astounding age of 57,000 years, there will have been ample time in a domesticated, omnivorous, predatory, cosmopolite animal like man, to have produced not only the endless varieties that now exist, but to have brought forth not only white, yellow, red and black, but gray, blue and spotted men.

It could scarcely fail however to have staggered these indomitable champions for the plurality of the races, who leaned so much on the discoveries of the Chevalier Lepsius, to enable them to prove the negro to be of a different origin from the white man, to be now distinctly informed by him in answer to a letter of Dr. Nott, that he was a believer in the unity of races, (pp. 232-33.)

They have only anticipated us as we have much more to say on that subject. Not only Lepsius, but his co-laborer, Bunsen and the most scientific men of England, Germany and of every other country, are of the same opinion, not on the grounds of humanity, as has been asserted, not from any reference to the teachings of scripture, but from the most minute personal investigations, favored by all the advantages which could be afforded, and conducted solely on the principles of science. Humboldt had travelled all over the world and examined all the races of men, Lepsius and Bunsen had copied ancient inscriptions, given us sketches of men and dogs, and other animals, figured on the monuments of Egypt,—all of them sought for truth and followed the teachings of nature wherever it might conduct them; Now they come before the world proclaiming their matured conviction of the truth of the doctrine of the unity of the human species. But this is not all, they have opened to us the hitherto obscure records written on these monuments, carried the histories of the Jews and the Egyptians to an early period, and every fact they have brought to light, only serves to strengthen us in the faith that the scripture thus submitted to the test of history, of science and philosophy, contains the inspired will of God to man. Let these investigations and discussions go on, not in the spirit and temper of Nott and Gliddon, whose compilations are intended to establish a theory, but with the philosophical knowledge, the calm and deliberate judgments and the courtesies of a Humboldt, Champolion, Lepsius, Bunsen and Horner. The enlightened Christian fears them not. When the erring and imperfect judgments of scientific men shall coincide in the true interpretation of the works of God in nature, they will be found in accordance with his inspired word.

Messrs. Nott and Gliddon will gain nothing by their taunt against the clergy. They say, "Our clerical adversaries need not lean therefore upon savans whose sole object is scientific truth." This had reference to the remarks of Lepsius. "*Je laisse de cote le point de vue theologique qui n'a rien a faire avec la science.*" The charge against "clerical adversaries," surely cannot apply to us, who have used all our efforts

to rein in such Biblical expounders as the authors of "the types," to the simple scientific question. From this, however they have so constantly bolted, that they have now scarcely anything to discuss but chronology and Biblical Criticism.

Of one fact, however, Mr. Gliddon seems fully sensible, viz: that the christian public will not surrender their English Bibles, even if they were indulged with a new translation from Gliddon and Nott, with all the learned commentaries offered us by Strauss and his pious fraternity. He states "our English version, is revered by the masses as a 'fetich,' (p. 592.) In the next page he says, "As positivists, we lament not that our brief span of life will have been measured long before a new English version may be authorized, because the developments of science will have rendered any new translation altogether supererogatory among the educated, who are creating new religions for themselves."—*Sic*.

Since then we are unquestionably determined to put up with our good old family Bible, which is hallowed to us by the tenderest associations, what must be the feelings and what the piety of a father who can place these "types of mankind" into the hands of his wife, his daughters and especially his sons?

We will not, however indulge farther in a train of thought which it is difficult to suppress.

We now proceed to offer some remarks on those portions of the memoir of Morton, by H. S. Patterson, M.D., which more particularly allude to the investigations and discussions in Natural History, in which we were engaged with Dr. Morton, and on which we differed very widely.

A few chapters of our work on the Unity of the Human Race—those portions referring to hybridity, had been previously inserted in the Charleston Medical Journal, a work well adapted to such a discussion. Two papers had been previously published in Silliman's Journal, in 1847, (pages 49 and 403,) by Dr. Morton, the subject of which was to prove "that different species of animals are capable of producing together a prolific hybrid offspring, therefore hybridity ceases to be a test of specific affiliation," consequently the mere fact that the several races of mankind produce with each other a more or less fertile progeny, constitutes in itself no proof of the unity of the human species." After this publication, Dr. Morton, with whom we were in occasional correspondence on scientific subjects, wrote to us requesting any further information we might possess, on the above subject, stating that he meant to publish other articles in continuation. We immediately wrote him the result of our investigations in reference to hybridity, which we presumed he would publish in his next number, and allow the public to form an impartial judgment. He wrote to us in a friendly manner, on other subjects, but made no farther allusion to hybridity. Aware, from circumstances not necessary to notice in this place, that he was but imperfectly acquainted with the species of Mammals and birds, and that his reputation would finally suffer from the errors he had unintentionally committed in the designation of species, and his too implicit reliance on authors of doubtful authority, we again urged him for the sake of the truths of science and his own reputation, to

review the very faulty articles which he had published under the sanction of his name. But in his reply, he studiously avoided even an allusion to the subject. Supposing that intelligent naturalists would be able to detect the errors that were but too prominent in the article, and that it would be overlooked and forgotten by others, we concluded to remain silent, unless a sense of imperious duty required us to pursue a different course. Three years passed away and Dr. Morton's articles remained uncontradicted. Finally he caused these articles to be reprinted in a pamphlet form, and sent them to various societies and individuals. At length Dr. Nott, of Mobile, in his "two lectures on the Biblical and Physical History of Man," quoted these papers on hybridity, (p. 17) as evidences in favor of the plurality of the human species. When we at length concluded to publish our views on the subject, we wrote a friendly letter to Dr. Morton, (Oct. 15th, 1849,) stating our intention to review his articles on hybridity, and at the same time requesting some information and the loan of one or two books which he had quoted, to which we had no access in Charleston. Strange to say, his answer was deferred over four months, without affording us an opportunity of consulting the works which we were compelled to review; and we were obliged to guess at their contents and publish the book without this examination. On the 14th February, 1850, he replied, informing us that he had written the letter four months before, but neglected to copy it. In regard to the book we particularly desired—he wrote; "The only copy of Rudolphi I have seen, is in my own library, and I had to send to Germany for it." Having been previously informed in Europe of some singular statements published by Rudolphi, which were not credited by naturalists, we wished to subject the book to a fair criticism. This may probably be the only copy in America, as we have endeavored without success, to obtain it from Germany. The booksellers sent us another of his works, but informed us that this book was out of print.

Notwithstanding our disappointment, in not finding Dr. M. quite as liberal as we had anticipated, we were determined that on our part this discussion, whilst it must be free and independent, should be of the most amicable character. In the beginning of our review of his articles, we stated, "We feel confident that our highly esteemed correspondent and personal friend, to whom the world owes a large debt of gratitude, for his researches and writings on Ethnography Medicine and the Natural Sciences, will not regard it as a violation of those ties which bind us to each other, if the same facts which he has collected and arranged, are reviewed, and respectfully but freely and fearlessly commented on by another. (*Med. Jour. March 1850, p. 170.*)

In his answer in the following number of the Journal, May 1850, p. 328, he observes:

"Permit me to commence by saying that I fully appreciate the kind

sionally have been betrayed into a weakness that savoured of irritability, is not to be wondered at, when it is recollected that he had culled from all manner of authors, so many strange, and in many instances, ludicrous examples of supposed hybridity, which were now proved to rest on no foundation that would bear the test of scrutiny.

To render the discussion less offensive to him, we distinctly stated that "our remarks were applied to his authorities" from which we would cheerfully have separated him, if unhappily he had not endorsed to a certain extent, the errors published by others. Another fact should not be overlooked, that in every case, as far as we can now recollect, our retorts were called forth by some previous hit of his own. This rather amused than offended us. We had stated in one place, that we had once travelled ten miles out of our road, to examine what was called a hybrid, between a bull and a sheep, which proved to be a large ram with a hairy fleece and rather straight horns. [Med. Jour. p. 178.] He asked us significantly, in his reply, (p. 329,) whether "this animal might not have been a cross breed between a sheep and a goat?" This seemed to be somewhat of a taunt against one to whom he had frequently applied to describe the animals in the collections of the Society of which he was the presiding officer. In our answer we told him, "If this was a mere playful remark, I do not particularly object to it, as the subject we are investigating, must appear rather uninteresting to the majority of our readers, and I am willing that in this dull journeying with us, they should be allowed to smile even at my own expense. But if you really are in sober earnest, and wish to convince me and the public that I do not possess sufficient knowledge to discriminate between a true sheep and the hybrids of a cow or a goat, I know no better means of producing conviction on your mind, than by inviting you to a personal examination. Now, my good friend, if you will come and partake of my hospitality, to which you are fully entitled, and which it would afford me great pleasure to extend to you, we will have an opportunity of examining some specimens in my cabinet—in cages and in the poultry yard, and we will go together on a tour of observation to this said 'hairy ram,'" etc. . . . "I will venture to say, that by the time this delightful excursion shall have been made, and these comparisons instituted, we will both be able to distinguish the difference between a true sheep and its hybrids from the cow on the goat."

We were, however, not a little perplexed to find that in Dr. Morton's very first paper in reply to ours, on hybridity, he branched off from the legitimate object under consideration, and began to discuss the scriptural question—to speak of Genesis—the sons of Adam seeking wives—the animals contained in the ark—the deluge, and then "refer to the weakness of mankind to delight in mystery, and to be perpetually looking for miracles," and threw out the old taunt about "Gallileo" and the "humiliation of the pride of man at his own ignorance," etc. This certainly, was a wide departure from the subject of hybridity which we were discussing, and more especially inapplicable to a clergyman who belonged to a tolerant church, and who professed to lay off for a time, the churchman's robe, and discuss this whole subject on the principles of science

alone. Then he spoke of the absurdity of regarding all the animals as being derived from Mesopotamia, and said "It is difficult for me to imagine that a practical zoologist can entertain a contrary opinion." All this while he professed to have our writings before him, in which we used the arguments that the scriptures, without any violation of the truths of inspiration, could be interpreted according to the views of St. Augustine, of Bishop Stillingfleet, Mathew Poole, Rev. Dr. Buckland, Dr. Pyc Smith, President Hitchcock and many others of the most orthodox and pious divines, as well as Hugh Miller and other scientific men. These contended that according to the phraseology of the Old Testament, universal terms were often used to signify only a very large amount in number or quantity—that the deluge, sent as a punishment, was universal as to mankind, but covered only those regions that were then known to men, which did not extend beyond the range of Syria, Armenia, Mesopotamia, Arabia and Egypt, the then inhabited parts of the earth, to whom the phrase "the whole earth" was applied, and that the ark contained the family of Noah—the animals that had been brought into domestication and those in the countries covered by the deluge. The Rev. Thomas Smythe, of this city, supported the same views in his work "on the Unity of the Races." Yet Morton as well as Agassiz and Nott, regardless of the views of Augustine, Stillingfleet, Poole, Smith and other enlightened Christian Divines, have exhausted all their arguments to disprove the contrary doctrine, without admitting that those whose views they profess to oppose, do not differ from them in this particular. They first conjure up a man of straw, and then concentrate all their energies to batter him to pieces. When this was brought to the notice of Morton, he made no reply.

This mode of procedure is always not only unscientific but unjust.

Believing that Dr. Morton would by his own industry and through the aid of his friends, at the extensive Library of the Academy of Natural Sciences, and elsewhere, collect all the cases of hybridity that were on record, we concluded not to interrupt his labors until they had been concluded. He published in nearly every number. We remained silent for eight months. We were fully prepared to account for every case of hybridity he had quoted, and to prove from his own facts that not a solitary race of animals had been perpetuated by the intermixture of any two species; that in nearly every case they were as sterile as the mule or the mongrel duck—that in a very few exceptional cases they could only be propagated by a resort to the full blood of the original species, that hybrids could not breed with hybrids, and therefore the supposed new race was at an end. One paper in answer to Morton had already been printed, when in July, 1851, his death was announced. Although a second paper was in the hands of the Editor of this Journal, we immediately inserted the following notice.

"Messrs. Editors—The recent death of the late Dr. Morton, which I deeply regret, and which men of science will deplore, induces me from a

quoted; and as these had not generally been endorsed by Dr. Morton, his name may be entirely left out of the discussion."

Until this day, this discussion has not been renewed on our part.

We will now return to the labours of Nott and Gliddon, through the instrumentality of Professor Patterson.

Nott says (Pref. p. 14.) "It is with no small gratification, I now feel assured, that through Dr. Patterson's effective memoir, Morton's cherished fame will evermore preserve its rightful place among men of science." Dr. Patterson, we are informed by the *New-York Medical Journal*, died on the 27th of April, and the *Types of Mankind* were published in the same month. Whether he ever saw his memoir in print, we are not informed. We are informed, however, in that Journal, that "while confined to his bed in his late illness, he wrote that excellent biography of the late Dr. Samuel George Morton, which is prefixed to the *Types of Mankind*." With the dead we war not.

Those who published the memoir and sent it with their highest commendations over the world, with the brand of infamy attached to our name, are living—they, and they only will be held responsible. We will only notice the memoir as far as it concerns us, giving the words which Nott and Gliddon have published, and our own publications which called forth their denunciations.

On page 46, we have these words: "The men who, in the middle of the 19th century, can still find the ancestors of the Mongolians and Americans, among the sons of Japheth, and who talk about the curse of Canaan in connection with negroes, (NOTE.—The doctrine of the unity of the human race, examined on the principles of science by John Bachman, D. D.) are plainly without the pale of controversy, as they are beyond the reach of criticism. There is even in some who have recently published books on the subject, such a helpless profundity of ignorance, of the very first facts of the case, that one finds no fitting answer to them but—expressive silence," etc.

The views we had published, are in the following words: "If we follow the teachings of history, we discover in Shem the parent of the Caucasian race—the progenitor of the Israelites and of our Saviour. In Japheth, that of the wide spread Mongolian, many of whom to this day, are dwelling in tents—as the various tribes in the East and on our Western continent fully testify—and Canaan, the son of Ham, although *we cannot regard his descendants as accursed*, is still every where the "servant of servants."

We will now turn to Morton, the only staff on which they lean in Ethnology. The following is his language, quoted by him with approbation from Murray's *Enc. of Geog.*

"Cuvier and other learned physiologists, are of opinion that the primary varieties of the human form, are more properly but three; the Caucasian, Mongolian and Ethiopian. This number corresponds with that of Noah's sons; assigning therefore the Mongolian race to Japheth, and the Ethiopian to Ham, the Caucasian, the noblest race, will belong to Shem, the third son of Noah." (Morton's *Crania Americana*, p. 2.) In turning to his colored map in front, of his introductory essay, he ranges the Mongolian on the Eastern continent, from the gulf of Siam,

near the tropical line, to the arctic ocean, and on our Western continent, from the 80th latitude, including the Eskimaus and the whole Polar family. In his *crania americana*, p. 70, he has figured four crania under the title of "*Mongul Americans*." These views in regard to the existence of the Mongul family, in the north of our western continent, were supported by him in the last manuscript.

The question now arises, why did Nott and Gliddon publish this sweep in charge against us, and not place the name of their oracle, Dr. Morton at the head of the list, and their own in juxtaposition among those who have exhibited this "helpless profundity of ignorance?" We moreover distinctly stated *we cannot regard his descendants (of Ham,) as accursed*. Why was our language misstated? why are we, by an unworthy phraseology, made to utter language directly the reverse of that which we employed? Truth and candor are virtues which all should prize, who wish to hand down to posterity the reputation of fair and honorable men. Denunciations on false charges are like torpedoes exploding in unskillful hands, and causing more injury to the assailant than to the assailed.

On page 47, Nott and Gliddon farther, publish under the name of Patterson, "nothing but an exaggerated assault could have drawn from him a reply. That assault was made."

If the fact of our pointing out the errors in science in respectful language, must be construed into an "assault," there will be an end of all discussions.

In pages 48 and 49, we have quotations from Dr. Morton's address before the Boston Society of Natural History, April 27th, 1842, in which they say he had clearly announced his belief in the diversity of the races. But why did they not come out with the plain statement of facts brought to the notice of Morton, in the very review which they profess to have read, and for which they had so mercilessly assailed us? He had on this occasion been addressing a Boston audience.

Let us hear him again, in the same year, six months afterwards, before his medical class in Philadelphia, Nov. 1st, 1842.

"The inspired historian has depicted the moral and physical beauty of our first parents, in the gardens of Paradise among the valleys of Mesopotamia. But temptation and sorrow soon blighted his perennial happiness; the earth became filled with iniquity, and by a universal calamity, the race of man, excepting a single family, was swept from the face of creation. Is it to be supposed that this family, which had been preserved from the common fate of humanity by a series of divine interpositions, would be sent forth on the wide world to struggle with the vicissitudes of chance? Is it not more probable that the same infinite power that conducted them unharmed through the deluge itself, would adapt them before their dispersion, to those varied physical circumstances with which they were henceforth to contend? The strongest moral contrasts are coeval with the descent of mankind from the ark. (See Gen. chap. 9th.; 25, 26, 27, and chap 49: 1-27.) and we may reasonably infer, that equally strong physical diversities were then established, constituting what are called in modern language, the races of men." (Introduc. Lect. Pens. Col. Nov. 1, 1842.)

They next quote from Silliman's Journal, 1847, in which they state, he had declared his belief in the primeval diversities of men, and then add—

"One would suppose that whoever had read the above publications could have no doubt as to Morton's sentiments, yet Dr. Bachman and others have affected to be suddenly surprised by the utterance of opinions which had been distinctly implied and even openly published years before."

These gentlemen ought not to have contented themselves with publishing a garbled extract, but given the whole stereotyped paragraph which he had published in his *Crania Americana*, in which his essay appeared, headed, "Varieties of the Human Family." This paragraph was repeated in all his subsequent works verbatim et literatim, and which reappeared here. The following passages they omitted:

"Further researches into Ethnographic affinities may render it probable that what are now termed five races of men, would be more appropriately called groups; that each of these groups is again divisible into a greater or smaller number of primary races, each of which has expanded from an original nucleus or centre. . . . Nor does this view conflict with the general principle, that all these nations and tribes have had, as I have elsewhere expressed it, a common origin," etc.

Messrs. Nott and Gliddon, had the whole subject before them. (*Med. Jour.* May 1841, p. 391) Thus after these facts has been fully proved, from his own writings, we added, "I do not blame Dr. Morton or any other man for changing his opinions, but why should he censure one who has referred to them in the language of kindness and regret? There is moreover a want of fairness in blaming those who refuse to accompany him in all the fluctuations to which his mind has been subject. If with the Bible in his hand, he, after the study of so many years, could trace all the races of man to one common origin, he had paused here, and enquired whether these diversities could not have originated without a miracle, he probably would not have made the assertion, which has since given pain to his friends." "I believe in the plural origin of the human race," etc.

We would just observe here, that Drs. Nott and Gliddon, have unravelled the whole mystery by publishing a private letter from Dr. Morton to Nott, in which he says "When I took this ground four years ago, (and in the *Crania Americana* my position is the same, though more cautiously worded) it was with some misgivings, not because I doubted the truth of my opinions, but because I feared they would lead to some controversy with the clergy." (*Types of Mankind*, p. 50.) Here then they have published his admission that he had entertained these opinions for eleven years, had veiled his sentiments "cautiously," so cautiously that they could be read on either side—using all his efforts secretly to undermine the doctrine of the unity of the human race—whilst ever and anon, he would publish a pamphlet tracing up all the races of men to the ark, to throw dust into the eyes of the clergy, and preserve the pupils of his medical school.

We have said enough on this subject to convince every reasonable mind, that these pretended guardians of the well earned fame of Morton,

would have acted more wisely not to have made this ungenerous charge upon one who would willingly have suffered this matter to remain buried in oblivion, and who only refers to it here in order to repel an unwarrantable attack.

On page 56, we have these words, "The next spring, (1850.) witnessed the publication of Dr. B's book on Unity, as well as his Monograph on Hybridity, in the Charleston Medical Journal, in which Morton is made the object of assault and attempted ridicule."

The monograph happened to be a part of the book. The assault was in the disturbed imagination of the trio, Messrs. Nott, Gliddon and Patterson, and the ridicule, if it could be so construed, was directed against the absurd theories of the authors on whom Morton too implicitly relied. Were we, to support a theory, to publish to the world the notions of Hamilton Smith and others, that "by producing cross breeds with the Quagga and two or three Dauws and asses, an improved austral horse may be attainable"—that it requires six species of wolf, anthus, dingo, jackal, d'hole and hyena, (animals that never could be domesticated,) to manufacture a dog, and that the best ornithologists have succeeded in tracing the common fowl to at least ten different species of wild cocks and pheasants, (one only having ever been brought into domestication,) we will be prepared to have our notions ridiculed. We deny, however, that there was the slightest attempt at ridiculing Morton. Two of his warm friends at Philadelphia, wrote to us at the time, expressing their gratification at our having been able to expose the absurdity of the theory in the language of kindness, to the individual who supported it, without the possibility of giving offence. It can be plainly seen what was the cause of Morton's chagrin, and the unallayed bitterness of his defenders. The long cherished doctrine of fertile hybrid races, to prop up their doctrine of the diversity in the species of men, was about to be demolished as it most assuredly will be—"hinc illae lachrymae." Why was it not convenient for them to answer our facts and arguments, instead of resorting to the vulgar weapons of personal abuse?

In the same page they belabor us for not having read Pritchard, before we ventured to publish. As we believed that this subject could only be satisfactorily examined on the principles of natural science, views which are also expressed by Agassiz, we selected our own mode of investigation and pursued our own train of thoughts. We possessed sufficient independence of mind, not to lean for support either upon Strauss for our theology, Morton for Ethnology, or Agassiz for Natural History. Compilations are in general evidences of poverty of mind.

They proceed in the same page, "No reader of ordinary intelligence, can open the book without perceiving the fact (ignorance of topic,) for himself."

The decision on the merits of a book, depends altogether on the capacity of its judges. We have a bundle of letters in our possession from eminent naturalists, both of Europe and America, Humboldt included, but as they have spoken of the latter rather disparagingly, (p. 52,) and as we have been rather surfeited with the mutual laudations contained in the "types of mankind," we shall withhold all these stimulants to the public appetite, and leave the unbiased reader to judge for himself.

"His tone was arrogant and offensive. Not only to the general reader in his book, but also to Morton in his letters, does he speak *de haut en bas*, as if from the height of the pulpit, he was looking down upon men immensely removed from him by his sacred office." "This faulty manner perhaps, results from his profession."

We have never, that we know of, met either with Dr. Nott or Dr. Patterson,—we presume, therefore, that they are ignorant of our manner in the pulpit. Moreover such has been the irreverence with which Nott and Gliddon have spoken of the version of the scriptures read in our churches, (See Nott's *Two Lectures*, and *Types of Mankind*. Gliddon, p. 466, down to his *Palæographic excursus*, 655,) that one would suppose they would avoid both churches and ministers where such "pitiful conceits," such "false revelations" and "juvenile hypotheses," are palmed on the audience as the word of God.

To abuse a man's profession when we are unable to meet him in argument, is the general resort of the weak and the ill-natured. The enemies of religion would much sooner vent their insults on a clergyman whose principles enjoin forbearance, than on a layman. This trio seems to have no love either for the clergy or the missionary. We have had some specimens of Patterson's courtesy. Gliddon politely terms them "Biblical dunces," "snubs of universal humanity," etc., and Nott, speaking of the diminution of the inhabitants of the Sandwich Islands, says, "they are daily sinking beneath civilization, missionaries and rum," (p. 410.) We recollect hearing an anecdote of Randolph, who was so much opposed to the tariff on wollens, as to cause him to declare he would go a quarter of a mile out of his way to kick a sheep. The politicians however, were in fault, and not the innocent sheep. Nor is the poor missionary to blame in this matter. He leaves home and country, and perils his life in a foreign land, and among savages, to rescue them from the temptations of rum and the debasing lusts of depraved white men. Surely the remnants of these tribes, now under the influence of christianity, are purer, and better and happier than their monogamous, heathenish, cannibal, forefathers?

They proceed—"He reminds Morton of the countenance he had heretofore given him—that he had subscribed for his book."

We will now see the mortal offence we committed in this case,—here is every word written on this subject.

"You will bear in mind that I did not commence this controversy; and you are aware how reluctantly I felt myself compelled, from the claims of duty, in behalf not only of the truths of science, but on a subject of infinitely higher moment, to review your articles. We will not, therefore, misunderstand each other. As long as you were adding materials to our previous knowledge, of the different varieties or races of men, I approved and sanctioned your labours. You will recollect I contributed the few materials that were available to me, to add to the number of figures of Indian skulls, (these had been obtained during the Florida war,) I became a subscriber to your very valuable work, on the *Crania Americana*. Regarding it in the most favorable light, I spoke of it accordingly. Little did I conceive that the collection of human skulls—even had they amounted to many hundreds more—would enable any

one to establish the doctrine of a plurality in species, without an investigation of the operations of nature in the forms and skulls of domesticated animals, that are in this respect similarly constituted with man."

We leave our readers to make their own comments on the above.

"The authorities relied upon by the latter, he treats with supreme contempt, individually and collectively, characterising them as pedantic, antiquated and musty."

We treated no author with supreme contempt. We neither used or thought of the word *pedantic*,—that word was introduced by the authors for their own purposes. Morton had complained, but in courteous language, that we did not value his authorities—we replied by stating that some of the authors he referred to as having been treated with disrespect, such as Temminck, had not even been mentioned by us, (See *Med. Jour.* July, 1850, p. 468,) and that he had spoken less favourably of Buffon than we had done, etc. But how can we decide on the statements of ancient authors in Natural History, without referring to the greater accuracy in these departments of the modern and more reliable authors?

In a note appears the following paragraph.

"Dr. Bachman's contempt for everything old, is certainly very curious in one so likely from calling and position, to be particularly conservative. Nor is this his only singularity. His pertinacious ascription of a remote date, to every one whose name has a latinised termination, reminds one of the old story of the backwoods lawyer, who persisted in numbering old "cantharides" among the sages of antiquity. He is particularly hard upon "old Hellenius," never failing to give him a passing flout, and talk about raising his ghost. The writings of Dr. B. do not indicate a very sensitive person, yet even he must have felt a considerable degree of the sensation known as *cutis anserina*, when he received the information, conveyed in Morton's quietest manner, that old Hellenius, with others of his so-called musty authorities, were his own contemporaries. The work of Chevreul, which he disposes of in the same supercilious way, bears the extreme date of 1846!" In a reference and note, (p. 724,) Mr. Gliddon says, "it was my good fortune to have marked for Dr. Morton, that passage in Chevreul's skillful paper, which Dr. Bachman so queerly ascribed to old and musty authorities."

These last notes have been inserted to show the fortunate aid with which Dr. Patterson was blessed in preparing his "effective memoir."

If every word of the above had been true, it does appear to have been a matter of small moment, too unimportant to have been inserted in the memoir of a deceased man, which whilst it did not add one feather to his own fame, was at the same time calculated to heap ridicule on another.

But what will be thought of the forbearance and christian charity of these expounders of the Bible, when the public is informed that "old Hellenius" collected his incredible stores in 1789 and published them in

and that the work of Chevreul was the very one about which we had desired information from Dr. Morton, in order that we might account for the very suspicious statement, about goats being converted into sheep, and vice versa; a statement which Hamilton Smith himself has since denied, and believed that there was no hybridity in the whole affair: and Dr. Morton himself says, "efforts have been made on a pretty large scale in Pennsylvania, to obtain a cross between the sheep and goat, but not a single hybrid has yet resulted." (*Ch. Med. Jour.* May, 1851, p. 379.) Dr. Morton, who it now seems, was helped to the reference by the "good fortune" of Mr. Gliddon, refused to give one line of information about the book. If, however, he erred in regard to our "contemporaries," what must we think of those who have made this parade and published the error of their friend, after having before them (*Med. Jour.* May, 1851,) the unmistakable evidences that Dr. Morton "in his quietest manner" had made a great mistake. And moreover, as they say they were "actuated by the sole desire to render justice to a beloved friend," why do they from the motive of gratifying their own prejudices against another, compel him in self-defence to disclose the evidences of a want of fairness and candor in that friend?

They proceed—"But there is an Italian proverb, which says "*non sempre chi cantando viene can tando via*," which Dr. B. was destined to illustrate. To his first paper, Morton replied in a letter dated March 8th, 1840, the tone of which is calm, dignified and friendly. He defends his authorities, accumulates new evidence and strengthens and defines his position."

In that paper, after having given a few pages on hybridity, he abruptly flew off from the subject, to the scripture doctrine, and gave a dissertation on the sons of Adam obtaining wives—"Ethnology"—"the Mosaic record"—"the animals in the ark"—"the deluge"—"the weakness of mankind in looking for miracles"—"Gallileo," etc.

Here was work cut out for a year's discussion. Immediately Dr. Nott at the close of one of his courteous and gentlemanly reviews, exclaims, "I have received and read Dr. Morton's reply to Dr. Bachman, on the question of hybridity as a test of species. It is the most perfect refutation I have ever seen, and it is to be hoped that no one will ever waste time again in advocating the idea, that prolificacy among races affords an evidence of common origin." (*DeBow's Rev.*, Feb. 1851. J.C.N.)

This was rather superlative language. But the public did not agree with him. All European Naturalists with whom we corresponded, thought it a very unscientific paper. Nor did Dr. Morton himself feel satisfied. He held on to some of his errors in one or two papers but being reminded of the injury he was inflicting on science, in perpetuating error, he relinquished some of the most important cases of hybridity, which he had brought forward as unanswerable proofs of fertile

Alba, was not a hybrid, but a true species, described by Gould, as *M. Yarellii*—again he gave in his adhesion. He had denied our statement that naturalists agreed that *Capra Aegagrus* was the origin of all our domesticated goats. He now admitted (*Char. Med. Jour.* May, 1850, p. 330.) "I stand corrected with regard to *Capra Aegagrus*, which is by general consent admitted to be the source of the common goat." These were admissions that ought to have cooled even the ardor of Nott. Thus his facts constantly diminished, until he had only the dog to lean upon, in support of his theory of fertile hybrids. To this, in a future number, we will give our attention.

They continue—"He charges Morton in taking part in a deliberate conspiracy, having its ramifications in four cities, for the overthrow of a doctrine nearly connected with the faith and hope of the Christian, for this world and for eternity."

Here we must be allowed to enter a little into detail. Dr. Nott had just published his two Lectures on the Biblical and Physical History of Man. He advocated the views which Gliddon has maintained in the book misnamed 'Types of Mankind.' We have only space to give a few extracts.

"How could the author of Genesis know any thing about the history of creation, or the races of men, when his knowledge of the physical world was so limited?" (p. 53.) "The book of Genesis is nothing more than an assemblage of very ancient fragments (or traditions) of unknown origin, put together, without order, and consequently of no historical value." (p. 60.)

"The whole history of creation and the human race, down to the epoch of the flood, is compressed into the first five chapters of Genesis, and human ingenuity could hardly originate any thing more confused and contradictory than this account."

"It is impossible that he (Moses) could have written all the ridiculous inconsistencies attributed to him." (p. 77.) He quotes 'Strauss' to prove that the historical account of Christ is equally 'inconsistent.' (p. 66.)

He does not believe that Christianity was intended for the black man.

He says—"It needs not the inspiration of a prophet to foretell that the religion of Christ can never be comprehended and adopted by African races, as long as their physical type remains unchanged." (p. 17.)

But as he insists that it cannot be changed, where then is the use in giving them a religion which they cannot comprehend? We on the other hand insist, that the command was to "go into all the world and preach the gospel to every creature," whatever may be his form or color.

He says, "these philanthropic efforts, so far from producing good fruits to the dark races, in the main, do more harm than good." (p. 18.)

We have in Charleston, we presume, about as many colored communicants as whites, and we believe the Christian religion has decidedly

Caucasian half, the benefit of the Christian religion, but what must be done with the African half of this same man, for whom, according to Dr. N., it was not intended?"

And now let us examine into the truth of the charges we are said to have made against "Morton of his *taking part in a deliberate conspiracy having its ramifications in four cities.*" Here are our words:

"Your publications and your private notes generously presented to individuals who were to become labourers in this controversy, together with your notices to the public of a forth-coming work, on the same subject, now in a state of progress—gave premonitions of an approaching attack on the long received doctrine of the unity of the human race. The recent demonstrations from the four points of our Union—Mobile, Charleston, Philadelphia and Boston—did not, therefore, much surprise me. Hence you will bear in mind that I did not commence this controversy."

Where, then, is the conspiracy with which we are said to have charged him? Nott, of Mobile, had just published his book. He had sent another paper on the same subject to be read before the Association of Naturalists convened at Charleston, where Agassiz brought the subject before the Association. There also Dr. Barratt's address (to whom Dr. M. had sent his notes), on the same subject, had been delivered before the Medical Society. Dr. Morton, of Philadelphia, had publicly announced and published in several journals, that his work was in progress, and Agassiz had just published, at Boston, his two papers in the *Christian Examiner* (*Diversities of the Human Species*), and sent them, in pamphlet form, broad-cast over the land.

We neither thought or spoke of a *conspiracy*. It was a gratuitous invention of their own. Whatever conspiracy there might have been had already long been proclaimed by themselves and in print.

Moreover we have now before us the evidences of their having fully carried into effect the intentions they long since announced to the public, with several additional labourers whose names did not appear in the programme. The "*Types of Mankind*" is before the public. We have a contribution from Agassiz, of Boston. One from Morton and another from Patterson, of Philadelphia. Nott and Gliddon, and Usher, from Mobile. De Bow's Review, from New-Orleans, where Nott, who always speaks favourably of himself, appears to have reviewed his own book; and finally we have the Southern Review, in Charleston, whose columns have long been given up to Nott. We recently applied to the editor for the insertion of a paper—his answer was that the October number was pre-occupied by an article on the other side, but that we might publish our article in the next January number, in the meantime we might have four pages in the October number, agreeable to our request.

How ridiculous, then, does the cry of a conspiracy sound in the ears of sensible men, when uttered by the co-labourers in such a combination?

The truthful Biography of Morton continues:

"In another paragraph (p. 507) he says that infidelity must inevitably spring up as the consequence of Morton's views."

Our language was: "Since the Scriptures unquestionably teach that all men are of one blood, should your doctrine of many and separate creations, and therefore not of one blood, be received, then infidelity

must unquestionably spring up in the very bosom of the Christian Church, since the Scriptures would be said to contradict themselves."

"The language of Scripture, in all its teachings on this subject, is, however, so plain that it cannot be mistaken by those who believe in its inspiration. The doctrine of the unity of the human race is clearly taught in various places, both in the Old Testament and the New. It is proclaimed as the bond of universal brotherhood—it is interwoven with the plan of man's redemption and with the commission given to the heralds of the cross to 'teach all nations. It may, moreover, be added that there is not a single text that can be interpreted to express an opposite doctrine. It is declared 'And Adam called his wife's name Eve, because she is the mother of all living.' Gen. iii.: v. 20. This is strengthened by another positive declaration: 'And the sons of Noah that went out of the ark were Shem, Ham and Japheth; and Ham is the father of Canaan; these are the three sons of Noah, and of them was the whole earth overspread.' Gen. ix.: v. 18, 10. In the new dispensation we are expressly told: 'God hath made of one blood all nations of men for to dwell on the face of the earth, and hath determined the bounds of their habitations.' Acts, ix.: v. 18, 19. These words are so plain that there is no possibility of misinterpreting them without casting off a belief in the veracity of the Scriptures and regarding all that relates to this whole subject as the inventions of ignorant and uninspired men."—*Ch. Med. Rev.*, July, p. 507-8.

But those who have read our paper will recollect that we distinctly disclaimed applying to Dr. Morton any charge of infidelity. He had stated "I never fully adopted and announced this conviction until I felt fully satisfied that it was in harmony with the sacred text and reconcilable with the sublime teachings of Genesis."—*Med. Jour.* May, 1850, p. 320. To this we replied in the July number, p. 504. "And here I cannot withhold my unqualified approbation of the reverence you have expressed for the Scriptures, and your anxiety to adhere to their inspired teachings. Your course differs so widely from that of your predecessors, who had advocated your theory—who treated Moses and the prophets as if they were ignorant and arrogant imposters—that I hail it as a new era in a discussion in which Christian men may now engage, without having their feelings shocked by the irreverent and offensive language of their opponents."

This was the language we used in every communication we addressed to Morton. We characterised our respect for his reverence of the Scripture as the word of God, as derived from his own impressive language. What must now be our astonishment, in reading an extract from his own letter of the date of January 29th, 1850, which, to suit their own purposes and convince others that he coincided with them in their imma-

human race had its origin in incest I should think that I had at once got the clue of all ungodliness. Two lines of catechism would explain more than all the Theological discussions since the Christian era. I have put it into rhyme :

'Q. Whence came that curse we call primeval sin?

A. From Adam's children breeding in and in.'—(*"Types,"* p. 409.)

Did it ever occur to this trio of pretended friends that if Morton ever wrote such a letter in a fit of levity and thoughtlessness, he never intended it for publication. Did it occur to them that his mourning family might be shocked on reading this poor doggerel and along with it to learn that his reverence for "the sublime teachings of Genesis" was intended for the public eye, and his private views for Dr. Nott?

That which is permitted by the will of the Creator for carrying on his designs in peopling the earth is not criminal or revolting. When the race was established he then gave laws for its government.

Farther on we read :

"Now we all know that when gentlemen of Dr. B.'s cloth use that word, they mean war, *usque ad necem*. Its object is simply to do mischief and give pain. It cannot injure the person attacked, so far as the scientific world is concerned—for there the phrase can only excite a smile—but it may impair his business, or his public standing, or still worse, it may enter his domestic circle and wound him through his tenderest sympathies. Was such the intention in the present case? Charity bids us think otherwise; and yet the attack has a very malignant appearance."

There is, however, a species of "malignity" which the broad mantle of charity cannot cover. There are actions in the lives of men, the intention of which cannot be misconstrued. This is exhibited when men first awaken public sympathy in behalf of a prominent deceased man and then, either covertly or openly, make false charges against one who met him on the fair field of open argument, who addressed him not as a dead but a living man—his equal and his friend—and when it is known full well that their motives are to weaken the influence of one who is likely to give them some trouble in the establishment of their theories, and whose reputation they find it more easy to assail than to refute his arguments.

"More of a theological polemic than a naturalist, he uses the tone and style proverbially displayed by the former and is offensive accordingly. He has his punishment in general condemnation and general scientific standing."

If by patiently submitting to the vile aspersions of Dr. Nott and his associates for nearly four years, without writing one line in self-defence, his scientific standing has been impaired, may not the period be at hand when public sentiment will place its brand "in general condemnation" on the motives and characters of his indomitable assailants?

In our closing article in Sept. 1850, p. 650, when we had resolved to suspend our labours until Dr. Morton and his friends, by the aid of the large library of the Academy of Natural Sciences, should have collected all the cases of hybridity on record, we expressed our views on

the manner in which these scientific discussions should be conducted. We pointed out the subjects for discussion before any new theory could be established. These were : 1st, a comparison of skulls of different races of men and of domesticated animals. 2. What are the true characteristics which constitute species in wild animals and what are they when applied to domesticated species and man? 3. Hybridity. 4. The physical causes of the changes in form and colour of the domesticated animals and men. 5. The question whether any species has been created in two or more distant localities. We then added :

"These and other subjects which occur to me, all of which should be known before any new theory can by any possibility be established, will afford room for many interesting inquiries and may be pursued without either giving offence to any one or creating alarm in any mind. They might afford very interesting subjects of experiment and investigation for physiologists and zoologists for several years. I have not a shadow of fear or doubt in regard to the final decision of this question. In the mean time, as this is a subject of deep philosophical and scientific inquiry, let us approach it in the spirit of philosophy and the objects of true science. Although I have many other and much higher duties to discharge, I would have no objection to give the facts which, on each of the above subjects, have been accumulating on my hands for the last thirty years. I have no taste for controversy and would gladly avoid it. I have, however, no aversion to an amicable discussion in search of truth. And why should it be otherwise here ?

"The naturalists of America are composed of a very small band. As long as they pursue the legitimate objects of science, they will merit and receive the respect and gratitude of the community. You and another eminently distinguished naturalist and friend, who welcomed me with cordiality, and honored me with his confidence when I was an invalid and a stranger in a foreign land, and whom I love as a brother, are the only two individuals in our country with whom I would be willing to discuss this subject even in a single line. Your happiness and fame are as dear to me as my own honour. Why should we 'fall out by the way.' The admonition of the patriarch will not be inapplicable to us : 'Let there be no strife between us, for we are brethren.'

"I remain, my dear sir,

"Very faithfully,

"Your friend and servant."

When we contrast this language with the unworthy charges contained in this pretended Biography of the life of Morton, we cannot fail to perceive the wide difference. When men make statements from publications which are not accessible to the public eye and refuse to quote the words of another, their motives may always be suspected. We un-

an acceptable professor in the Pennsylvania Medical College four years. In Geology he would have become prominent had he not been diverted from these pursuits by his stronger attachment to the study of Craniology. In the latter department he had no equal in America. The delineations of skulls in his *American and Egyptian Crania* were truthful, and his short descriptions were generally correct; but the mere knowledge of skulls in the ever varying heads of tribes of men will not allow us to decide on species without subjecting them to the same process by which we decide on the varieties of domesticated animals. With the latter he had but a slight acquaintance. We require not only figures of skulls but of countenances. The figures in Hamilton Smith are the best parts of the book. Other works that have appeared since the days of Pritchard should be consulted. Pickering—the recent works of Latham and Martin—not written to establish theories, but to give facts, should not be overlooked. We regard Schoolcraft's voluminous works on the Indian tribes, recently published by Congress, as containing more correct and valuable information on the American Indians than the combined works of all that have ever written on this subject. In natural history, Dr. Morton had a general, but not a precise knowledge in either of the departments. He was the friend of science, and contributed largely, by his punctuality and zeal, to promote the interests of the Academy of Natural Sciences, of which he was, at the time of his death, the presiding officer. Above all, he was a man of integrity and great moral worth. The only suspicions that were ever published against his truthfulness, candour and sincerity are proclaimed to the world in the "*Types of Mankind*, by Nott and Gliddon."

In looking around to ascertain the sources from which these personal attacks originated, we are not left to any doubtful conjecture.

On account of a prominent element in our Southern institutions, Dr. Nott had long been labouring, not to prove that the negro was a variety of man, inferior in intellect to the white man, although physically his equal, which all of the South, who were believers in the unity admitted; but that he was of a different species and not of the same blood. This doctrine was acceptable to the politicians of the South, hence he had gained a welcome admittance into our Southern journals. As soon as our work on "*the unity of the races*" appeared, he showed his hand, and commenced a warfare which, to all appearances, will not be brought to a close during his life time. His first attack, as far as we know, was commenced in *De Bow's Review*, Feb. 1851.

He introduces the subject with an ominous flourish of trumpets, like the rumbling of thunder before a storm. Commencing with "*Pope's*

of mortification and disappointment—mortification from its utter want of Christian charity and courtesy. * * * * Really bad books are usually calculated to do harm; but this, though it may lead astray for a time many who read little and think less, is destined to do immense good. Its ill-temper will destroy all its force," &c.—(p. 116)

Perhaps he may have somewhat underrated the book which he so scornfully condemns and which he conceives will do good to his cause. He had just published his two lectures on the biblical and physical history of man. Having in his first lecture fairly established, in his own mind at least, the doctrine of the plurality of the races, he triumphantly exclaims in the beginning of his second lecture: "The question of the unity of the human races, received simply as a question of science, we think may be fairly regarded as settled in the negative." This book of ours, then, according to Nott, "utterly wanting in Christian charity and courtesy," would do no harm, but aid his cause. How comes it then that, after the whole affair had "been fairly settled," they find it necessary once more to go over the whole ground, publish the "Types of Mankind," of 738 pages, with 703 quotations and references, inventing defamations against the man who has the fearlessness to oppose their deep designs, and calling in the aid of dead and living men—"Help me, Cassius, or I sink"!

We quote the above disparaging remarks on our work merely to show the similarity of sentiment between the biographer and the reviewer. A number of reviews of our work are before us, but none, except those written by Nott or those under his influence, have regarded our language as "wanting in Christian charity and courtesy."

On page 131 he says: "One cannot but be amused at the cool affrontery with which the Dr. kicks his opponents out of Sunday School," &c. We merely refer to the language as not particularly courteous or refined.

The habit of kicking out of Sunday School is not practised by any of the clergy, and the taunt is of the same character with that in reference to the Polynesian dying out under the influence of rum and the *Missionary*.

In the *Southern Review*, p. 424, he says: "We think he [referring to us] had better quit reading entirely, until he can learn to understand plain English, and writing, until he learns common courtesy."—Nov. 1850, p. 425.

Perhaps we erred in not having selected Dr. Nott as a preceptor.

are two species of prairie wolves east of the Rocky mountains, but as we examined some hundreds of skins at the furriers at New-York, besides carefully prepared specimens, and as Mr. J. W. Audubon had seen the living animals in all their varieties, heard their howl, and traced them from the North-Western prairies to Mexico and California, we felt it more safe to describe them as varieties of one species, leaving the subject for the further investigation of naturalists. We, however, ex-honorate Dr. Nott from any intentional design to misrepresent us here, as he seems thoroughly unacquainted with every branch of natural history and we doubt whether he can distinguish a wolf from a hyena.

In turning hastily over his recent work (p. 64), he makes the red fox, one of the best known animals in our country, common to both continents. Any school boy who has read Godman would have told him that the red fox of Europe (*vulpes vulgaris*) and the red fox of America (*vulpes fulvus*) were totally distinct. The moment he leaves the leading strings of Morton and Agassiz he involves himself in inextricable difficulties.

In the Southern Quarterly Review, Nov. 1850, Dr. Nott says: "So extraordinary has been his misunderstanding of the plain language of Agassiz, Morton and Pickering," (in reference to their views on the unity of the races.) He has afforded us a sad view of the *plain language* of Morton, "*cautiously worded* on account of the *clergy*." Nott and Gliddon have just published Agassiz' very last views, in which he says: "I still hesitate to assign to each an independent origin (perhaps rather from the difficulty of divesting myself of the opinion universally received, than from any intrinsic evidence). I must in the presence of these facts, insist at least upon the *probability* of such an independent origin of all nations, or at least, of the independent origin of a primitive stock for each, with which at some future period migrating, or conquering tribes have more or less completely amalgamated."—(p. 72.) Thus we have no more than a *probability*, and our friend Agassiz has made no farther advance on either side. His language, then, was not plain at the time we were censured for not fully understanding him—it is no plainer now. We quoted Pickering to show that he agreed with us in all the important views we had expressed. That man's original birth-place could not have been in a cold climate, that the aboriginal Americans had unquestionably been derived from the Moguls and the Malays, and that he had affixed to his valuable work a map, on which was placed a circle in Ethiopia, from which, in dotted lines, he pointed out the probable paths pursued by man in his migrations to the farthest ends of the earth. To confute us, he brings a passage from Pickering to show that on the Egyptian monuments there was no evidence of a change of type. This, however, does not disprove the facts we have given from Pickering's own work.

In our work (p. 37) we stated that the creation was a miraculous work of God; that he stamped on the first human pair a constitutional power to produce these permanent varieties. We then added: "God *might* have created the first progenitors of our race millions of ages earlier than at a time when they actually appeared, and if he so constructed the human constitution that it would by its own organization

be capable of producing varieties that were to become permanent in their character, the effect would be produced by a natural process—varieties of men would be formed without a miracle.”

In his review (p. 128) he pretends not to understand our “notions of chronology,” and makes us say: “God *may* have created the first progenitors of our race millions of ages earlier,” &c. Thus misapplying our remarks, which were on another subject, viz.: that God chose his own way and selected his own time in the creation of men, and he leads his readers to suppose that we had reference to chronology and admitted that man might have existed millions of ages earlier than is generally supposed. But he cannot convict us of saying this without misquoting us and substituting his own word *may* for that of *might*, which we had used to prove an undeniable fact of a different character.

The authors of the “Types” have published a letter from Morton, in which equal injustice is done to Pritchard by false quotations. They make Morton say (p. 50): “Look at the last page of Dr. Pritchard’s great work—the last page of his fifth and last volume, and there *he gives it as his matured opinion* that the human race has been chiliads of centuries upon the earth.”

Let us now see what Pritchard actually said “in the last page of his fifth and last volume”:

“The Hebrew chronology may be computed with accuracy to the era of the building of the temple, or at least to that of the division of the tribes. In the interval between that date and the arrival of Abraham in Palastine it cannot be ascertained with exactness, but may be computed with a near approximation to truth. Beyond that event we can never know how many centuries nor even how many chiliads of years may have elapsed since the first man of clay received the image of God and the breath of life. Still as the thread of genealogy has been traced, though probably with many and great intervals, the whole duration of time from the beginning must apparently have been within moderate bounds and by no means so wide and vast a space as the great periods of the Indian fabulists.”

In a note he adds: “This general conclusion as to the extent and limits of chronology deducible from the Hebrew Scriptures coincides with the result of the Chevelier Bunsen’s researches, though the views entertained by that great scholar are, so far as they have yet been explained, different from my own.”—*Pritchard’s Researches*, vol. 5, p. 570.

In the recent work of Lepsius (Egypt, Ethiopia and Sinai) the reader may find the views of Bunsen and Lepsius recorded. The date of the

own responsibility ; for, although perhaps unconsciously, he appears to see every thing through coloured glasses.*

The questioning the veracity of another, even by an insinuation, is always resented as a personal insult by men of the world who pretend to the character of gentlemen, unless it be hurled at the head of an unfortunate clergyman, who, whilst he submits to the imputation as a matter of duty, cannot but regard it as a wrong. The utmost stretch of Christian forbearance does not, however, forbid an occasional retort.

There are some sayings and doings in these *Types of Mankind* which we have designated "*Types of Infidelity*," as a set-off against the new title of "*curiosities of natural history*," which Nott has affixed to our work (De Bow's Review, Feb. 1851, p. 118), that are calculated to awaken suspicions. The similarity in the charges made in the reviews and reiterated in the memoir, got up in the same heedless style, without a regard to facts or evincing much delicacy or wit, may they not, by some cause as yet unexplained, claim the same paternity ? Those private letters from, and sayings of, departed worthies, may they not have been seen "*through coloured glasses*" and published for effect ? That memoir, which drags in poor Harlan, who is dead and has left no representatives, and the living clergyman who is a non-combatant, has strangely omitted the literary squabbles which occurred among the members of the academy during the administration of Morton, comes to us in a very questionable shape. The note of reference (p. 404) about the book which it was his "*good fortune*" to find is marked by the initials of Geo. R. Gliddon. The other (p. 55) about "*the backwoods lawyer*," "*cantharides*," and "*cutis anserina*," in the same refined and courteous style, bears evidence of having been mixed and stirred up in the same crucible by these worthies. We would just hint the bare possibility that those portions of the memoir referring to us may have been smuggled into that solemn memorial in which they purposed to erect a "*literary cenotaph to the memory of Morton*."—(p. 626.) In that pretended memorial, extracts from his private letters have been published that throw suspicion on the veracity and impeach the principles of Morton, and it has become the duty of his true friends to use their best endeavours to wrest that memory from the grasp of his unwise and egotistical eulogists.

That Italian proverb "*Non sempre chi cantando viene*," &c., throws its shadow in the direction of "*moslim lands*," (p. 716) and seems to have emanated from the same research and written by the same hand from which proceeded the closing flourish of the "*Types*"—"*La medicina diventa amara*"—addressed to H. Ex Boghas Youssouf Bey—Mohammed Ali's Prime Minister, "*Cairo*," &c.—(p. 716.)

* Mr. De Bow, in a note, states "it is painful to publish these strictures of Dr. Nott," but he could not interfere—as the object of the discussion was "*truth*." If all these personalities and unworthy insinuations were true, and necessary to the establishment of truth, neither apology or explanation were requisite. If other-

There is a scene in the *Types*, described by Dr. Nott (Introduction to the *Types of Mankind*, p. 50), where our great Southern statesman sent for Mr. Gliddon to aid him in inditing "that memorable dispatch to King," and by his "*Ethnology*" "put a final stop to all intermeddling with the domestic institutions of other nations," which might be received with some misgivings by the captious critic. It is true the "Dr. gives it on his own responsibility, for, although perhaps unconsciously, he appears to see every thing through coloured glasses."—(Nott in *De Bow's Review*, p. 118.)

Those extracts from the private letters of Morton, published by Nott and Gliddon "on their own responsibilities," by which they have attempted to grace their "literary cenotaph" with the head of Janus—that catechism turned into rhyme—that prediction of the yet undiscovered fact that "man walked the earth with the *Megalonyx* and *Paleotherium*"—may not all have been seen "through coloured glasses"? Might not the family and true friends of Morton demand as searching a scrutiny into the manuscripts of Morton as Gliddon gave to the tenth chapter of *Genesis*? And if no interpolations in the text were found, might they not next inquire why these extracts of private and familiar letters were published to the world? We confess we have no other but circumstantial proofs. We have no defence to offer against the charges made against us about "the misgivings" of Nott in regard to "the facts given on our own responsibility" and "the coloured glasses," &c. We merely throw out a hint of the danger that might result from questioning "facts" given by others "on their own responsibilities."

We have entered into this long defence to offer reasons to the public why we resolved in the beginning to enter into no discussions with Dr. Nott. We stated in our work on the *Unity* (p. 36): "The American [meaning Dr. Nott] who seems to have laboured rather to deny the historical veracity of the Scriptures than to prove the diversity of the races, may perhaps claim the attention of the Theologian; but having produced no new fact or argument to bear on the subject to which we are restricted in this purely scientific discussion, he requires no further notice." That he denies the historical veracity of the Scriptures he has fully admitted, and that in a scientific point of view he has produced "no new facts or arguments in favour of the plurality of the races," may be easily seen in his writings. He leans on Agassiz in zoology and on Morton in hybridity and ethnology. No sooner had Agassiz started an opinion that men had not originated in pairs, but were created in groups in many places thus requiring the Creator to multiply himself

been informed that we were once more heartily abused, not by our ancient opponent, but by a new co-labourer. The abilities of Dr. Nott as a physician, or a politician, we have not called into question. His private character we have no right to assail and know nothing on the subject. His writings, however, savour too much of the wrangler, and too little of the close research of the naturalist and the calm and dignified temper of the philosopher. He delights to live in the atmosphere of controversy. For ourself we feel no disposition to waste the remnant of life that is left us "in idle disputes," and the possession of a tranquil mind is too valuable to be exposed to these rude shocks. We have endeavoured, amid many imperfections, through a long life to deserve the character of truth and Christian integrity, and this is all that we have to transmit to our children and to the church with which we are connected. Nothing but this fresh assault would have induced us to write one line in self-defence against any thing that might have been published under the signature of J. C. N. He has just declared (p. 61) that he has done with the religious question, and adds: "The broad banner of science is herein nailed to the mast." Long may it wave there—we will not climb up the mast to remove the nails which rivet his flag, unless it be in self-defence.

In conclusion we would here add, that we have waited for several years for the farther developments which might be made on this subject, in order to prepare a second edition of our work on the unity of the races, which has long been called for. Our materials have so accumulated on our hands, and so many new facts have been presented under the different heads, that portions of the book require to be re-written and large additions to be made.

If by the permission of Providence our life is spared, we stand pledged to prove:

1. That all the varieties of men are of one species. This important point, which was so strenuously denied by the believers in the "diversity" until Agassiz' two papers were published in Boston, seems now to have been abandoned by those who appear willing to shift their sails according to the word of command. The cause we advocate has gained much in this admission. But they were compelled by the force of truth to give in their adhesions.

2. That if all the varieties of men are of one species, then, in conformity with the operations of nature in every known species of vertebrated animals, they are all of one blood. The same species of quadruped, bird, reptile, fish, insect, or plant, has never been found to exist in any part of our world on two widely separated localities, unless it was transplanted there by human agency—or by winds or currents. Therefore, if the varieties of men all over the world are not so many distinct species, they will be proved to constitute only one species, created in one locality, migrating from thence to the ends of the earth and producing endless varieties. In a word, that the operations of nature have been the same in the human race that is exhibited in the domesticated animals that have accompanied man in his wide migrations—living in all climates—exhibiting all shades of colour and varied in form and intelligence even more than the varieties of men.

3. The sketch by Agassiz, "on the natural provinces in the animal world and their relation to the different types of men," will next be considered. It is written with much ingenuity and with the pen of a naturalist and is moreover purely scientific. The map also is sufficiently characteristic. To this sketch, with the map annexed, we will devote especial attention, and we are sure that our friend Agassiz will be gratified to correct any error he may have committed in his sketch, if it is clearly pointed out to him, which we will endeavour to do.

We have only space here to give a very brief sketch of our own views on this subject, and must leave farther details to another occasion. There are zoological provinces, on which multitudes of naturalists have laboured during past ages, but they are as yet very imperfectly defined. They apply to a small number of species of quadrupeds, birds and plants in each province. Thus the monkeys and sloths, the parrots and humming birds, and among plants, the palms, etc., are confined to the tropics, and the white bear, the arctic fox, etc., to the arctic region, in the North; but in all these regions, there are other animals of a higher organization adapted to infinitely wider migrations. As we ascend in the scale to carnivorous animals, and those adapted to extensive migrations, we find many who overstep all the zoological boundaries assigned to other species, and who wander far and wide over the earth: at the head of which man is placed. We will just select as examples, Agassiz' arctic realm in the first column of his tableau, which appears to be more characteristic than any of the rest. There he has given as the types of this realm, the Eskimau man, the white bear, walrus, rein deer, harp seal, right whale, eider duck, and rein deer moss. Fortunately, by the aid of Richardson's *Fauna Boreali Americana*, and the naturalists who accompanied the expedition of Parry, Ross and Lyon, we are in possession of very accurate accounts of the animals, birds and plants that exist in the arctic regions. (Geology, Zoology and Botany of Parry's 2d voyage, by Prof. Jamaisson, J. Clarke Ross and Sir Wm. Hooker.) Let us refer to a few of these and then inquire why Professor Agassiz has excluded them from his tableau? The common wolf, (*canis lupus*) and the ermine exist there, and have been found as far to the north as the foot of man has trodden; both species are found around the polar regions, on both continents, both extend far into Asia—to the tropics in Europe, through Canada and the whole United States in America, to Oregon and California; both are found in Texas, and are noticed in Capt. Sitgreaves' expedition, as existing in New Mexico, and the wolf is found down to the Isthmus of Panama, (pp. 44, 45.) This extensive range will cover all the ground assigned by Agassiz to every tribe, form of skull and shade of colour, of his Arctic, Mongul, European and his American man. The beaver and the otter are found at the arctic ocean, range through Canada and the whole of the United States; both exist in the rivers

We might extend our remarks to the mink and several other quadrupeds, existing at the arctic ocean, whose extensive ranges give evidence that they are not willing to be confined to the narrow limits of our friend Agassiz' arctic fauna. Of the birds, Prof. Agassiz has only selected the eider duck, whose migrations during winter, seldom extend to the south of Boston or New-York. But why has he done this? Why not have selected as representatives of his arctic family, the snow goose, Canada crane, the golden plover, the red phalarope, etc., that extend from the farthest north, through Canada and the United States, to the Pacific ocean on the west and to Mexico and the tropics on the south? The snow goose and golden plover, are also common to both continents, and are abundant in Asia and Europe. In land birds, why did he not select the raven, (*Corvus corax*), which was "found in the most northern parts of the arctic regions," "had its residence in the high cliffs of Fort Bowen," and "approached the ships" embedded in the ice, in search of food." "During the winter, they were frequently observed to have a white ring around the neck, caused by the accumulated encrustments of the vapour of their own breath, and giving them a very singular appearance." (Zoology, Par. Voy. p. 6.) This bird is also a permanent resident in Canada, and in all the States of the Union, except on the seaboard. We have seen it in the mountains of Virginia and Georgia. According to Capt. Sitgreaves, (p. 8,) "It is abundant in Texas, New Mexico and California." It is found in Oregon and the Russian settlements, on our North Western continent. It exists moreover, in Asia, and is a common bird in all Europe, and is a permanent resident every where. We believe Prof. Agassiz has somewhere described our American raven as distinct from the raven of Europe. If he will take the trouble to institute a comparison at the Academy of Natural Sciences, at Philadelphia, where many specimens from both continents exist, we will venture to say that he will not find the difference of a feather or a shade. The views we here express are also in accordance with those of Mr. Cassin, at the Academy, one of the best ornithologists in America. The raven would have been a fine specimen to prove how little dependence could be placed on arbitrary realms. This, as well as hundreds of other species of birds, like the wolf, ermine, beaver, otter, etc., are constituted like man, to brave all climates and go wherever they can find food to support them.

Of the extensive ranges of the right whale, in the Pacific and Atlantic oceans, we will, on another occasion, give an account, from Lieut. Maury's admirable essay and chart. In the plants of the arctic fauna, Prof. Agassiz might have selected the winter green, (*Pyrola rotundifolia*), and a dozen others which we intend to bring to his notice, that exist not only in his arctic fauna, but in the north of Europe and Asia, and are described by Elliott, Nuttall and Eaton, as existing as far to the South as Carolina, and we would add, that we have found specimens of the *pyrola rotundifolia*, in the mountains of Georgia. Even in his selection of the rein deer moss, (*Cenomyce rangiferina*) he was peculiarly unfortunate. It is true it exists within the arctic circle, but it extends over the whole of the north of Europe. We have met it in England and Scotland. It is described by Hooker and other European Botanists. In America, it extends through

Canada and the United States. We have seen it in the mountains of Virginia, and Mr. Henry Ravenel, an excellent botanist, has recently found it in South-Carolina.

We now proceed to notice his Esquimaux man, whom he has restricted to the polar regions. Blumenbach classed the polar family among the Mongolians. Morton followed his example, (*Crania americana*, p. 6, as also in his map, inserted at the end of his preface.) In a recent admirable work of "Latham on the Varieties of Man," (p. 286,) and in that of Martin's *Natural History of Man and Monkeys*, (p. 220,) the Esquimaux and the whole hyperborean families are arranged under the head of Mongolidæ..

If, however, Prof. Agassiz adopts a new theory, makes a new subdivision of the human family, in opposition to the teachings of Blumenbach, Morton, Pickering, Latham, Martin and others,—sketches out "the natural provinces of the animal kingdom, and prefixed a tableau to accommodate it to the sketch, surely he would not insist, that the world should adopt his theory, or be satisfied with his tableau, without a rigid examination of all the facts that have any bearing on this subject? If, then, the Esquimaux, according to Morton and the most reliable authors, is a part of the great mongol family, and if, according to Morton's map, it extends to the tropical line on the eastern continent, and according to Pickering, "the American continent is still inhabited by Mongolian tribes; and whilst some of them wander to the north, farther than civilized man has hitherto been able to follow, others are still the nearest dwellers to the southern pole." (*Physical History of man*, p. 16,] then it would seem that the Mongul and his varieties has scarcely a wider distribution than the wolf and the ermine, the beaver, cougar, the snow goose, the wild mallard, and many plants. In this case, small provinces would apply to a small number of inferior animals and plants, but the circle would require to be greatly widened to include an immense number of the higher order of animals; and finally, of man, the lord of this lower creation, and the most perfectly organized being on earth.

4. A review of Morton's iedited manuscripts.

5. Geology and Palæontology in connection with human origins, by W. Usher, M.D.

6. Hybridity of animals as a test of species. The great majority of cases presented by Dr. Morton, we have already scrutinized—all the new ones collected before his death, will be examined. Inasmuch as Dr. Nott stated (p. 275,) that in placing the question before the public as it actually stands, he "used not only Dr. Morton's ideas, but his language, freely throughout the chapter," it will be unnecessary to enter into any controversy with him.

tions and the necessary fatigue of composition, must be pursued during those snatches of time, that would more properly belong to recreation or to rest.

JOHN BACHMAN.

To be continued.

P. S.—We are induced yet to offer a few remarks on the bearing of the doctrine of the Unity of the Human Race, on the domestic institutions, and vital interest of the South; and this note is intended expressly for the mature reflections of our Southern community. Politicians who resorted to DeBow's and the Southern Quarterly Reviews for arguments in defence of slavery, such as have been instilled into their minds by Dr. Nott and others, have been taught to believe that the best arguments that could be urged in favor of slavery, was to prove the negro of a different species. Hence, those who have supported the doctrine of the unity, have sometimes been stigmatized as abolitionists and the enemies of the South.

We trust that one who has never lived in a State where no slavery existed, and has been identified with the institutions of South-Carolina, for the last forty years, one who in the North as well as the South, openly and fearlessly defended them on every necessary occasion, may be allowed to express his sentiments on this subject without the danger of having his motives misinterpreted.

The following are our views. That all the races of men, including the negro, are of one species and of one origin. That the negro is a striking and now permanent variety, like the numerous varieties in domesticated animals. That varieties having become permanent, possess an organization that prevents them from returning to the original species, although other varieties may spring up among them. Thus the many breeds of domesticated animals that have arisen, some only within a few years, would never return to the form of the wild species, without an intermixture. That the negro will remain as he is, unless his form is changed by an amalgamation—which latter is revolting to us. That his intellect, although underrated, is greatly inferior to that of the Caucasian, and that he is therefore, as far as our experience goes, incapable of self-government. That he is thrown on our protection. That our defence of slavery is contained in the holy scriptures. That the scriptures teach the rights and duties of masters, to rule their servants with justice and kindness, and enjoin the obedience of servants.

The opposite doctrine, is that the negro is a different species, having a different origin. That the Bible, which teaches the doctrine is not inspired, no more than a book of fables,—that therefore nothing that is said by Moses, by the prophets, or by St. Paul on this subject, is to be believed.

We would here observe, that if every word of this were true, and the negro was proved to be of a different species, it would not release us from our obligations as men and Christians. That if they could even succeed in proving the negro of a different species, the South would gain nothing, whilst we would have abandoned all the strong arguments that are derived from the scriptures in the right of holding this species of property.

It should be borne in mind, that when the most violent abolitionists of the North, Garrison, Abbey Kelly, etc., were met by arguments from the Bible, in favour of our institutions, they openly renounced the Bible which taught this doctrine, and denounced all the institutions of Christianity. Thus our belief in the diversity of races, brings us no advocates from the open infidel.

The next class is that to which Dr. Nott belongs—who deny the historical veracity of the scriptures. To these belong the men who advocate the views supported in the *Christian Examiner*, who published the papers of Agassiz. As far as we are informed, they of the North are abolitionists to a man, of which the learned, excellent and almost orthodox Channing was the representative of the moderate, and Theodore Parker, (who is so favourably quoted by Dr. Nott, to prove the great superiority of the Caucasian,) that of the most ultra and abusive party. They abandoned the inspiration of the scriptures, but this rendered them more rabid in their curses of slavery and the South.

Knox, who wrote "the Races of Men," and advocated some of the doctrines held by Nott and Gliddon, in regard to the plurality of races, was, as far as we can gather, from such a heedless, rambling writer, equally an unbeliever in the historical veracity of scripture. He advocated some of the most absurd doctrines. He appears to have been a radical in politics, and a transcendentalist, leaning on the theory adopted by Oaken. He believed that races born in one region, would soon die out, when removed to another: hence he pronounces the coming doom of America, which had received its inhabitants from Europe. "The ladies," he informs us, "early lose their teeth." "In both sexes the adipose cellular cushion interposed between the skin and the aponeuroses and muscles disappear, or at least loses its adipose portion; the muscles become stringy and show themselves; the tendons appear on the surface; symptoms of premature decay manifest themselves."

"Now what do these signs, added to the uncertainty of infant life in the Southern States, and the smallness of their families in the North, indicate?" The climate has not been made for him nor he for the climate. [p. 57.] Dr. Nott, although not prepared to endorse this assertion, about the Americans dying out, because they have removed from their European soil, still thinks this subject worthy of enquiry. (pp. 68, 69.) A glance at the hardy sons and daughters of New-England, and of the Germans in Pennsylvania, among whom there have been few admixtures, since the days of the revolution, and their large families—together with the unexampled increase of our population from 3 to 25 millions in three quarters of a century, ought to dispel all apprehension of the descendants of Europeans dying out in America. This book is filled with all manner of absurdities, and full of prejudices against, and abuses of our country. But because it advocated a plurality in the races, it was favourably reviewed in the *Southern Review*. The writer who was evidently unacquainted with the whole subject, referred to us in no very flattering terms. Knox, had said—"He (the negro) is no more of a white man, than an ass is a horse or a zebra." This was favorably quoted by Morton, and might sound very pleasantly in the ear of a Southern politician, but he could not be very highly flattered by the sentence which this believer in the plurality

of the races passes on him. "Employ the coloured man; your brother, as a slave, and live under the terrible fear of his vengeance,—terrible when it comes as come it will; unrelenting, merciless. A million of slaveholders cut off in cold blood to-morrow, would call forth no tear of sympathy in Europe; 'bravo' we should say; the slave has risen and burst his chains—he deserves to be free." (Knox, p. 163.)

Having gained nothing from those who discard the Bible, as an uninspired book, what, we ask, has produced that unexampled unanimity of sentiment that now exists in the whole South, on the subject of slavery? Our table is crowded with pamphlets and sermons, with speeches in Congress, and dissertations by clergymen and laymen, on the subject of slavery. The teachings of the Bible are the most effective weapons that are used. There it is shown that the ancient Israelites were the owners of slaves. There the duties of master and servant are taught, and there is Paul's letter to Philemon, entreating him to receive back his runaway slave. Our representatives in Congress used the arguments contained in the scriptures, and their opponents dared not tell them that the historical parts (and all that refers to slavery is historical) were uninspired and untrue.

Moreover, it should be added, that the doctrine of the diversity of species can never be maintained. It is as contrary to science as it is to the scriptures. The suspicion of interested motives, will be excited against the South in supporting a false theory by a denial of the holy Scriptures. We can effectually defend our institutions from the word of God. When we abandon that strong tower of defence, we are left without weapons exposed to the ruthless assaults of its enemies. In a political point of view, we regard the effort made by Nott and Gliddon, to establish their theory by a denial of the veracity of the historical Scripture, as more dangerous to our institutions, than all the ravings of the abolitionists.

The Editor of the Southern Review, in announcing a review on Nott and Gliddon's book, from a "competent hand," says of the "Types"—"Of course it is full of heresies; but for our own part, we really like heresies and encourage them on all occasions, they serve as thunder-storms," etc., (p. 270) We have no reason to doubt his assertions. On our part, however, we neither like the "heresies" nor the "thunder storms" that follow in the train. As long as true naturalists write on science, and do not meddle with the Scriptures, no one will be dissatisfied with them; but when they wish to establish a false theory in science, that can only be supported by an attempt to destroy the veracity of the Scriptures, they cannot calculate on the patronage and support of the christian community. The 'heresies' of Dr. Cooper, promulgated at the South-Carolina College, have left deep traces on the minds of the succeeding generation, which no "thunder-storms" have as yet "purified."

In the same number, the publisher announces that his vocation ceases with that number, unless there should be an increase of patronage. It is not likely that those who for years past, have been consigned to the tender mercies of Dr. Nott, with whom, from a sense of self-respect, they cannot enter into controversy, will bestir themselves to increase its patronage.

J. B.

ART. X.—*Erichsen's System of Surgery*. Edited by JOHN H. BURN-
TON, Philadelphia.

FROM the large number of works on surgery lately published, it would appear to be essential to establish the reputation of a surgeon, that he should publish a treatise, and so widely spread is this weakness to see one's name among the honored list of authors, that we find works coming from nearly every surgeon who holds any conspicuous position either in a hospital or university. They consider it an obligation which they owe to the profession and which must, at all hazards, be cancelled. Necessarily among these is found a great deal of sameness. Book after book follows the same old routine. Sometimes old established rules are broken through, classifications destroyed and new arrangements of subjects made, more for the sake of a change, than offering any valid or useful improvement.

It was with such views of recent works on surgery that we glanced over *Erichsen's System*, but were surprised to find that we could be so interested in its perusal. His style is so terse and language simple and precise, that we could see at once that the author must be one who knows well how to instruct, and that his object was to impart practical information to the student. In this he has been eminently successful. From the preface we learn that the book contains the substance of a course of lectures which Mr. Erichsen, as a Professor in University College, London, has been in the habit of delivering. His intention at first was to confine himself to surgical injuries and diseases and the operations required by them. But, to make it more useful to students, he has prefixed some remarks on the nature and treatment of inflammation and also general observations on operations, but more particularly amputations. These he has condensed in the first fifty pages, devoting nearly nine hundred pages to special surgical diseases, with a full description of their pathology; using physiology to elucidate any obscure point and entering largely into detail concerning the most approved and best method of treating them. For pathology he refers us principally to the excellent work of Paget, from which he has drawn largely. This surgical treatise, besides the large amount of instructive matter which it contains, is exceedingly valuable on account of the numerous statistics of operations which have been collected for each special disease, and which are very useful in forming a correct prognosis.

and utility of existence; and the more certainly the surgeon can accomplish this the better will he do his duty to his patients, and the more successful will he be in practice. Success, then, is the result of an operation, whether that result be the preservation of life or the removal of a source of discomfort. To this, dexterity and rapidity in operating are in the highest degree conducive, but there are various other considerations equally or still more necessary, the solution of which can only be afforded by an intimate general acquaintance with the science of surgery and medicine. The diagnosis of the nature and extent of the connexion of the local disease has to be made, lurking visceral affections must be detected and if possible removed. The constitution of the patient must be prepared for the operation; the best time for its performance seized, and after its completion the general health must be attended to in such a way as shall best carry the patient through the difficulties he has to encounter, and any sequelæ or complications that arise must be met by and must be subjected to appropriate treatment. There, as well as the simple performance of the operation, are the duties of the operator, and on the manner in which these are performed, as much or even perhaps more than on the mere manual dexterity displayed in the operation itself, will the fate of the patient depend. It is well known that the result of operations differs much in the practice of different surgeons of acknowledged dexterity, and this variation in the proportionate number of recoveries cannot be accounted for by any difference in the degree of manual skill displayed in the operation itself, but must rather be sought in the greater attention that is paid by some surgeons to the constitutional treatment of their patients before and after the operation and their more perfect acquaintance with the general science and practice of surgery."

He then goes on to state that success in operative surgery depends greatly upon the selection of proper cases, and that the patient's constitution influences the success of an operation far more than the mechanical dexterity of a surgeon. He says what is verified every day, that nothing is more common than to see patients sink after the most dexterously performed operations for hernia, stone, or the ligature of arteries, &c., owing to some constitutional vice, whilst, on the other hand, some patients will make the most remarkable and rapid recoveries after they have been mutilated with but little skill.

He then mentions some general diseases which would render doubtful the success of operations; among these we find persons of irritable and anxious minds or of feeble and irritable habit of body, as nervous and hysterical females, &c., also patients suffering from cardiac affections, chronic disease of the lungs, and more particularly those affected with diseased kidneys. If any tendency to erysipelas exists, the operation should always be deferred, if possible. Chloroform he uses freely and extols highly in all cases, except a few diseases of the heart; even in such he has used it without any bad effects following. He deprecates the system of dressing the wound immediately after operations and invariably adopts the plan recommended by Liston of leaving the wound open, with a piece of wet linen interposed between its lips for two or three hours, till all hemorrhage has ceased and the surface has become

glazed. The linen is then removed, all small coagula are taken away and the lips united by suture.

After giving a detailed account of the manner in which amputations should be performed, he introduces statistics to show the relative mortality between amputations performed for disease and for injuries. Of forty five cases operated upon for injuries in University College Hospital fourteen died, being thirty-one per cent. Of ninety-five amputations for disease, eighteen died, which is only nineteen per cent. In Malgaign's statistic tables we find that one hundred and seven patients died of one hundred and sixty-four amputated for injuries, being sixty-five per cent. Of three hundred and fifty-five amputations for diseases, one hundred and seventy-four were fatal—forty-nine per cent. These results are explained by the greater frequency of shock after injuries requiring amputations, and the greater liability to gangrene and pyemia. As a rule the more chronic the disease the greater the chances of success. If these two tables could be taken as a fair criterion of English and French surgery, what a wide difference exists in favour of the English practice, their mortality being one in five, whilst the French lose two-and-two-thirds in five, more than one-half of their patients. This proportion, bad enough as it is, might even be considered above the general average, for Malgaigne is spoken of as a very successful operator, from the simple fact of his having adopted the English treatment of wounds, which he is making great efforts to promulgate, in the face of the universal custom of sharpie packing.

In speaking of the dangers of amputations for injury, tables were made showing a much greater mortality from primary than from secondary amputations after inflammation had set in. This is readily explained by the greater severity of injuries demanding immediate interference than those in which it is thought justifiable to attempt to save the limb. We should not infer from these statistics that primary amputations are objectionable and should be delayed till suppuration commences, for of two alternatives, leaving a badly crushed and mangled limb, exposing the patient to gangrene, erysipelas, pyemia, &c., or removing it at once, the latter is always to be adopted as exposing the patient to the least danger.

Although as a general rule he prefers deferring amputations until reaction comes on, there are some cases in which the presence of a crushed limb appears to prolong the shock and prevent the patient from rallying, notwithstanding the administration of stimulants. Under these circumstances a surgeon would be justified in operating before action is restored, and in such cases chloroform is extremely beneficial by preventing the pain and dread of the operation from still further depressing the vital energies. In these cases of long continued shock, great care is required in ascertaining that there is no internal injury giving rise to the depression, but that the shock is really dependent upon the mangled limb.

lations springing up from the sides and bottom and thus filling up the wound, and fifthly, by the growing together of two granulating surfaces. Most surgeons are satisfied with two modes of healing wounds, one by adhesion through the medium of lymph deposited between the cut surfaces, the other by granulation. These two divisions are sufficient for all practical purposes, and surgeons treat wounds accordingly. The three first divisions of Erichsen's system appear to be unnecessary subdivisions of what we call union by the first intention. No wound, however small, can exist without some minute vessels are injured, which amount of irritation is sufficient to cause a lymph effusion. This lymph is always the agglutinating substance. The first method of healing wounds is supposed to be the simple and direct coalescence of the opposed surfaces; they grow together in a few hours, without the intervention of inflammation or its products. This variety of union, he says, is exceedingly rare. *Healing by scabbing* consists in the direct adhesion of the lower part and sides of a wound under a crust of dried blood, which forms an air-tight covering. The absence of inflammation is necessary for this mode of healing wounds. The third division is that universally adopted as the ordinary mode of healing wounds. In discussing the injuries of nervous tissues and speaking of the unpleasant consequences sometimes resulting from them, more particularly in delicate women, he mentions having more than once seen a puncture of one of the digital branches of the ulnar nerve produce a kind of painful paralysis in its trunk, rendering the arm nearly useless. He has seen the same effects happen in the median nerve, from the puncture of the finger with a needle, a very slight cause for so serious a result as paralysis of the arm.

Wounds of veins are dangerous and sometimes fatal from loss of blood, from the occurrence of diffused inflammation of the vessel, or from the entrance of air into the circulation. In experimenting upon the introduction of air into the veins, he has observed the following phenomena: on exposing the internal jugular vein low in the neck and puncturing it at a place where the flux and reflux of the blood are plainly discernable, there is perceived in the first inspiratory effort made by the animal after the wound, a peculiar lapping or gurgling, liquid, hissing sound, the nature of the sound depending partly on the size and the situation of the opening in the vessel. At the same time a few bubbles of air are seen to be mixed with the blood at the orifice in the vein. The entrance of air is immediately followed by a struggle during the deeper inspirations in which fresh quantities of air gain admittance, the entrance of each portion being attended by the peculiar sound above described. On listening now to the action of the heart, a loud churning noise will be heard synchronous with the ventricular systole, and the hand will, if applied to the parietes of the chest, perceive at the same time a peculiar bubbling, thrilling or rasping sensation, occasioned by the air and blood being as it were whipped together between the columnæ carneæ and chordæ tendinæ. As the introduction of air continues, the circulation becomes gradually more feeble and languid, the heart's action, however, being fully as forcible, if not more so than natural. The animal soon becomes unable to stand; if placed upon its feet it

rolls over on one side, utters a few plaintive cries, is convulsed, extrudes feces and urine and dies. If the thorax be immediately opened, it will be seen that the heart's action is continuing regularly and forcibly and that the pulmonic cavities, though filled, do not appear distended beyond their ordinary size.

He thinks that death occurs in consequence of the air and blood being beaten up together in the right cavities of the heart, into a spongy froth, which cannot be propelled through the pulmonary vessels; hence there is a deficient supply of blood to the brain and nervous centres, and fatal syncope comes on, attended usually by convulsions. The common supposition is that the air, distending the cavities of the heart, paralyzes its muscular walls, cuts off the supply of blood from the brain and death ensues. The indications laid down for treatment are, first, to keep up a due supply of blood to the brain; secondly, to maintain the powers of the heart until the obstruction in the pulmonic capillaries can be overcome or removed; thirdly, to remove, if possible, the obstruction in the capillaries of the lungs. These he fulfils by, first, plugging up the wound to prevent further ingress of air; secondly, compressing the aorta and axillary arteries so as to divert the whole of the blood that may be circulating in the arterial system to the brain to supply the deficiency, a valuable piece of advice recommended by Mercier; thirdly, to keeping up artificial respiration so as to maintain the action of the heart until the capillary obstruction has been removed, the air either being dissolved by the blood or passing through the capillaries into the air-cells of the lungs, disappears.

In the treatment of wounded arteries he says that the surgeon should never be satisfied until he has secured both extremities of the injured vessel at the point wounded, and not be contented to practice the Hunterian principle of tying the artery higher up the limb; for the hemorrhage will invariably continue from the distal extremity of the vessel. The accident of secondary hemorrhage following the ligature of a wounded artery, usually happens from the distal and not from the proximal side of the wound, from its greater disposition to slough, having its vasa vasora destroyed, and partly from the less perfect occlusion of this portion of the artery. The ligature must, in such cases, be applied to the bleeding point if possible. If hemorrhage can not be checked by other means, the *derniere* resort is amputation.

In treating fractures, he adopts Paget's views concerning the formation of callus, viz: that superficial bones heal differently from those more deeply seated. In superficial bones no deposit occurs between the fragments to form a provisional callus, but union ensues, to take place di-

patient is not confined to the bed, but as soon as the bandage is perfectly dry and stiff he may move about without any fear of retarding the process of consolidation, whilst under the old manuel he had to expect at least a confinement on his back of six weeks or two months. He uses the many tailed bandage thickened at the site of the fracture, by strips of pasteboard well smeared with thick starch or folds of coarse brown paper, saturated with the same substance. A temporary splint is used whilst the apparatus is drying, which is applied on the outside of the bandage to keep the limb to its proper length and shape. In the course of three or four days after its application, the apparatus will usually be found to have loosened somewhat, the limb appearing to shrink within it. Under these circumstances it becomes necessary to slit it up with a strong pair of scissors, along the most muscular part of the limb. The edge is pared off and the apparatus reapplied by means of tapes or a roller, so that the limb is again firmly grasped. In some cases he has found it advantageous to envelop the whole limb in a thick layer of cotton wadding before applying the starch bandage. This being elastic accommodates itself to the diminution in the size of the member and thus keeps up more equable pressure. If the fracture is compound, a hole can be cut in the bandage opposite the seat of injury. The moderate pressure of the bandages, aided probably by the great evaporation that goes on during the drying of so extensive and thick a mass of wet starch which produces a distinct sensation of cold, takes down the extravasation in the limb most effectually, and enables the patient to leave his bed about the third day after the injury. So that very commonly all patients with simple fracture of the leg and many of those of the thigh, especially in children, are treated as out door patients. This practice has been in use for some time in the French hospitals. Four years ago we saw Velpeau applying dextrine bandages to cases of fracture on the morning of their admission into his wards.

In his description of special fractures he explains the singular displacement in fracture of the inferior portion of the radius by impaction or a locking of the fragments, which accounts for the great degree of traction usually required to remove the deformity and the absence of crepitus until this forced traction has been employed. This view he has formed from the examination of many specimens and from the dissection of recent ones. In the treatment of this injury he uses an angular or pistol shaped splint on each lateral surface of the arm to which the arm and hand are firmly attached by a roller.

Transverse fracture of the patella unites most frequently by a thickening of the aponeurotic structures or facie, which envelope the bone and seldom by a ligamentous or fibrous band, which most surgeons consider the ordinary mode of union.

In fracture of the ribs he suggests the application of a roll of adhesive plaster around the chest, instead of the ordinary bandage. The plaster must be a foot wide and should be sufficiently long to make one and a half turns round the body. It should be applied very tightly and should be left on for ten or fifteen days, when it might require renewing. It supports the chest more firmly and uniformly than an ordinary bandage, affording the patient great comfort. When on disloca-

tions he mentions a very singular variety of spontaneous luxation occurring, without any apparent disease of the joint and without pain, the patient's attention being attracted to it by the deformity of the limb. It occurs in paralytic limbs or in those suffering from a rheumatic or neuralgic condition of the joints. He mentions a case having entered his ward, in which there was a spontaneous dislocation of both shoulders and hips. We can not see how such a complete dislocation could possibly occur without laceration or destruction of the ligaments, and these are by no means painless, imperceptible lesions.

In injuries of the skull and simple fractures, he still holds to the old practice of using active precautionary measures which, he says, should be adopted without delay, with the view of guarding against the occurrence of inflammation of the brain and its membranes, even though no symptoms have as yet declared themselves, viz: blood freely taken from the arm, free purgation, head shaved and ice constantly applied, &c. The bleeding should be repeated frequently and should never be omitted, as it is of more service than any other means. The lancet appears to be the great remedy with all English surgeons and is in universal use. We are not such enthusiasts of blood letting, and certainly never use it until absolutely called for. We are as successful in the treatment of injuries of the skull without such active precautionary measures. Blood letting is very badly borne with us, perhaps from some modification of our systems by our climate. Surgeons, with a very extensive practice seldom or never resort to the remedy.

In injuries of the chest connected with internal hemorrhage, the first indication consists in diminishing the quantity of blood circulating through the lungs, and thus endeavor to arrest the bleeding from these organs. With this view, free venesection to the extent of thirty ounces or more should be practiced. Hemorrhage might be at once stopped by this proceeding. If it returns resort must be again had to the lancet, and repeated again and again till it has had the desired effect. The most experienced surgeons are unanimous in their opinion that at this stage of the injury the patient's safety lies in free and repeated venesection. The diet is nearly absolute, ice and barley water. When hemorrhage has been checked the blood must be early let out of the chest by free incisions, or it will speedily putrify, giving rise to extensive formation of pus in the pleural cavity.

In injuries of the intestine, with wound requiring sutures, he prefers that the needle should only penetrate the peritoneal and cellular coats; no muscular tissue being taken up in it, lest retraction of the included fibres by dragging upon the stitches, might re-open the wound. We think that much greater security is obtained by transfixing all the intestinal coats, without increasing, in the slightest degree, the dangers which are likely to arise, and at the same time the ligature can more readily escape into the intestine. In those cases in which we have seen the intestinal wound closed by a simple interrupted suture, the ends cut off close to the knot, and the wound healed, we think that the best method is to

just above the ankle, or if not there, immediately below the knee. The arrest is accounted for by the greater freedom of the collateral circulation here, than in other parts of the limb. In traumatic gangrene he amputates at once, never waiting for a line of demarcation. In senile gangrene he recommends the practice of Mr. James and Mr. Garlike, of amputating the thigh high up, as very successful, when the health is otherwise good and the constitution tolerably sound. The operation should be performed as near the trunk as possible, as there is a better chance of meeting a healthy condition of the vessels and a greater degree of vitality in the tissues.

Whillow meets with rather a singular classification under the head of erysipelas of the fingers. He considers it truly an erysipelatous affection for the following reasons:—1st. Because the causes, whether of season, infection or local irritation, appear to be the same in both affections. 2dly. The constitutional disturbance is always very severe for so slight a disease, and assumes the same character of speedy depression that we observe in erysipelas. The inflammation of the affected finger is invariably diffuse, never being bounded by adhesion, but always tending to terminate in suppuration and sloughing; and, lastly, so soon as the disease spreads beyond the affected finger, or to the back of the hand, it assumes a distinctly erysipelatous appearance and character.

In a discussion on *pyemia*, he differs from most pathologists in his explanation of the morbid condition. He answers in the following paragraph, the question to what are the symptoms and destructive effects of *pyemia* due. He says "there can be little doubt that they are owing to the alterations that take place in the blood itself. It is only in this way that the remarkable diffusion of the disease, the variety of tissues affected, and the wide spread tendency to suppuration that characterizes it, can be explained. That the blood undergoes important changes in this disease is unquestionable; it is thin, dark-coloured, and after having been drawn from the body forms a loose spongy coagulum, from which a moderate quantity of rather turbid or milky-looking serum separates. On examination under the microscope, it will be found that besides the ordinary red globules, the blood contains often in large quantities, corpuscles that in some cases closely resemble the ordinary white ones of healthy blood, and at others present such exact similitude to puscells, that the most practiced eye fails in detecting a difference. These corpuscles may be few in number, and at other times so abundant that they occupy the field of the microscope to the exclusion of the red. The existence of these corpuscles in the blood, which I believe will invariably be found on careful examination, more especially in that taken from the larger views, constitutes one, if not the essential element of the disease. That they are true puscells, in many instances, would seem to be probable from their microscopic appearances; in others again they do not present the true characters of the puscell, differing from it in shape, or in the absence of a nucleus, and in their more irregular outline, resembling indeed more closely the white corpuscles, or some of the ill-developed granulation or exudation cells, that are met with in lymphatic abscesses occurring in cachectic constitutions.

He does not think that all cases of *pyemia* are dependant on suppura-

tive phlebitis, for in some of his post mortem examinations no such lesion could be detected. Although frequently co-existing with, he thinks it may occur independently of suppurative phlebitis. For the occurrence of this affection, there must be some local inflammation of a suppurative character, in a broken down constitution, forming aplastic lymph. This fibrin forming in the blood may assume the corpuscular character, and the cells which are invariably met with in this disease are, in reality, the conditions under which super-fibrination of the blood, with corpuscular lymph, would necessarily show itself; and as plastic fibrin with the blood would occasion athenic inflammatory fever, so the formation of aplastic lymph in that fluid may occasion a corresponding low type of constitutional disturbance.

In describing tumors, he makes a division of semi-malignant holding an intermediate stage; some of which although they resemble benign growths in appearance, under certain circumstances might increase very rapidly, taking on the characteristics of malignancy, whilst some perfectly innocent, cartilaginous looking tumors, differ in structure from the normal tissue.

The treatment of malignant tumors by pressure is one which has been recently reclaimed, and is said to possess certain advantages in some cases. Although there are no well authenticated cases in which pressure has effected a cure, there are many reported in which the growth and progress of the tumor have been retarded and the sufferings of the patient relieved. Pressure can only be used in the occult stage of cancer, and should be employed early. Mr. Erichsen has not been more successful than other surgeons, in curing cancer by the knife, although he thinks that in several instances it has been the means of prolonging life and ameliorating the condition of the patient. He recommends the operation to be performed as early as possible, and with the use of chloroform he is an advocate for operations at all stages of the disease.

As a syphiliograph, Erichsen endorses the views of Ricord, viz: two distinct specific diseases, gonorrhoea and syphilis, both arising from distinct specific poisons, and followed by a distinct train of symptoms. As sequelae of gonorrhoea, he mentions orchitis, conjunctivitis and scleritis by metastasis; also, rheumatism and cutaneous eruptions. In syphilis, he makes the same divisions, and follows the treatment of Ricord.

Among aneurisms we find some valuable statistics of the ligature of the arteries, according to the different methods, principally of carotid, subclavian and innominata arteries. His remarks under these various heads are well worthy of a careful perusal.

In treating acute arthritis, he advocates Mr. Gay's practice of making free incisions into suppurating joints, as a great improvement over the common method of using punctures. When small openings are made, air becomes mixed with pus in the cavity of the articulation, which renders it offensive and irritating, and as it cannot readily escape, it increases the local mischief as well as constitutional disturbance. By freely laying open the joint all this is prevented; exit is given to the pus through one or two incisions that extend the whole length of the articulation. No constitutional disturbance can occur from pent up and putrid matter, and the joint stands a better chance of healthily granulating.

Mr. Erichsen, although a strong advocate for caustic applications in chronic laryngitis and diptheritis, places little confidence in the report of those surgeons who sponge out the air tube with as little difficulty as cleaning out the nares. He does not believe that this practice, though commonly spoken about and professedly employed, is ever in reality carried out, from the acutely sensitiveness of the glottis and its resistance to foreign bodies by violent spasmodic fits of coughing. He says "not only does physiology and ordinary experience tend to disprove the possibility of such a procedure, but repeated experiments, both on the living and on dead subjects, have led me to the conclusion that it is utterly impossible to pass a whale-bone, whether curved or straight, armed with a sponge, beyond or even between the vocal cords. I have frequently endeavoured to do this in cases of cut throat, in which the air tube having been laid open, the passage of the sponge could readily have been observed, and in which the facility of doing so would have been greatly increased by the larynx being no longer subservient to respiration; but in no one instance have I succeeded in passing the sponge so as to make it appear at the external opening. From the repeated observations I have made on this point, I have no hesitation in expressing my conviction that the sponge has never been passed in the living subject, beyond the true vocal cords, though I believe that, in some instances, with the requisite dexterity, it is possible to introduce it for a moment between the lips of the glottis, so as to apply the solution freely to these parts, not however without inducing the most intense and spasmodically suffocating cough." When requisite he uses the nitrate of silver in powder, passed in a tube to the back of the mouth and drawn into the larynx during inspiration, or he uses a syringe filled with a concentrated solution of the salt.

So many with us are in the constant practice of swabbing the throat in tracheal and laryngeal affections, that there is no longer any doubt about the practicability of passing the sponge into the trachæa, and some from constant practice have acquired such dexterity, that the sponge can even be carried into the bronchias as seen by the handle of the instrument protruding from the mouth inclining in the opposite direction to the tube in which it has entered.

Hernia, with its anatomy, pathology and treatment, is the subject of a long dissertation, in which, however, we find nothing new.

After discussing the pathology and treatment of stone in the bladder, we find a comparison drawn of the relative merits of lithotomy and lithotrixy, from which we will make a few extracts.

"Lithotomy and lithotrixy differ so entirely from one another that it is useless to attempt to establish a comparison between the different steps of the operation. It is, however, a question of very considerable importance and interest to ascertain by which operation a patient can most safely have a calculus removed from his bladder. In determining this point it is not only necessary to make a comparison between the results of cases that have been subjected to the two procedures, but more especially to determine those circumstances that influence the result of either operation in particular cases—to determine, in fact, in what

cases lithotomy, and in what lithotripsy holds out the best prospect to the patient. I should unquestionably consider lithotomy as more generally applicable, although there are many cases in which lithotripsy is preferable. Where either method offers an equal chance of success, the preference should be given to the crushing operation. From statistics very little information can be gained in comparing the results of the two operations. For crushing, the cases are always selected with care, whilst we are compelled to cut any case that offers. Although Civiale lost but one in forty-two, Malgaigne estimates the mortality as one in four in hospital practice, and one in eight in private practice. Dr. Mott lost but one in fifty cases of cutting for stone, whilst Civiale lost one-half of his patients from lithotomy.

"The circumstances that chiefly determine the surgeon in his selection, are the age of the patient, the size and character of the stone and the general condition of the urinary apparatus. Age is an important element; children always stand cutting well. In old age, lithotripsy is greatly to be preferred. Where the calculus is very hard, as of oxalate of lime, or lithic acid, or if of very large size, or if strictures exist in the urethra, or the bladder diseased, or the kidneys throw off an albuminous secretion, cutting should be practiced. Where the stone is small or soft and where the urinary organs are healthy, lithotripsy is followed by the best success. Recurrence of the calculus is much more common after crushing, evidently arising from some fragment remaining as a nucleus in the bladder. The relapses in Civial's cases were one in ten."

The last subject treated in the work is ovariectomy, which our author is inclined to recommend and reports a great many cases collected by Lee, Phillips, and others, in which successful cases preponderated. Of two hundred and fifty-three operated upon, one hundred and fifty-two were successful; that is to say, the patients got quite well after the removal of their disease. He prefers the operation of gastrotomy in many instances, to tapping, which is only a palliative remedy. The precautions necessary are, that the tumor be perfectly free without attachments to the uterus, as determined by finding that organ floating on the introduction of a uterine sound; there must not have been any previous peritonitis and the general condition of the patient must be good.

In conclusion, we take pleasure in recommending this work to the profession as a valuable treatise on special surgery, from which they may glean all the improvements in pathology and treatment, in a condensed form. It is also an excellent book of reference, and for students is one of the best works that we are acquainted with. The language is

- ART. XI.—1. *Homœopathy: Its Tenets and Tendencies.* By JAMES Y. SIMPSON, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, &c., &c. First American, from the third Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8vo. pp. 304.
2. *Homœopathy Fairly Represented: A Reply to Professor Simpson's "Homœopathy Misrepresented."* By WILLIAM HENDERSON, M.D., Professor of General Pathology in the University of Edinburgh. First American, from the last Edinburgh edition. Philadelphia: Lindsay & Blakiston. 1854. 8vo. pp. 302.

(Concluded from page 379.)

ADMITTING that the symptoms, recorded in the Homœopathic *Materia Medica* as the effects of a remedy, are such in reality, and that a disease consists in the totality of the visible symptoms only, how, we enquire, can the homœopathic practitioner select a remedy whose symptoms correspond precisely with those of the disease he is treating? The number of medical substances treated of in homœopathic works amounts to about two hundred and seventy-six, each of which is said to produce from one hundred to fifteen hundred symptoms, with endless combinations. The symptoms recorded under each article are, moreover, not different and distinct from those produced by other articles, but they are very similar, each article producing almost every variety of pathological phenomena which can be conceived, so that, if we take into consideration the numerous symptoms of which each disease is said to consist, we see how impossible it is for the practitioner to ascertain at once the remedy whose effects will cover the totality of the symptoms of the disease. And unless the remedy be capable of exciting the entire group of symptoms, which bear the closest resemblance to those of the disease, Hahnemann contends that a certain and permanent cure cannot be effected. He says "that we must, in the first place, form a perfect image of the disease we propose to treat, before we think of applying a remedy, and, as the disease is made up of the totality of the symptoms, one symptom more or less must constitute a new disease."

Supposing it possible to find a remedy whose artificial symptoms correspond exactly with the disease, it would require several days of laborious research and careful comparison before that remedy could be determined, and, consequently, acute diseases would run their course before their remedy was ascertained. This rule, then, which Hahnemann laid so much stress upon, and which, he said, would render "the art of curing as certain as the science of mathematics," is found to be inapplicable,

avail themselves of allopathic—sometimes heroic—doses when the urgency of the symptoms requires more than mere faith, or the patient expects a decided impression to follow the exhibition of the remedy.

“The fate of the Duke di Cannizzarro, well known some years ago in London as the Count St. Antonio, is an exemplification of this. Having to take homœopathic globules at intervals for some slight ailment, in order to save trouble he took three doses at once, and died two hours afterwards; the supposed globules being a concentrated preparation of *nux vomica*.

“I have been told that a strong homœopathic convert, who read the first edition of this pamphlet, gravely maintains, that this case of the Duke of Cannizzarro, as told by Mr. Lee, fully proves that all our reasoning about the inefficiency of infinitesimal doses is evidently wrong, seeing they may be so potent as even to poison a man. It is, however, I believe, almost quite unnecessary to add, that the Duke was *not* poisoned by infinitesimal doses of *nux vomica* in the globules; but by these globules having been fraudulently made up by the attendant physician, so as to contain of *nux vomica*, or rather *strychnia*, larger than any allopath even would use—a kind of fraud which homœopaths have been detected practising upon their credulous patients also in this country. (On homœopathic globules, powders, &c., drugged with large doses of morphia, mercury, &c.—See the *Medical Gazette* for 1847, p. 294; *Lancet* for 1850, p. 300; *Pharmaceutical Journal* for 1851, p. 318.) Dr. Taylor, the distinguished toxicologist and chemist of Guy's Hospital, found one third of a grain of morphia in a powder *professedly* homœopathic and infinitesimal. I was informed, last summer, by Dr. Horace Green, of New-York, of another instance of death, from a gentleman swallowing, in sport, a number of homœopathic globules, which, on examination afterwards, were found drugged, not with infinitesimal and innocent, but with large and poisonous doses of *strychnia*. And a small dose of this acute poison might suffice. A dose of *strychnia* (the essential principle of *nux vomica*), amounting only to half a grain, has proved fatal, within a quarter of an hour, to an adult.

“The author of the ‘Confession of an Homœopathist,’ in referring to the due and adequate drugging of the Hahnemannian doses, amusingly remarks: ‘Patients who are skeptical of the truths of homœopathy, from a love of variety, or a hundred other reasons, will consult you. As these persons are inclined to ridicule infinitesimal doses, it is sometimes highly useful to give them powerful doses of various highly concentrated medicines, in globules similar in appearance to all the rest, but consisting of morphia, strychnine, arsenic, corrosive sublimate, and such like: a few of these mingled with your sugar and starch globules will cause effects to be felt by the skentic which will quickly overcome

Were it known, our wonder-working powers would be reduced in the estimation of the public and the regulars.' P. 197."—*Tenets*, pp. 45, 46.

It may be asked, if homœopathy be so absurd and inconsistent, why has the system been so patronized, and why are its propagandists so ready to defend their doctrines, and to appeal so confidently to experience in proof of the special efficacy and success of their system of treatment? To this we reply, that a recovery succeeding a certain plan of treatment is no proof of the efficacy of that treatment. Nature alone, uninfluenced by medical means, in a large majority of the attacks of disease, is sufficient to restore the constitution to a state of health. Every quack medicine which has had its day has, as every one knows, its hundreds and thousands of supposed cures (coincidences?) to boast of; so it is no matter of surprise that homœopathy, in accordance with the general rule, should boast of its cures, and adduce the results of experience in attestation of them.

"'Pure experience (it is averred and argued by Hahnemann) is the only, the infallible, oracle of medicine,' and the 'idle declamations (of opponents) must cease before the dicta of infallible *experience*.' In this strain, in former times, Bishop Berkeley strenuously, and apparently soundly, argued regarding the universal curative effect of Tar-water; so argued Dr. Solomon regarding his Balm of Gilead; Mr. Perkins regarding his Metallic Tractors; Dr. Morrison regarding his Pills; Mr. Lee regarding the effects of the internal and external use of Brandy-and-Salt, &c. &c. In the present day we have exactly the same proof of 'infallible *experience*' every where as earnestly and anxiously repeated—by Dr. Dickson regarding the results of his Chronothermal treatment; by Mr. Coffin regarding his courses of Lobelia, &c.; by Ling regarding the cure of all diseases by rubbing and movements; by Priessnitz regarding the cure of all affections by the internal and external use of cold water; by Luz in relation to the treatment of all diseases upon the Isopathic principle, '*œqualia, æqualibus curantur*,'—'the same is cured by the same;' by Hahnemann in relation to the treatment of all diseases upon the homœopathic principle of '*similia, similibus curantur*,'—'like is cured by like.' The promulgators, pupils, patrons, and patients of each of these diverse universal systems of cure, are, like Hahnemann and his followers, all equally ready, and equally entitled to appeal to the results of 'PURE EXPERIENCE as the only, the infallible oracle' and criterion of the particular medical doctrine, and exclusive mode of cure, which, for the time being, they severally and zealously happen to favour and follow.

"To decide amidst so many very strong and yet very opposite asseverations, may not always be an easy task for trusting and credulous minds,

in solving this problem, it is far too frequently forgotten, that men labouring under disease, even the most acute, and consequently much more so when suffering under slighter ailments, do not as a general rule die, even when totally and entirely left without any medical treatment whatever. When reasoning on the efficacy of different medicines and different modes of cure, the mind is constantly liable to fall into the greatest errors, by neglecting this leading truth. For when a man, in a state of disease, takes, for the purpose of curing that disease, a millionth or a decillionth of a grain of oyster-shell, or sulphur, or other homœopathic drug, he comes—not very unnaturally, but very illogically—to attribute his cure to the infinitesimal dose of oyster-shell or sulphur, from being unaware that the disease, if left to itself, does very often spontaneously get well, and was virtually left entirely to itself when he took his Hahnemannian dose of oyster-shell or sulphur. While the homœopathic patient anxiously attributes his cure to the infinitesimal dose which he has swallowed, the cure is in reality effected by the natural sanative powers of the constitution (the old *vis medicatrix nature*); or by nature aided, not by medicine, but by Medical Faith; or assisted by proper regimen and strict diet—the last, great and potent auxiliary means, of which all homœopathists very properly and powerfully avail themselves.”—*Tenets*, p. 24.

“The evidence that is necessary to prove or disprove any proposition in the science of medicine, is of a peculiar kind. It differs entirely from that species of proof which satisfies a court of law. Both direct and circumstantial evidence, which would leave no doubt in the breasts of judges and juries, have often not the slightest tendency to render a medical fact even probable. The declarations, and even the oaths, of the most conscientious, disinterested, and able men, are all insufficient. The reason of this is, that few men, even those of considerable capacity, distinguish accurately between opinion and fact. When a man asserts he has been cured of a particular disease by a certain drug, he is apt to think he is declaring a fact which he knows to be true; whereas this assertion includes two opinions, in both of which he may be completely mistaken. The first is an opinion of his having *had* the disease specified; the second, that the medicine employed *removed* the disease. Most people are convinced that they are acquainted with the malady they are afflicted with; they consider it a mere matter of fact; and when they are cured, they have as little doubt of the remedy that accomplished it. This belief is often strengthened by the confident declaration, and specious behaviour, of the person who exhibits the remedy; and if the person possesses gratitude, this also heightens the delusion. He is thus easily prevailed upon to swear positively both to the disease and the remedy, as if they were both plain facts obvious to the senses; whereas, both the one and the other are frequently beyond the reach of human knowledge. Such cases, to the truth of which the patients will

bable the fact is, the more numerous are the affidavits, and the more respectable the signatures. Clergymen, judges, and peers, are daily swearing, that they have been cured of incurable diseases; but the meanest apothecary smiles with contempt when he reads their splendid testimonials. * * * * Yet if any one should attempt, by investigating each particular case, to refute it, he would soon discover the impossibility of succeeding. I have been requested, on various occasions, to make such researches, and generally found that the patients were completely convinced of the truth of the attestation, whether the remedy was physic, magnetism, or a tractor. In short, they were ready to take the most solemn oath to what was quite impossible to be true." —*Moore's Reply to the Antivaccinists.*

Homoeopaths, it appears, are as dupable as their dupes, and sometimes they have the tables turned on them. An instance of which is related in the London *Medical Gazette* for January 20th, 1843.

"A gentleman who had been out of health some time, and had paid but little attention to the direction of his medical attendant, was advised to consult a homoeopathic physician; he did so, and remained under his care six weeks, the doctor making his visit as often as he pleased: he improved in health—in short, was cured. 'Now,' exclaimed his friend, 'you henceforth stand up in defence of homoeopathy.' 'Not at all; I am more than ever convinced of its fallacies and humbug. I have followed the plan of diet, etc.; which plan was often urged upon me by former medical friend, but not one of the billicath powders or globules have I taken.'"

Mere "*Medical Faith*," independently of the exhibition of medicines, can and does exert a powerful influence over the progress of disease, as the records of medicine amply prove; and it is a principle of which homoeopaths have availed themselves to the fullest extent. Professor Trousseau states that a Parisian, who determined upon making his fortune by medical charlatany, secretly selected as his panacea distilled water as the most innocuous substance which he could use for his purpose. But he received so many attestations of the great potency of his medicine, and of the wonderful cures which it had worked, that at last even he himself became convinced, and died in the belief of the universal efficacy of the waters of the Seins.

Dr. Routh tried experimentally the effects of medical faith, in some fifty or sixty cases.

"My plan (says he) was to employ three kinds of coloured water—red, yellow, and blue; to work upon the imagination of my patients, describing this water to be a deadly poison, and having it labelled accordingly, giving express caution to keep the medicine from the children. I invented a series of symptoms as likely to follow—from 20 to 30 drops a dose. It is but right to state, that in many cases the result was null, no effect appearing to have been produced. In some patients, however, chiefly neuralgic cases and weak-minded individuals,

there was. In one case this colored water produced such alarming symptoms that I was sent for in a great hurry to see my patient, a strong well-built man about twenty-five years old, labouring under some dyspeptic affection. I was informed that after every dose of the medicine taken (which consisted of 30 drops of water coloured with the compound lavender tincture), syncope, with convulsive movement, followed. A diminution to 20 drops re-assured my patient, and the fits did not occur again. My colleague, Dr. Taylor, found coloured water produces such distressing symptoms in a female, that he was obliged to omit it. If such effects were produced among *out-patients*, by whom all dietetic regimen was neglected, where rest and quiet were not enforced, what good effects might not have followed the employment of coloured water with these adjuvants?—*Fullacies of Homœopathy*, p. 24.

Sir H. Davy cured a paralytic man in a fortnight, by placing daily under his tongue the bulb of a pocket thermometer, the palsied man believing all the while that he was inhaling some infallible specific gas.

As a modern instance of the treatment of disease by medical faith, we have the statements of the French missionary Huc, related in his book of travels through Tartary and Thibet. The Lama-Physician, observes M. Huc, is his own apothecary, and prepares his specifics from vegetables only, which he either infuses in water or makes up into pills. When his stock of medicines is exhausted, he is by no means disconcerted, but writes the names of the remedies upon slips of paper which he rolls up, moistens with his saliva, and gives to his patient, who swallows them with the same confidence as if they were genuine medicines. "To swallow the name of a remedy, or the remedy itself, say the Tartars, comes precisely to the same thing."

Dr. Simpson relates the instance mentioned by Dr. Horace Green, which, being too pungent for homœopathic ears, called down upon the latter the merciless ridicule of Dr. Henderson, to which we antecedently alluded. It is as follows:—"A lady had been useless and bed-ridden for years;—the spine was her own alleged seat of disease; and endless measures had been tried to restore her to health and the power of standing and walking; but they had all been tried in vain. In consequence of the earnest advice and glowing representations of some female homœopathic friends, she had long been anxious to ascertain, if homœopathic treatment could be of any use in her distressing case; but her husband refused to give his consent, believing homœopathy to be 'a discreditable delusion and a quackery.' Fortunately, however, for herself, her husband left her for a week or two on a sporting expedition; and as soon as he set off with his gun and dogs, she seized hold of the opportunity which she had long desired, of consulting a celebrated homœopathic physician. The physician listened long;—examined into her case most attentively;—made before her

the next, etc.,—and that, before her husband returned, she would be able to walk and enjoy life. The patient diligently swallowed the globules; all the predicted effects duly followed; and when her husband returned home, he was astonished and overjoyed to find his sick and bed-ridden wife up and well. The crime of consulting a homœopathist against his declared wish was readily forgiven, seeing the results of the homœopathic treatment had been so happy and beneficial; but he asked for a sight of the wonder-working 'globules' that had produced so gladsome a change in his wife's health, and in his own prospects of domestic happiness. On being shown the globules, the acute and loving husband earnestly desired them to be most carefully preserved and locked up, lest, perchance, his partner's distressing ailments should at any future time return. He then went and informed his usual family physician of this secret, that his wife had got well under a homœopathist; adding, that still he did not believe in homœopathy itself, for he found the globules which she had swallowed were not homœopathic drugs, but specimens of some small percussion pellets that were to be sent to him for examination (his gun being one formed for the use of the percussion-pellet, in preference to the percussion-cap,) but which had not arrived at his house till some time after he had left. The servant had mistaken the packet of pellets for the packet of homœopathic globules; and the lady had swallowed them under the belief that she was swallowing the homœopathic physician's medicine."

When the homœopathist treats a case solely by his attenuated doses, he virtually trusts the cure of the patient to nature, regimen and medical faith; for no one in his senses can believe that the decillionth of a grain of any article can have any possible effect upon the human economy. Dr. Shoubert of Dramburg, who was intimately acquainted with Hahnemann and his practice, informs us that "Hahnemann himself even, would, in his hours of sincerity, probably have sanctioned the truth of this statement. It has been often questioned whether Hahnemann, like the 'prophet' Joe Smith, used the talents bestowed upon him only to dupe others, or whether he was enough of a visionary to believe in his own various incredible doctrines. The published evidence of Dr. Schubert, goes so far to prove that the founder of homœopathy was himself far more knavish than foolish. 'We hear it (remarks Dr. Schubert) continually asserted, that Hahnemann placed no confidence in the powers of nature in curing diseases; but from my intercourse with him, I am quite satisfied that no physician ever trusted more to the *vis medicatrix naturæ*. It requires, indeed, but very little reflection to enable us to perceive, that it was through the closest acquaintance with the curative powers of nature, that Hahnemann was led to adopt his new system of medicine. I have heard him declare that he looked with contempt upon medical practice, and he thought that a patient would be none the worse if left to himself. He had a thorough conviction that all curable diseases might, under proper attention to diet, be removed by the efforts of nature alone. He looked upon these as his sheet-anchor. On one occasion he said to me, 'I give medicines but very seldom, although I

always prescribe small powders. I do this for the sake of keeping up in the patient's mind the firm belief that each powder contains a particular dose of medicine. Most patients will get well by adopting a simple mode of living, and by placing a boundless confidence in their medical attendants. Ordinary practitioners know nothing of this practically, although they are always talking of the healing powers of nature.'—*Casper's Wechenschrift* for 1845. On Hahnemann's use of small powders, to "quiet the patient or his friends," but which contained sugar only, see Dudgeon's Edition of the *Organon*, p. 302.

We have examined the law, *similia similibus curantur*, and the theory of the action of infinitesimal doses, considered the curative indications of disease, and estimated the value of the so called provings, which together constitute the system of homoeopathy. It remains now to notice the cardinal and irreconcilable differences between this system and that of legitimate medicine, and apply the homoeopathic rules in practice.

The theory of disease, which is received by scientific men as satisfactory and correct, is that each disease depends upon some internal pathological change of structure or function, which produces corresponding external symptoms by which it may be recognized. The received theory of cure is a logical deduction from the theory of disease. The first indication being to remove the disturbing cause, secondly, to modify or counteract its effects, and thirdly, to restore the healthy state of the system.

This differs in no wise from the established methods of obviating or counteracting the effects of causes in other departments of science.—Legitimate medicine is one of the sisterhood of the sciences. She recognises all the laws of nature, and all the processes of observation, comparison and experiment which philosophy has sanctioned, making use of the facts which have become the property of humanity, and appropriating the results which are applicable to her own special purposes. And in order to understand the phenomena which appear in health and disease, and to perceive the relations which obtain between the human organization and external nature, the physical and biological sciences must be thoroughly mastered.

Medicine, properly so-called, has been styled inexact and defective, as if its votaries had been less philosophic than those of the other sciences. A very slight acquaintance with the nature and scope of practical medicine will convince any one, that from the very nature of things, its questions must depend, for solution, upon the state of the sciences which precede it in the natural order of evolution.

All the phenomena of life are more complex than those of chemical and physical science, hence less easily reduced to simple laws, and, though the methods of investigation are correspondingly multiplied, the definite determination of any question is necessarily rendered more difficult and uncertain.

Homoeopathy discards all that has been previously acquired, and begins from a new starting point, as though all human discoveries had heretofore been nugatory and false. The maxims and laws to

which we have adverted, as constituting homœopathy, may be compressed into a pamphlet, and with this and a portable box, an individual may practice the art without any previous preparation or study.

As we elsewhere stated, Hahnemann rejects the theory that internal or structural changes constitute disease, and maintains that disease consists in the totality of the symptoms only, and depends on an internal alteration of the vital force, which he calls a "spiritual change" that can only be altered by the spiritual or dynamic force of medicines.

Now a disease may consist in mere functional derangement, without any structural change, yet in the great majority of cases, modification or perversion of function exists in connection with, or depends upon changes in the constitution of the part, as post mortem examinations abundantly attest.

Without dwelling on those changes of structure which are displayed on the visible parts of the body, as in ophthalmia, &c., which are too palpable to be denied by the most inveterate homœopathist, we may instance those changes of structure in the internal organs, which we can by physical signs, by exploration, by the eye, the ear, the hand, detect with almost as much precision as if they were exposed to our view, as in pulmonary and cardiac diseases. In pneumonia we can now trace with accuracy the successive changes of structure, and we detect organic derangements of the heart with the same certainty, which enables us to adapt our remedies to counteract the morbid tendencies of the disease.

Homœopathists ignore these facts and refuse the evidence which a post mortem examination would reveal. The crepitating rale and the dull sound on percussion are more characteristic signs of pneumonia than the cough, pain, or hurried breathing, yet homœopathists deny the existence of the former, and administer a remedy whose artificial symptoms being similar, are supposed capable of removing the latter, hence curing the former.

To say that there is no need of noticing the internal changes, because the removal of the outward symptoms will cause the internal changes to disappear, is contrary to common sense, and all we observe in the progress and course of disease. The homœopathist reverses the natural order of things, and holds that in disease, to remove the effect, the cause will cease. This principle if true in medicine, must be true in ordinary life; yet homœopathists are allowed to go at large, as if they were in possession of their mental faculties, when no sane mind can admit the

Relative Treatment of Pleurisy by the Legitimate Physician and by the Homœopathist.

Acute pleurisy may be defined nosologically, as homœopathists would regard it, or pathologically as to its real nature.

NOSOLOGICAL DEFINITION OF PLEURISY.—"Acute pain in the chest, aggravated by inspiration, commencing with chills or rigors, followed by increased heat, a hard and accelerated pulse; short, dry cough; and by difficult, short, or disordered respiration."

PATHOLOGICAL DEFINITION OF PLEURISY.—"Inflammation commencing in, or implicating one or more parts of the pleural expansions, attended by either more or less of a consistent albuminous exudation, false membrane, or adhesion, and by a fluid effusion, varying in their characters with the varying states of different cases; causing pain, symptomatic fever, disordered respiration, dulness on percussion, and alteration of the respiratory sounds."*

In the treatment of pleurisy, the rational practitioner aims at the reduction of the internal inflammation going on in the pleura, which really constitutes the disease, and the absorption of the inflammatory secretions, if such have occurred. When the morbid action is arrested, the external symptoms vanish, and the effects or results likewise disappear. The therapeutic means at his command, are so efficient, that death, "observes Dr. Walshe, is rare a result of the disease, that I have neither myself (and I have carefully attended to the point since my attention was first drawn to it, years ago, by M. Louis) lost a patient from pure primary idiopathic pleurisy, with or without effusion, nor know of an occurrence of the kind in the practice of others." The homœopathist on the other hand, gives his patient some remedy which he thinks capable of stimulating the external (nosological) symptoms. In the last edition of his organon, Hahnemann, for instance, deliberately states, that "by a small globule of the decillionth dilution of aconite juice, with abstinence from vegetable acids, the most violent pleuritic fever, with all its alarming concomitants, is changed into health, and cured in a few, at most twenty-four hours."

Relative Treatment of Tapeworm by Legitimate Physicians and Homœopathists.

The presence of Tapeworm in the alimentary canal, is generally accompanied by the following symptoms:

"Dull pain in the forehead; giddiness; buzzing in the ears; dulness of the eyes, which are rounded by a dark circle; œdematous eyelids: dilated pupils: frequent and anæmic movements of the eyes:

mornings; itching at the nose, anus and vulva; grinding of the teeth, especially during sleep; constriction of the throat; swelling of the belly; gurgling, shooting pains, and a sense of pinching about the umbilicus; a feeling in the morning as of a foreign body moving in the bowels; amelioration of all the symptoms under the use of farinaceous food, hot bread, and coffee; finally, depression of spirits, and a train of nervous derangements in protracted cases."*

The homœopathic principle, that the removal of the totality of the external symptoms by a medicine capable of stimulating these symptoms, fails here to remove the offending cause, even allowing that these external symptoms can be removed. Their law, *similia similibus curantur*, is nugatory here, and no homœopathist has been bold enough to affirm, that any medicine they possess can produce tapeworm. The legitimate physician, taking a rational view of the disease, and knowing the real cause to be an internal pathological state, and that the external symptoms are merely the remote effects of this, administers a medicine which experience has proved capable of removing the disturbing cause, (killing the worm and expelling it from the system,) and obliterating thereby the external symptoms by which the disease manifested itself. A vermicide as the male-shiel-fean, the Kosso, the pomgranate has been found efficient in fulfilling all these objects, but they produce no artificial symptoms when administered in health at all corresponding with those of tapeworm. "Neither," observes Dr. Pereira, "botanical characters, sensible qualities, nor chemical composition, would have induced us to suspect that Kosso possesses the valuable anthelmintic properties which experience has shown that it does. Our confidence in the anthelmintic properties of Kosso rests on experience only; and the experience on this point is very strong. All modern travellers in Abyssinia, are agreed on the great success of the remedy on the natives of that country; and the experience of physicians in France, England, Germany and Switzerland, confirms the favorable report made by those who have seen the Kosso used in its native country."

Relative Treatment of Polypus, by Legitimate Physicians and Homœopathists.

"The adherents of the old school," says Hahnemann, "believe that by putting a ligature on polypi, etc.,† . . . they have removed the affection, conquered the disease, and conducted a rational treatment towards the cause. But what is the result? The new forms of disease that sooner or later, but inevitably, appear, caused by this mode of treatment, (but which they pretend are entirely new diseases,) which are always worse than the original affections, sufficiently prove the error of their notions, and might and should open their eyes to the deeper-

* Professor Wood's Practice of Medicine, vol. i. p. 629.

† Hahnemann, in the paragraph cited, enumerates a long list of other diseases (*see Organon*, p. 14.) but for brevity's sake, we take as illustration the first on the list, viz., polypus.

seated immaterial nature of the disease and its dynamic (spiritual) origin, which can only be removed by dynamic means."—*Organon*, p. 14.

Taking polypus of the uterus, as an example, we find it accompanied by hemorrhage, which induces various symptoms, which are more than mere spiritual changes, and affect the system very seriously. Moreover, the mechanical symptoms palpably reveal themselves to the senses, not only of the patient, but of her attendants also. Hahnemann's arrogance is here conspicuous—admitting that the polypus may be removed by mechanical means, he, as related above, denies the efficacy of the orthodox method which the experience of thousands attests as successful. "If a polypus, (as the late Dr. Gooch truly observes,) be detected and removed, the person not only lives, but regains perfect health; if it be mistaken and neglected, it occasions the death of the patient."

Relative Treatment of Itch by Legitimate Physicians and Homœopaths.

In no disease is the treatment of homœopaths and regular physicians more dissimilar, and the results more conspicuous than in this.

"Even a primary itch eruption, of recent origin," says Hahnemann, "though it may have spread all over the body, may be perfectly cured, in persons that are not too weakly, by a dose of tincture of sulphur, thirtieth dilution, given every seven days, and in the course of from ten to twelve weeks, (accordingly with ten or twelve such globules,) so that it will seldom be necessary to aid the cure with a few doses of *carbo vegetabilis*, (vegetable charcoal) thirtieth dilution, also given at the rate of one dose in the week, without the slightest external treatment, besides frequent change of linen, and good regimen."—(*Organon* p. 292.)

It is now established beyond the shadow of a doubt, and the fact is admitted by homœopaths, that the disease called itch, consists primarily in the presence of a small insect (*Acarus Scabiei*) embedded in the skin, whose removal puts an immediate stop to the disease. The means for accomplishing this, have been ascertained by most careful experiment; and Itch is now cured in the Parisian Hospitals, in two or three hours, by the application of iodide of sulphur and potassium.

"As sulphur," observes Dr. Neligan, "never fails to cure the disease, it is the universally admitted remedy for it; none other, indeed, would require to be alluded to, were it not that the unpleasant odor of sulphur renders its employment in some instances inadmissible. The general method of using this substance for the treatment of scabies is by the localunction of ointments containing it, either alone or compounded with alkalies; in the latter form it is generally employed in the present day in France, and found to be most efficacious there when used alone: the combination, first introduced by M. Helmerich, is composed of two parts of sulphur, one of carbonate of potash, and eight of lard. The surface of the entire body, but more particularly of the affected parts, should be first washed well with a strong solution of soft soap, the pa-

tient then placed for a quarter of an hour in an alkaline bath, containing a pound of the carbonate of potash, to twenty gallons of water, at the temperature of 92°, the skin well dried, and this sulphuro-alkaline ointment afterwards thoroughly rubbed in: the disease may thus be effectually cured in two or three days, a single friction, preceded by the alkaline and saponaceous bath, being used daily. Out of upwards of 700 persons, treated on a plan nearly similar to this at the Hospital St. Louis, in Paris, M. Bazin only met with six unsuccessful cases, all the rest being cured on the third day.* M. Hardt, who succeeded M. Bazin in the charge of the itch wards in this hospital, has introduced a plan of using the sulphuro-alkaline ointment there, by which the period required for a complete cure is reduced to *two hours*. His method is as follows:—"On the admission of the patient, the whole surface of the body is rubbed for half an hour with soft soap, *savon noir*; he is then placed in a bath for an hour, and the body well rubbed while in it, and at the end of that time, general friction made, and continued for half an hour, with Helmerich's ointment. 'The acarus,' add M. Hardt, 'is thus killed and the patient consequently cured.' Of 400 persons thus treated, it is said that four only required to return for further advice, and of these, two had recontracted the disease."—*Treatise on Diseases of the Skin*, p. 520.

We might include lead-colic as another example, and inquire if a little more lead will cure the disease? If Homœopaths be consistent, that which produces the symptoms resembling most closely the disease, will cure those symptoms, consequently lead will cure lead disease.

The boast of Hahnemann, that "homœopathy is the true and only way whereby disease can be most surely, rapidly and permanently cured by art," is found on examination of his own mentioning to be vain; and if his own definition of a perfect cure be taken as a criterion, Allopathy is infinitely superior to Homœopathy.

In legitimate medicine and surgery, there are many, more or less, accurately ascertained and established, general laws, the practical application of which, constitutes the art of the respective sciences. These laws are of every grade of generality. As examples of them, we have certain contagious diseases which impart an immunity to individuals who have been once affected by them; and the practical application of this law in inoculation and vaccination whereby millions of lives have been saved.

As a general law, cinchona arrests intermittent fever, opium, irritative diarrhoea; iron cures chlorosis, iodine, goitre and glandular swellings, lemon-juice, scurvy; sulphur destroys the itch-insect, upon which the disease depends; and mercury corrects hepatic derangements. Should the effects not always follow the exhibition of the remedy, such deviation from the general rule does not affect our faith in its potency; for, as we know that there are many causes continually acting in the animal economy, there must necessarily be a complication of effects, and sometimes one cause will counteract the effect of another, according to existing circumstances.

* *Journal de Médecine et de Chirurgie Pratiques*, Dec. 1831, p. 520.

The laws laid down by Hahnemann, upon which he based his system, are regarded by him and his followers as *universal and infallible*; but we have shown them to fail in divers instances, and to be of only a low type of generality. The law, *similia similibus curantur*, has been found on examination, to hold in appearance only. The prevention of small-pox by vaccination, the strongest example which can be selected in favor of the homœopathic law, is explicable, by the law *aequalia aequalibus curantur*, the same is cured by the same; for almost all pathologists are now of opinion, that small-pox and cow-pox are identically the same disease, with the difference that the cow-pox has been modified by passing through the body of the cow.

Another example to which they frequently advert, is the restoration of frost-bitten limbs by cold applications, as frozen sour-cROUT or snow. In this instance, the good effect derived from frictions of snow, is not explicable on the principle that "like cures like," but on other acknowledged therapeutic principles. There being but one kind of cold, the application of snow would be the application, not of a like remedy, but of an agent identically the same as the morbid cause. The requisition of the homœopathic law, that the like medicinal agent must be stronger in degree than the original disease, is also violated by the application of snow-frictions; for the members of the human body are not frost-bitten until the temperature in them is reduced twenty or thirty degrees below the freezing point; and snow held in the human hand rapidly acquires a heat of 32°, at which it melts. Hence, the medicinal cold, instead of being greater than the original cold of the frost-bitten part, is in reality much less. The injury which results to those parts of the human body which have been exposed to extreme degrees of heat or cold, is owing to the rapidity with which they are allowed to return to their natural temperature. When snow-frictions are employed, the temperature is slowly reduced, and if the patient be removed gradually to a warmer atmosphere, the original parts are restored to their normal condition. In this case the application of snow-friction is not in reality the application of cold, but of comparative heat to the frozen part, and is referable to another law than that of *similia similibus*.

In favor of their pretended universal laws, Hahnemann and his followers have appealed to another variety of evidence. They contend that we may obtain incontrovertible proofs, by studying the effects upon healthy individuals, of remedies which cure certain diseases in the sick. We have neither space nor inclination to follow this inquiry in relation to many diseases and their remedies. We will only instance the remedies to which we have already adverted, and inquire whether opium produces irritative diarrhoea, iron, chlorosis, iodine, glandular swellings, lemon-juice, scurvy, or sulphur, itch?

It is the opinion of Dr. Hahnemann, that lemon-juice and iodine will

of their age have solemnly believed, for example, in the doctrines of augury, of astrology, of alchemy, of sorcery, &c. &c. "Man" as has been remarked by Southey, "is a dupable animal; and quacks in medicine, in religion, and in politics know this, and live upon the knowledge of it." The founder of homœopathy, contrary to the statements of his disciples, worked successfully upon this principle of human nature. At the advanced age of eighty he married Mademoiselle Melanie d'Hervilly, a French lady who visited Coethen in order to consult him, and removed with her to Paris. She procured for him, from M. Guizot, the permission to practice in Paris; and in that gay capital the sage of Coethen altered his manner of living to suit the tastes of his new customers.

The following from the pen of an American lady, who visited Hahnemann professionally in Paris, may convey some idea of the man and the interest he manifested for the practice of his art :

"It was scarcely nine o'clock when I entered Hahnemann's magnificent mansion; but his saloons were already crowded, and one o'clock struck before I gained an audience. A valet, in gaudy livery, who had taken my card some four hours before, then approached, and informed me that I would now be received into the consultation chamber.

"I followed him through a succession of apartments, all richly furnished and embellished with numerous busts of Hahnemann, of various sizes. A door was thrown open, and I entered the consultation room.

"At the end of a long table sat a lady, dressed in the most *recherche* demi-toilette, with a gold pen in her hand, and piles of books and papers strewn around her. * * * She rose to receive me, and it was not until she resumed her seat that a shrivelled, little old man became visible. He was reclining in a sumptuous arm chair, with a black velvet skullcap on his head, and in his mouth a richly enamelled pipe, that reached almost to his knee. His face reminded me of a ruddy apple that had been withered by the frost; but the small dark eyes, deeply set in his head, could scarcely have glittered with more brilliancy in his lusty youth. As I took the seat which Mrs. Hahnemann designated, he noticed me with a *look* rather than a bow, and removing the pipe from his mouth, deliberately sent a volume of smoke across the table—probably in token of greeting.

"Mrs. Hahnemann addressed me, and wrote down my answers to her numerous questions. Hahnemann sat puffing away as though his existence depended upon the amount of smoke with which he was surrounded, and apparently intent alone upon his pleasant occupation. But when I spoke of our long visit to Germany, he suddenly took the pipe from his mouth. 'Sprechen Sie Deutsche!' were the first words he addressed to me.

"I had only to utter 'ya wohl,' when a species of promethean fire seemed to shoot through the veins of the smoking automaton; he laid down his pipe and commenced an animated conversation in his own language.

"As soon as politeness permitted, I led back the subject to the point from which we had originally started. At the first medical question,

the pipe returned to its former position, the expanded countenance shrivelled up again, the distended muscles relaxed, the erect form sank back into a withered heap, and was quickly enveloped in smoke."

Before dismissing our subject, we will mention some of the means by which homoeopaths have endeavored to propagate their doctrines. The writers in the British Journal of Homoeopathy would have "Homoeopathy regarded not as a science merely, but as a sublime devotion, a *form of religion*, a rainbow of divine union, holding out to mankind the promise of speedy regeneration." They say that this regeneration was accomplished by an expiatory sacrifice, and was made in the person of Prince Alphonso, heir apparent to the imperial throne of Brasil, who died a few days after his birth, from being given to a consumptive nurse, and for want, as the authors allege, of proper homoeopathic advice. "It seems," continues the same writer, "that it is only by excess of evil that man can return to good. In order that humanity should renounce the worship of false gods, nothing less than a DEICIDE was necessary. It is by a DEICIDE that Allopathy behooved to mark its last hour."

Such folly and absurdity abound in the same Journal and in the writings of many British and Continental homoeopaths.

During the past year, a sermon was preached, in London, by a clergyman of the Church of England, the Rev. Thomas R. Everest, in aid of the Hahnemann Hospital, and was subsequently published.

"The sermon is replete with Hahnemannian theology, as well as Hahnemannian medicine. The reverend author who boasts of "some years of intimacy" with Hahnemann, conceives it was not likely that "the sacred pages of the Word of God should be *quite silent* on the subject" of the discoveries which his friend imagined he had made some nineteen centuries afterwards. And accordingly he finds they are not "silent." One of Hahnemann's supposed highest and greatest generalizations in pathology, was, the dogma or doctrine that about seven-eighths of all the chronic diseases of the men and women of this and other ages, were the result of the unobserved and unknown presence of *Psora*, or (to use its plain English synonym) Itch, and were to be cured by the remedies capable of curing Itch. But, argues the Rev. Rector of Wickwar, our Saviour actually meant to cure the Itch (that fertile source of human disease and suffering,) when he issues "the solemn command to his disciples," "*cleanse the lepers*," (Matt. x, 7, 8;) the leprosy of the New Testament, and the *psora* or itch of Hahnemann being, according to Mr. Everest's own grand discovery, quite identical. Of course, according to this new Hahnemannian theology, all our learned theologians have hitherto been in error in supposing, that the verse in which this phrase occurs, ("And as ye go, preach, saying, The kingdom of heaven is at hand. Heal the sick, *cleanse the lepers*, raise the dead, cast out devils.") included a warrant of miraculous gifts to the first disciples, for the purpose of testifying to men the truth of the great mission intrusted to them."—*Tenets* p. 38.

"Only think of Dr. Wiseman, or Dr. Hugh M'Neile, Dr. Candlish, or Mr. Drummond, attempting to render their spiritual ministrations more

efficacious to their flocks by asking them, as Mr. Everest proposes, to get themselves properly drugged and prepared for the reception of the gospel truth by due doses of anti-psoric globules."—*Tenets*, p. 42.

"It has been occasionally argued, that there must be some truth in homœopathy, because, in our own country, some few men of talent, not conversant with medicine,* or connected with the medical profession, have, from time to time, swallowed Hahnemannic doses, or professed an adherence to the creed and doctrines of homœopathy. But, alas! hy-men of the highest intellect have, in the history of the world, very often formed, for the time, the highest abettors of almost every kind of transitory medical charlatanism. No child of the present day believes in the curative influence of the sympatheticæ applied,—not to the wound—but to the weapon which made the wound. Lord Bacon, himself, however, "the wisest of men," confessed that he did not entirely discredit the efficacy of this sympathetic weapon-salve; or the healing powers of charms and amulets; and Roger Bacon was a believer in a Universal Elixir of life. The Honourable Robert Boyle, one of the most distinguished philosophers and writers of his day, and a man who was deservedly elected President of the Royal Society, supposed that he cured himself of an ague by wearing a medicated bracelet around his wrist: he, "Cudworth, the philosopher, and many other eminent men of unquestionable competency," testified to the success of the supposed effects and cures produced by the exorcisms and manipulations of Valentine Greatrakes; and the Rev. J. Flamstead, the first Astronomer-Royal, made a voyage to Ireland, to be cured by the touch of the hand of this exorcist. Dr. Hartley, the celebrated mathematician, published an octavo volume in favour of Mrs. Stephen's alleged specific for gravel and stone, with 150 cases in proof of its alleged efficacy, his own case being among the number; but he himself died of the very disease for which he had vaunted the discovery of an infallible cure, and of which he alleged himself to be cured."—*Tenets*, p. 48.

We cannot, perhaps, better conclude this long and somewhat tedious discussion, than by quoting the following from Dr. Simpson's book which already done us so much good service.

"Mankind seems capable of thinking and reasoning more accurately upon almost every other kind of practical art and science than they do in regard to medicine. Neither lawyers themselves, nor the public, would listen, for instance, for a moment to any astounding quackery in law. Suppose any law-reformer should undertake to prove that a pay-

* Dr. Whately, in the Preface to his work on Logic, remarks :—"A sailor will perhaps despise the pretensions of medical men, and prefer treating a disease by common sense; but he would ridicule the proposal of navigating a ship by common sense without regard to the maxims of nautical art." Dr. Whately's remark, so

ment by an "infinitesimal" mustard seed or straw was quite equal in its legal amount and potency to a round sum in gold, would he not be rated not merely as illogical, but as actually insane? The same character, I believe, would be accorded to any quack-agriculturist who should rise up at the present time and declare, that during the past thousands of years the true science of agriculture has remained totally mistaken and undiscovered, and that *one grain* of guano, or of phosphate of ammonia, or of other manures, if properly diluted and shaken in an ocean of fluid as large as the Baltic or Mediterranean seas, would form a mixture of 'terrific potency' (to borrow the language of the homoeopaths) for fertilising the soils, not only of a single farm, but of whole countries and kingdoms. Or what character would the world attribute to any politician, who should gravely propound that our paupers and others need not be longer a heavy pecuniary burden upon the poor-rates or the state, as he had discovered a cheap and 'infallible' law or method of permanently supporting each and all of them upon—an infusion of a few grains of curry-powder?

"In relation to such subjects, in the practical arts and sciences of law, agriculture, politics, etc., the public mind readily enough perceives the exquisitely ridiculous character of any such charlatanic phantasies and delusions. But all past history and all modern history alike show that, in relation to medicine as a practical art and science, the matter stands widely different. For every few years, some reformer or another has sprung up (and presuming upon the ignorance of the public in relation to the complex vital laws of health and disease,) has roundly declared that the true nature and true treatment of human disease were unknown till his time, and that it has been left to him and to his sagacity to discover at last a new infallible cure, or a new infallible system of cure, for the various ailments of mankind. And all past as well as all present experience further shows, that the greater the degree of dogmatism and effrontery, with which any such medical pretender proclaims his doctrine, and the greater also the abuse which he bestows upon the medical profession at large, the greater, in all probability, will be his success in gaining patients and patrons amongst the credulous public. Nay, sometimes, apparently the more there is of irrationality in any new or revived charlatanic medical doctrine or practice, the more converts it is likely to gain; provided only its proposer and his credulous patrons show no scruples of conscience in suggesting and promulgating imaginary cures. The designing pretender of the new doctrine may further be a person totally unacquainted with physiology, or pathology, or medical science in any way, and yet all past experience shows that this is

as I have already remarked, a similar species of charlatantry were attempted in relation to most other arts and sciences, the delusion would be at once detected, and the imposture duly announced, whilst, in medicine, the delusion would, on the other hand, probably make the propounder's fame and fortune, and in the course of years be forgotten.

"Have not (says Dr. Marshall Hall) all systems had their brief day, and then failed hitherto?" "What," he asks, "should we think of an ignorant peasant turning quack engineer? I do indeed (he remarks) remember to have seen persons, when their watch would not go, give it a good shake; and once or twice this coarse mode of watch therapeutics may have succeeded in removing some unknown obstacle to its movements, and the little machine may have resumed its horological career. Such a lucky accident in experiments on the human frame and its diseases, has made the fortune of a quack in medicine. Yet would any one in his senses deliberately deliver over his watch to the issue of such an experiment? Would not the shake be more likely to move the displaced pivot still further from its proper position in the many wrong directions, than to replace it in the one right one? Yes to such experiments—to such violent and ignorant experiments—many intrust the well-being of the most complicated and delicate of all machines, that 'harp of a thousand strings,' the human frame!"*

"The curious amount of true folly often betrayed by the credulous public, in thus patronising new and pretended universal methods of cure can only fully be appreciated by physicians who are acquainted with the actual nature of the diseases which are to be cured; or by others, who, if not physicians, may for a moment give themselves the trouble of considering and mastering the pretensions of the new empiricism, whatever it may be. I have quoted in a preceding page (p. 91,) from Sir Benjamin Brodie, an account of a London empiric, who, some years ago, rose into rapid practice in consequence of alleging that he had made a great discovery in the healing art, viz., that mere dry rubbing or shampooing, in his particular way, was a remedy for all disorders. No doubt the simple rubbing or shampooing of the surface, such as this quack practised, is one useful means of treatment; and has been generally recognised by the profession in all ages as such in some particular case; as where there is stiffness of the apparatus of motion from disuse, and from the slightest forms of functional disease and injury. No doubt, also, the whole class of "*malades imaginaires*," and hypochondriac artists, lawyers, old ladies, and others, suffering from the mere effects of sedentary habits and want of sufficient physical exercise, may have their health improved by being forced once or oftener for an hour or so daily (under the name of shampooing) to exert muscles and parts to which they would not otherwise give due healthy exercise, and

on every thing whatever, however elementary, relating to the structure, functions and diseases of the body. Yet by that time, Long's rubbings, and liniments had, it was said, gained for him £100,000 out of the pockets of the credulous public of London, and noblemen, clergymen, generals, etc., stood forward at his two trials to attest and swear to his medical knowledge.

* Practical Observations and Suggestions in Medicine, p. 9.

to have the surface of their skin cleansed and excited by it. But to imagine, as this London empiric and his patrons alleged, that rubbing the skin would cure the various internal diseases of the abdomen, chest, eyes, ears, spinal cord, etc., for which he applied his frictions and pinchings, is an absurdity of which even an unprejudiced child might see the true character. The practice was applied, for instance, (to take one example only,) in cases in which one or other side of the body was entirely palsied, in consequence of previous attacks of hemorrhagic apoplexy. Such an apoplectic attack produces, as is well known, that palsy of the side which follows it, by injury of the opposite hemisphere of the brain, from an effusion or effusions of clots of blood into the substance, or upon the surface of the brain. To dream and fancy, that in such a case, the disease could really be removed by merely rubbing and shampooing the skin, is to suppose, that this rubbing or shampooing of the skin could remove a clot of blood situated in the interior of the head, or mend the local injuries of the brain left by that clot. The watchmaker might as well expect to mend the broken wheel or mainspring of a watch by rubbing the surface of the glass face or gold back of the watch, as a rational man could expect to rectify the lesions and injuries producing a hemiplegic side, by rubbing the patient's surface. And certainly a quack engineer might as sanely imagine, that he could rectify the diversified derangements and injuries that are apt to take place in the interior of a locomotive engine, by merely rubbing and barnishing the surface of the shell or casement of the locomotive, as a medical practitioner could expect to cure the lesions of the interior of the human body by rubbing the surface of the body.*

"One other strange circumstance seems to hold true of *all* types and forms of medical quackery, viz.,—that the victims and patrons of it are generally ready to assert, in the most uncompromising terms, and even to swear to, all kinds of improbabilities and impossibilities in regard to its effects. Thus (as I have already quoted from Dr. Curie, at p. 49,) among the numerous witnesses, Nobles, Generals, etc., brought forward by Mr. St. John Long, at his trial for manslaughter, in consequence of his applications and rubbing, Lord Ingestre swore, "that he saw St. John Long draw several pints of a liquid like mercury from a patient's brain," viz., the brain, I believe, of the late Sir F———s B———t. And I have at the same time (p. 48,) cited Hartley, the metaphysician, as gravely and seriously publishing to the world an account of his own

* The case of the rubbing empiric, described by Sir Benjamin, illustrates another curious general fact in the history of such practices—namely, the unbounded sums that the public will freely and ardently pay to such medical pretenders, while, at the same time, they often grudge the educated medical practitioner his comparatively infinitesimal fees. In the notice of this London rubber, it is stated, for instance, that—"A young lady, whose lower limbs had been paralytic from infancy, was brought to him from the country to be cured. At the end of a year, £500 having been expended in the experiment, she returned home in the same state as when she

cure, and of the supposed cure of 150 others, by Mrs. Steven's absurd specific, the fact being—that he himself, and no doubt most of the other 150, died of the very disease of which he gravely and publicly avowed himself and them to be thus cured. No doubt, also, many of these 150, like Dr. Hartley, cozened themselves, and tried to cozen others into the belief, that they were cured of their maladies, when such was not in any degree the fact. "For there is," observes a late medical writer, "a strong inclination in mankind generally to assist in propagating in imposition to which they have themselves yielded; both because it makes them objects of interest to the public, and because it appears to justify them in having given it their confidence."* They are, in consequence, often indiscreetly, and almost unwittingly, led to asseverate as true, about such new practices, things which have no actual foundation in sober truth. We have already quoted Dr. Moore as correctly observing, in relation to this very curious trait in human nature, that "Clergymen, Judges, and Peers are daily *swearing* that they have been cured of incurable diseases; but the meanest apothecary smiles with contempt when he reads their splendid testimonials. . . . Yet, if any one should attempt, by investigating each particular case, to refute it, he would soon discover the impossibility of succeeding. I have been requested (he adds,) on various occasions, to make such researches, and generally found that the patients were completely convinced of the truth of the attestation, whether the remedy was physic, magnetism, or a tractor. In short, they were ready to take *the most solemn oath* to what was quite impossible to be true!" (See antecedently, p. 93.)

"Almost all pretended new systems of medicine and cure agree in one other respect. Whatever may be the original doctrine which has caught the imagination of the new reformer, and has been made by him the basis of his system, this original fundamental doctrine, always, sometimes comes to be supported, enveloped in, and,—as it is sometimes termed,—“proved,” by other additional doctrines. The original primary fancy, or supposed primary fact, requires to be duly clothed in other secondary fancies and “facts” of the same kind, in order that the whole may come out under the appearance of a consistent creed. Even the poor rubber, for example, has pretended synovial deposits to rub down in parts where no synovia ever existed; he has to clear away synovial swellings around the trunks of nerves going to weak limbs, and finds, or pretends to find, on the bodies of his patients these swellings in localities where every tyro in anatomy knows there are no nervous trunks; he sets himself deliberately to rub down the natural projections and tuberosities on the bones, as if any amount of rubbing could enable him to change the necessary forms which Nature has given to the skeleton, etc.; he declares indigestions and cancers, cataracts and amauroses, congenitally deformed or idiotic brains, and sprained thumbs, to be all equally cases of displaced or deranged synovia, and to be affections all equally curable by external manipulations; and he cozens his victims into the belief, that the normal swellings of bones, glands, etc., are abnormal and diseased, and can and must be discussed by pinching and

* Medical Gazette for 20th January, 1843.

shamponing the surface,—the whole constituting a medical doctrine that any rational man unacquainted with the history of past medical quackeries, would, *à priori*, suppose to form an imposture, which the intellect even of a South Sea savage could easily enough see through and despise; but yet all past medical experience amply and humiliatingly shows that even such a crude medical creed, and even such a gross medical deception as this, can always for a time get patrons and enthusiastic promulgators among those educated but weak-minded patrons of quackery in civilized England, who are ever ready to adopt every new extravagance in medicine (and sometimes in religion too,) and ever ready also to asseverate and swear to all kinds of untrue and impossible cures in attestation of that which, at the time, happens to be their own latest form of medical credulity. In respect of subordinate creeds and subsidiary hypotheses, the doctrine and system of homœopathy is but a modern reception of the history of a hundred other infallible doctrines and systems of cure which have been propounded and disappeared before it. And, like several of them, it is as extravagant in some of these its minor doctrines and details, as it is in its primary foundation.”

BIBLIOGRAPHICAL NOTICES.

ART. XII.—*Lectures on the Diseases of Infancy and Childhood.* By CHARLES WEST, M.D., Physician to the Hospital for Sick Children, &c. &c. Second American, from the second enlarged London Edition. Philadelphia: Blanchard & Le. 1854. (Through Mr. John Russell.)

THESE lectures, popular at the time of their delivery and first publication, have become more worthy of the perusal of the Medical public, as they have been carefully revised: many formulæ have been introduced, and a minute alphabetical index has been appended. The work now contains the results of 640 observations, and 199 post-mortem examinations, chiefly made among 16,276 children who came under the authors notice during the ten years of his connection with the Children's Infirmary in Lambeth.

ART. XIII.—*Remarks on the Efficiency of Quarantine as a means of prevention to the spread of Epidemic Diseases in cities and other populous communities ; being the Anniversary Oration before the South Carolina Medical Association for 1854.* By WILLIAM T. WRAGG, M.D. Charleston : Walker & James.

THE recent discussion in our daily journals of the subject of quarantine and the important interests at stake in its decision, make this well written and able paper of Dr. Wragg's worthy of a more extended notice at our hands. The question of the necessity or not of quarantine is no trifling one, for upon its decision depends the most active measures which a community can take, even extending to the passage of laws *ex post facto* in their character, or in another way the leaving trade to take its own channels unrestricted and unregarded by the authorities. The proper determination of the question depends upon much personal observation, upon a keen and comprehensive view of a mass of facts and a collection and comparison of minute details and circumstances which require the closest sifting and the most impartial judgment. The medical knowledge necessary to the investigation must also be derived from varied sources.

From a review of these considerations, we see that every citizen and not simply those that live in cities, is interested in its just determination.

Dr. Wragg's views, as expressed in this pamphlet, are distinguished for their moderation, and exhibit an unprejudiced and candid statement of the opinions which his judgment sanctions. He believes in the efficiency of both quarantine and sanatory regulations, the one to keep off from us as far as is in our power, the seeds of the disease, and the other to render our ports a *nidus* less fit for its reception, growth and dissemination, and the two may well go hand in hand. Let us quote one or two paragraphs :

"These measures are just such as I have been considering. The judicious combination of a vigorous sanatory police, and a sagaciously devised Quarantine system, can alone aid us. Without them we are powerless to ensure ourselves from the consequences of foreign poverty and negligence, while with them we may defy the approach of the scourge.

"The last of the classes of diseases to which I have alluded, as arising under an epidemic form, and thus producing such distress in a community as demands of the public authorities the institution of a system of precautionary measures, is that in which the exciting cause is due to a combination of circumstances in which there can be no well grounded suspicion that the exuvire of human bodies, either in health or disease, is an essential element. For the exclusion of these the action of the sanatory police above is called for. To make this action efficient the whole subject of sanatory improvement must be included. The drainage and leveling of the surface, the improvement of the soil, the construction of the habitations, especially in view of ventilation, warming, and the means of getting rid of filth, refuse, &c., an abundant water supply, including the command of warm and cold water for washing,

bathing, &c., good food well cooked, sufficient clothing, ample room, so as to avoid crowding—these are a few of the items that go to form the sum of this important system of measures.

Now, if asked the question which of the two systems is most conducive to the public health, I should be disposed to avoid a direct reply, and say that one without the other can only be of partial effect. For some diseases Quarantine is indispensable, for others, sanitary measures, and for a third set, neither one of them without the other, can be of any avail. But for the efficient action of each—for their beneficial effect to be enjoyed in perfection, they must be combined. I do not hesitate to say, that, in my opinion, Quarantine laws, of the strictest kind, enforced with all the power of the most vigilant police, will, at times, be utterly powerless, without such a state of the public health as can alone be ensured by the most rigid enforcement of sanitary measures.

“Situated as we are, these last are totally indispensable. With the views I entertain concerning the nature of that class of disease among which Yellow Fever is found, if sanitary measures could be made so perfect in their conception and execution, as to obviate the tendencies of our soil, climate, and other local peculiarities, to the origination of the disease, I do not hesitate to say that Quarantine might be dispensed with. But since such can never be the case—since we can never hope to see the natural tendencies of our *entourage* so thoroughly overcome by artificial means, the only resource we have for accomplishing our end, is the adoption of both measures and their vigorous and efficient prosecution.

Let us, therefore, take them both together, and, by their means, work out for ourselves the important end. Let us banish these epidemic diseases from among us. Let us elevate the standard of health, and it will follow, as an inevitable consequence, that we will not only increase the average duration of life and decrease the rate of mortality, but we will increase our population and wealth. And in the train of wealth will follow general improvement in every department of life; increase in the comforts of living—better education—higher tone of morality—fewer instances of pauperism, and, in short, an upward tendency in all things.”

“There can be no reasonable doubt that the difficulties in the way of enforcing Quarantine, renders it idle to rely upon that measure alone for protection against most of the diseases liable to take the epidemic form. But it is not less certain that something may be done by well devised and honestly executed laws for that purpose. Will it be prudent, then, to omit anything likely to contribute to the desired end? Let the merchant himself answer the question. But, if he does not, let the Government

is stopped, and the consequences of which are permanent injury to trade?

"These views, however, cannot be elaborated in a short space. They are only touched upon to show that our situation does not so far correspond with that of European countries, as to admit of our applying their facts and arguments to ourselves without some reserve. I would not be understood as advocating Quarantine beyond a certain point. Not as in itself of very great importance in Yellow Fever, but as part of a sanatory system. Yet I would not be willingly classed with those who, in order to bolster up a theory of the nature of certain diseases, would condemn any rational and prudent measure that might in any way conduce to the great end.

"Sanatory measures *first*; Quarantine *next*; and in time of disease, or when panic is taking hold of the minds of the people, *both together*.

"Much stress has been laid on the introduction of Yellow Fever into New Orleans and Philadelphia last summer by the contagionists. Dr. Fenner's history of that epidemic can hardly be said to prove this, and it is now fully established that at least two cases of that disease had occurred in Philadelphia previous to the arrival of the bark Mandarin. Had New Orleans and Philadelphia been in a cleanly state, we may at least doubt whether such an awful plague would have marked the annals of 1853."

XIV.—*A Universal Formulary, containing the methods of preparing and administering officinal and other medicines, the whole adapted to Physicians and Pharmacutists.* By R. EGGLESFELD GRIFFITH, M.D. A new edition, carefully revised and much extended. By ROBERT P. THOMAS, M.D. With illustrations. Philadelphia: Blanchard & Lea. 1854. (From the publishers.)

THIS large and valuable work, the first edition of which we noticed, we cheerfully commend to our readers. We have ourselves found this, as well as Ellis' Medical Formulary, of great service, and we think no Physician should be without one or both. Besides the very long list of valuable formulæ receipts, &c., it contains much other useful matter with respect to incompatible substances, antidotes for poisons, doses of medicines, management of the sick room, including dietetic preparations, &c. &c. Near the end there is a list of diseases with reference to the various formulæ, adapted to their treatment, which are contained in the work.

696 *Abstracts from Foreign and Domestic Journals.*

ANR. XV.—*Hand Book of Chemistry, Theoretical, Practical and Technical.* By F. A. ABEL, Professor of Chemistry in the Royal Military Academy, Woolwich; and Assistant Teacher of Chemistry at St. Bartholomew's Hospital; and C. L. BLOXAM, formerly First Assistant to the Royal College of Chemistry. With a preface by Dr. HOFFMAN, and numerous illustrations on wood. Philadelphia: Blanchard & Lea. 1854. (From the publishers.)

WE have examined this work, though casually, and we have been induced to conceive a very favourable impression of it from its fullness, the amount of material it contains, as well as the care with which it appears to have been prepared. It seems to us, however, not so much adapted to the requirements of the Student as a text book, as for the purposes of reference and consultation, when one desires to know the chemical composition of bodies, the mode of analysis, the apparatus used, and the numerous details of a practical character, which the working Chemist has such frequent need for.

ABSTRACTS

FROM FOREIGN AND DOMESTIC JOURNALS.

PATHOLOGY AND PRACTICAL MEDICINE.

Prevalent Diseases—Cholera Morbus. (Translated for the Charleston Medical Journal and Review, from the *Journal de Medecine et de Chirurgie pratiques, Paris.*)

When we sent our last issue to press, cases of cholera had, for a fortnight before become so numerous, that we almost expected an early invasion of the epidemic in our capital. This expectation, however,

and '49. Changes which have supervened in the medical constitution, independently of the large number of patients daily entering our wards, were quite sufficient, without any over anxiety on our part, to make us fear such an event. Here, for example, is what was observed in the end of April, and what even now we sometimes observe. A person in habitual good health, after a day spent in pleasure or business, is taken suddenly, with malaise, efforts to vomit, violent colics; soon after with vomiting and diarrhea, diminution of animal heat, cramps, and the physician when called recognises a veritable attack of cholera. Very often in spite of all remedial measures, the patient sinks into a state of profound prostration, and yields after a few hours, with mixed symptoms of cerebral congestion and adynamia.

These accidents, as we have stated, assumed about the middle of April, a certain importance in the medical constitution, adding sensibly to the daily mortality, but what ought to be regarded as the most disagreeable symptom in the public health, was the influence of the epidemic among the mass of the population; as has been noticed in previous epidemics; thus were observed a large number of what are termed *choleric accidents*, depending seemingly upon those causes which produce the cholera itself; diarrhea was extremely prevalent; it commonly yielded to ordinary medication, but it sometimes proved excessively rebellious, and there have here happened many deaths from it; as was also observed in England a year ago. Healthy persons, too, were suddenly taken with general refrigeration; they complained of no suffering, but were apparently totally unable to warm themselves, and singularly enough, while they shivered as in an access of intermittent, the skin was not always sensibly cooler to the touch than naturally. Others complained of cramps in the stomach, desire to vomit, or perhaps a little vomiting, a constant malaise, aversion to food, weakness, discouragement; the digestive functions were disturbed, and it was upon the whole, quite difficult to specify their disease, without attributing it to the epidemic influence above.

Thus, we repeat, have commenced all epidemics of cholera. Let us add, that the gravest diseases, such as pneumonia, angina pseudo-membranosa, measles, scarlatina, typhoid fever, were also very numerous, and became often complicated, at their commencement or termination, by choleric symptoms greatly increasing their gravity. At this moment the cases of cholera have become exceptional, and almost devoid of influence upon the statistic of mortality, and the general sanitary condition is much better, the hospitals amply sufficing for the necessities of the moment.

We will not pause to seek for an explanation of this sudden and unexpected apparition, or of the unlooked for calm which has so suddenly supervened: We know nothing of the causes of cholera, and must wait until our ignorance be less complete concerning the etiology of epidemics generally, and of this in particular, before attempting to emit any theory upon the subject; but in this little invasion of the Asiatic plague, as in preceding ones, one incontestable fact has overruled all others, viz: the existence of a premonitory diarrhea, observed almost constantly, and scarcely ever followed by cholera, when seriously

grappled with. Some exceptions have doubtless been observed, which, however, have only tended to confirm the rule. But we think, that in default of positive knowledge of the causes and nature of the scourge continually *threatening* us, this point must be considered as established, and that diarrhoea which heralds so to speak, the enemy itself, must be carefully watched and skillfully combatted, while there is yet time, and before in its momentary course, it shall have entirely passed beyond the resources of art.

SURGERY AND SURGICAL PATHOLOGY.

Local Anaesthesia. (Translated from the Journal de Medecine et de Chirurgie Pratiques, for the Charleston Medical Journal and Review.) Experiments upon local anaesthesia are still continued both in France and England, and we do not consider ourselves too sanguine, in expressing a well grounded hope that sensibility may, after a while, be locally diminished and suspended, by impregnating in some way, the external and accessible internal organs with vapour of chloroform. We have unfortunately not witnessed ourselves any thing decisive in this matter as yet; but in England it appears that results have been more favourable. Two important communications have been addressed to the Medico-Chirurgical Society of London, and to the Surgical Society of Ireland, which show that in these places experiments upon this subject are being rapidly pushed forward, and that a large number of physicians have already obtained very remarkable results. Thus Mr. Snow has discovered that lint (charpie) impregnated with chloroform, placed upon the skin, and covered over with a bit of oiled silk, occasions at first a degree of burning in the part; after a few moments, sensibility becomes diminished, and the skin may be transfixed with pins without pain; but on pursuing his investigations, he has found that the epidermis opposes itself more or less to the anaesthetic influence, and that when it was previously removed, insensibility of the dermis was very easily determined, and it could be pierced and lacerated with impunity.

This observation is of great importance to the success of local anaesthesia: it is moreover in conformity with the experiments of other

has communicated to the Society of Obstetrics many remarkable facts, and among others, the following :—A poor woman was suffering from a cancer of the rectum opening into the vagina and causing intolerable pain, against which large doses of opium had been administered. The vapour of chloroform directed upon the part, produced an instantaneous relief. It was then practicable to clean and dress the cancerous surface, a thing which its extreme sensibility had before rendered quite impossible, and this practice being continued, the patient soon found herself so much more comfortable that she was able to rise and reappear in society. An insufflation of the vapour into the rectum during fifteen or twenty minutes, sufficed to obtain abundant repose, and it was specially used for this purpose repeatedly. During three months these fumigations with chloroform have been continued, and have in no wise lost their efficacy. The patient finds herself quite improved from its use, and since its first employment there has been no necessity to have recourse to opium or any other narcotic.

Another observation, due to Dr. Ringland, is no less remarkable. A child, but two months old, was affected with a bronchite, attended with suffocation and extreme agitation, and had not slept a moment for seventy-two hours. A great number of remedies had been tried, but without the least amelioration. At length, a small quantity of the vapour of chloroform was injected into the rectum by the process of Mr. Hardy. After an interval of less than a quarter of an hour, the child became calm, and fell into a profound sleep, which lasted for two hours. When it awoke, it took the breast, which it had constantly refused before its sleep. The employment of the anaesthetic vapours was resumed, and shortly afterwards the child fell once more into a deep sleep. It was necessary to awaken it after several hours to give it the breast. This process was repeated several times and conducted to a complete cure.

A few days since, says Mr. Hardy, a physician was speaking to me of a woman who was suffering from a violent pain in the ear. She was unable to procure a moment of repose, notwithstanding the use of numerous remedies. Finally it was resolved to apply a *douche* of chloroform to the meatus. After the lapse of ten minutes the pain ceased, and the patient slept profoundly. The following day she was troubled with a return of the pain, which, however, yielded immediately to a renewed application of the vapour. M. Hardy has observed a case precisely similar; the patient had introduced into the meatus, some cotton steeped in chloroform, without other effect than a sensation of heat. The same substance employed in vaporous *douches*, calmed the pain entirely after about eight or ten minutes, and no further uneasi-

at a time. An aneurism of the popliteal artery first suggested to him the propriety of modifying the instrument which had so well succeeded upon a small scale and upon surfaces of limited extent. The tumour in this case was treated by compression, and the pain resulting from the process was such that the patient was utterly unable to allow the bandaging to remain in situ. M. Hardy then bethought himself of an instrument constructed upon larger proportions than the first. By its means veritable vapour baths can be administered, and if it be desirable to act with vigour, a whole member, the pelvis, abdomen, or chest can be entirely enveloped. M. Hardy promises himself important results from the application of these vapours of chloroform, in a host of affections now happily modified by means of poultices, fomentations, embrocations, &c., methods far less powerful than the anaesthetic vapours, associated or not with water and other medicaments.

Many members of the Society have also declared that these vapour douches had very well succeeded with themselves, and one makes mention of the ablation of a finger, without pain to the patient.

If we may judge of the matter, by the two communications above cited, this method is received with much favour in England, and we may soon expect more important results.

MATERIA MEDIOA AND THERAPEUTICS.

Collodion in Gonorrheal Orchitis. (Journal de Medecine et de Chirurgie Pratique, Paris. Translated for the Charleston Medical Journal and Review.) If there was wanting anything to illustrate the difficulty of appreciating the effects of a medicament, we would cite what is now taking place at the Academy of Medecine, where we find surgeons differing widely with respect to the employment of collodion in gonorrheal orchite. It is known that impermeable coatings, and particularly collodion, have been recommended in superficial inflammations, where they act either by hindering the contact of air and the cutaneous transpiration, or rather perhaps by instituting a mild, equable, and uniform compression, such as could be obtained by no bandaging whatsoever. However they may be, the collodion has been tried several years ago, in gonorrheal orchite, and our readers may recollect our accounts of the experiments of a Belgic physician, M. Dechange, upon this substance. Many other practitioners have repeated the same experiments, and quite recently a professor of the College of Medecine of Bordeaux, M. le Docteur Costes, published in the Medical Journal of that city, three cases of orchite rapidly cured

pended, and the cure obtained was much more rapid than is customary under ordinary treatment.

Again, a military surgeon enjoying at the Academy of Medicine a well-merited reputation, M. Bonnafont, has communicated to that learned society numerous facts, in which the orchitis has been arrested, and in fact jugulated, in a very extraordinary manner, by the use of collodion. This substance has been already employed by him, no less than seventy times, never causing accidents and always effecting a cure in a very few days; once only the pain produced by the collodion was somewhat intense, but was in that case attributable to scarifications which had been made upon the scrotum. M. Bonnafont closed his remarks by declaring that in his opinion the collodion constitutes the surest and most energetic remedy that can possibly be opposed to orchitis, either gonorrheal or traumatic.

Here then is a medication which receives the high approbation of men distinguished in the science, and who have based their opinions, moreover, upon a considerable number of facts. We see, however, that M. Bonnafont's memoir has given rise to some sharp discussions, and among the most distinguished surgeons of that assembly, the collodion has found an opposition, which slightly impairs our confidence in its virtues. M. Ricord, very advantageously situated to appreciate the various treatments of orchitis so common in the *Hopital du Midi*, has commenced by declaring that the substance which had procured for M. Bonnafont such remarkable and constant cures, had never succeeded with himself; that as often as he had recourse to it, it had occasioned intolerable pains, and that the duration of the disease had in no way been modified by its use. Eight instances have been observed by him, and universally, the pretended remedy has determined atrocious suffering; the disease moreover always ran its course, as if no remedial means whatsoever had been employed. M. Robert has not derived any better effects from its use, and he is aware that M. Puche, a colleague of M. Ricord at the *Hopital du Midi*, has not been in any wise more fortunate than himself.

Finally, M. Velpeau does not seem to place much reliance in the collodion. After having employed it a great number of times he has finished by abandoning it, doubtless because he had not had occasion to be pleased with the results of his investigation. It is therefore not without surprise that he has seen prescribed a remedy whose inconvenience and slight efficacy he had already determined.

Must we not be amazed at such divergence of opinion upon a point which does not *seem* to be susceptible of discussion? Is it an affair of such difficulty to encase the scrotum in collodion, that it could be affirmed that M. Bonnafont has proceeded with greater skill than his confrere? Does the collodion itself act differently according to the sub-

supportable, and which did not hinder a cure, while M. Ricord has been obliged to renounce entirely the use of the remedy, the patients preferring to quit the hospital rather than to submit to such a system of medication.

From this we see with what caution we must proceed to judge of the facts upon which the recommendation or rejection of a remedy must rest. Nevertheless it is not impossible that the honorable academicians now so divided in opinion, may at last come to an understanding and give us the solution of the enigma. The Academy, astonished at the different results to which surgeons of equal merit and scientific loyalty, have arrived, has been unwilling to nominate any committee to examine a question which would doubtless have been very tardy in adjustment. It has invited M. Bonnafont to associate himself with M. Ricord, and to experiment before him upon some of the orchitic patients always to be found in his service. That physician will bring his collodion and will operate in the presence of his confrere, and certain it is that one or the other will depart *convinced*. It is certainly the surest and most expeditious mode of introducing order and harmony once more into our scientific counsels.

Lacto-Vairolic Inoculation. (Translated for the Charleston Medical Journal and Review, from the *Journal de Med. et de Chirurgie Pratiques*. By W. H. FORD, M.D. See a translation on the same subject prepared by the editors and published in the July issue.)—A procedure substituted for another procedure, is by no means always a progress; far from it, it is frequently a obstacle to the natural march of our ideas, which tend, as we know, towards perfection; and it is not without reason that the wise man has said, "Le mieux est parfois l'ennemi du bien." That reflection suggested itself to us upon perusing, recently, the pitiful experiments made at Lyon, to improve vaccine, and which have only resulted, as was in fact inevitable, in compromising that salutary operation in the eyes of the public.

In 1852 a physician of the hospital of Hôtel Dieu at Lyons, having received into its wards three nurslings, with their mothers, of whom one was affected with small-pox, fearing lest the disease should extend to the young children, and not having any vaccine virus at command, inoculated them with variolic matter mixed with milk. This lacto-variolic inoculation was followed by a local eruption quite similar to vaccine, passing through the phases peculiar to that affection. There was no general eruption, and the children appeared preserved from the small-pox. M. Brachet, the author of these inoculations, communicated to the Medical Society of Lyon, these three remarkable facts, and requested the institution of experiments for the purpose of determining

tigations. This gentleman, in the course of the year 1858, inoculated a certain number of children with matter from patients affected with discrete variola, and mixed with cow's milk. A second was then inoculated from the pustules of the first child; next, a third from the second, and, according to the account of the young physician then interne at Hotel Dieu (Lyon) M. Antonin Bossu, who has given us in his *these* the history of these inoculations, eighteen children were thus inoculated with complete success. The eruption was almost always local, and when, exceptionally, it became general, its character was extremely benign. Moreover, the children thus experimented upon, had become insusceptible to the vaccine disease, and probably also to the small-pox, since not one of them was attacked by it, although several cases existed in the same wards while they remained in the hospital.

The parties, therefore, had apparent reason to congratulate themselves upon the issue of their experiments, when a few months since they were resumed, and several children inoculated in the same way; but this time the results have been sadly different. The eruption after having remained local for several days, soon extended to the rest of the body, and our experimentalists had to deal with veritable confluent small-pox. Two of the little subjects succumbed. This unforeseen result, though so much to have been expected, has, as may readily be imagined, set a term to these variolic inoculations, and the improvement of the vaccine virus has for the moment been relinquished. We repeat that it was easy to foresee these results, because these experiments were by no means new. At Marseilles, in fact, this lacto-variolic inoculation had been already tried and abandoned, since it was soon apparent that it was after all, an essential inoculation of unmodified small-pox. Every body knows that inoculation from the variolic pustule, without extraneous admixture, determines very often no more than a local pustulation; that when the pustules invade the whole surface of the body, they still remain occasionally quite benign; but that also, in a large number of cases the disease exhibits itself with all the characteristics of confluent, and spontaneously developed small-pox. It is only too apparent that the milk no more modifies the variolic virus, than does pus, water or any other liquid, and that if in these cases an unmixed lymph had been used, the results would have been purely identical.

These unfortunate experiments, though prompted by an honorable zeal, would scarcely be worthy of mention, but for their inevitable influence in tending to unsettle the confidence which all men of good sense feel in the efficacy of vaccine, and thus to produce the most deplorable consequences. What do men aim at in thus constantly laying vaccine under suspicion? One proclaims that the vaccine virus has served its time, and that we must return to the source of the cow-pox, and employ none but a *regenerate virus*; another assures us that this same virus quickly exhausts itself within the human organism, and that it must be renewed every ten or twenty years, more or less, in accordance with the scruples of the *revaccinators*; a third hopes to obtain the desirable *regenerate virus*, by a lacto-variolic inoculation, and thus, as has been also recently done for syphilis, introduces and extends the

very disease from which he wishes to defend his patient, and entirely to extinguish; a fourth goes still farther, and accuses the vaccine virus of being the source of an hundred evils. Finally, it would be apparently the desire of these persons to make us believe that the discovery of Jenner is only one of these facts which have been so much exaggerated in medical importance, one which it is necessary to reduce to its proper equilibrium, and which nobody cares to take under his individual patronage. When we say nobody, we speak perhaps somewhat too hastily. While the *savants* and learned societies, by their inopportune and idle discussions, impair the credit of that admirable institution which they ought to sustain to their utmost, while certain isolated physicians dream of useless modifications in an institution sanctioned by the experience of half a century, the readers of these sheets, the *real* practitioners, consecrate their time, their vigils and their logic, to combat the deplorable effects of that sort of conflict which certain men would fain create in the domain of science. Families which have heard of these dissensions, become more and more hostile to the vaccinal inoculation, and physicians continually fail by reason of the popular indifference. If, after many solicitations, they are accorded the *privilege* of vaccinating a child, they are almost sure that it will not be presented to them the following week for the verification of the little operation. It is quite enough that the parents have been once incommodated, and that they have, from regard for their physician, submitted their child to a practice which learned men declare powerless and even dangerous. There occurs, perhaps, soon after an epidemic of small-pox and this infant, whose vaccination no one has carefully supervised, furnishes still another argument to the partisans of revaccination. Thus it is that vaccine falls into discredit, and that *savants* assume towards vaccinating physicians positions in no wise tenable. Is it not pitiable to see a learned society authorise its committee upon vaccination to experiment with a virus mixed with *milk*, as if such practice was not opposed to every law of physiology, as if experience had not already pronounced upon the dangers of that practice, as if a single member of that body was ignorant that inoculation with the pure and recent virus of small-pox determines very frequently, *only* a local eruption? Of what further consequence were *three* more facts of its innocuity affixed to the number of those already possessed by science, and which certainly show that had it not been for the discovery of vaccine, variolic inoculation would have become a universal practice? It is, we repeat, much to be regretted that the members of the Society of Medecine of Lyons, had not well weighed these facts before plunging into an unadvisable experi-

at length comprehend that the greatest service they can possibly render to the institution, is to accept it such as it has been handed down to them, and as it has been entrusted to their care, and never to agitate questions bearing upon it.

MISCELLANEOUS.

Iodine Injections in Leucorrhœa. By Dr. RUSSELL. It is not my design, in this communication, to enter into any theoretical inquiries respecting the nature of leucorrhœa, or the *modus operandi* of the remedy I propose for its cure; my object being simply to give the results of my experience in relation to iodine as a remedial agent in this obstinate, and in many cases intractable disease; and in doing which, I shall record facts, well assured that one well-attested fact contributes more to the advancement of the science of medicine than three-fourths of the theories to which the press has ever given publicity.

I consider this disease to consist in inflammation of the vagina or the internal cavity of the uterus, or of both; and in the majority of those cases which have continued for a number of years and resisted the ordinary modes of treatment, ulceration to a greater or less extent will generally be found to exist. For this condition, I have found no remedy equal to iodine; and, in illustration of its effects in my hands, I will briefly detail several cases from a number that have come under my observation:

CASE I.—In July, 1848, I was requested, by Mr. W——, to visit Grace, a favourite mulatto servant, aged 48 years, who, he informed me, had leucorrhœa of twelve years' standing; had been under the care of a number of physicians during this period, and had been subjected to a great variety of treatment, but without avail. I found her confined to bed, very much debilitated and emaciated, face cadaverous, pulse quick and feeble, skin cool, urine scanty, severe pains in the lumbar and pelvic regions, and œdema of the lower extremities. The vaginal discharge, often escaping by gushes, was excessive, and her general health had become seriously involved from the effects of this long-continued drain to the constitution. Upon examination per vaginam, the vagina, os, and cervix uteri were found to be in a sub inflamed condition, and denuded of epithelium. The os was partially everted, and when deprived of its adherent mucus, presented a vermilion colour. The external cervix was enlarged, indurated, and ulcerated. The body of the uterus was sensibly enlarged, and descended within two inches of the os externum. The secretions from these several parts varied essentially in character, and when discharged externally resembled somewhat, in quantity, consistence, and colour, the yolk of an egg intermixed with purulent and sanguinolent matter, and all blended in a thick, opaque, tenacious plasma.

Ascertaining that she had never used iodine in any form, and believing it would afford her relief, I ordered the aqueous solution of the following strength to be thrown up the vagina twice daily, and retained

several minutes, the parts being previously well syringed with warm water and castile soap, and the patient placed in a horizontal position, with the hips elevated :

Rx. Iodini gr. i; potass. iodid. gr. ii; aquæ pluvialis zi. M.

As this solution ceased to create any sensation of warmth or excitement in the parts, it was gradually increased to treble its strength. The muriated tincture of iron, in the proportion of twenty drops three times daily, was given as a tonic.

Under this treatment, with a nourishing diet, she soon began to improve; the irritated condition of the parts gradually subsided, the muco-purulent discharges by degrees ceased, the cervical ulcers regularly healed, and at the end of three months from its commencement, she had regained her health and strength to such an extent as to enable her to resume her occupation as cook to the family. I may add, that her mammæ, which was very small and flaccid, became full and enlarged while under the influence of this medicine, and for several weeks secreted rather copiously a brownish watery fluid. This secretion being tested with starch, produced the characteristic blue colour, showing that the iodine was absorbed.

CASE II.—August, 1848. Mrs. W—— consulted me; she was aged 19; small and delicate figure; had been married four years, and dates the commencement of her present “weakness” to an abortion which occurred about six months subsequent to her marriage. Prior to marriage was remarkably healthy and active. At the time I saw her, she was anemic and emaciated, countenance chlorotic, eyes sunken; pulse feeble, menstruation painful, and either scanty or profuse. The vaginal discharge was constant and copious, muco-purulent, slightly streaked with blood, and very offensive. An examination, with the speculum, revealed an irritated condition of the vagina, with relaxation and loss of its natural rugæ, and accompanied by a partial displacement of the uterus. The cervix was enlarged and indurated, with several ulcers upon its external surface. She had never submitted to medical advice, contenting herself with the use of some simple domestic remedies. Correcting the torpid condition of her liver by means of the usual remedies, I prescribed the aqueous solution of iodine and the muriated tincture of iron as above, recommending a nourishing diet, with free exercise in the open air.

muco-purulent, and excessive. The cervix was soft and tender to the touch, and when seen by the speculum was found enlarged and presenting a dark greyish appearance. The os was patulous, with tumefied edges, and of a reddish tint. A slight abrasion was found on the posterior lip.

Astringent vaginal injections—as the nit. argent. acetat. plumbi, &c.—were advised, and to relieve the enlarged and indurated condition of the liver and spleen, I prescribed the following:

R. Prot. iod. mer. ℥i; pulv. aloes ℥iiss; ext. hyoscyami ℥i. M. Div. in pilulæ xxiv. One pill to be taken every night; at the same time five drops of nitro-muriatic acid in a wineglassful of the infusion of gentian, three times daily, was administered. Under this treatment, the visceral derangements totally disappeared in about eight weeks, and her general health was greatly restored.

The leucorrhœa still continuing (no benefit having resulted from the use of astringent injections), and no amelioration in the condition of the parts being found on a second vaginal examination which was now made, I ordered the aqueous solution of iodine and the muriated tincture of iron as above recommended. In six weeks after using these remedies, she declared herself well; shortly afterwards became pregnant, and was in time delivered of a fine healthy child.

Other cases could be adduced to prove the remedial powers of iodine as a local remedy in leucorrhœa, but as they are somewhat similar to the above in all essential particulars, it is unnecessary to introduce them here. Regarding the disease as being essentially a local one, our mode of treatment is principally local, and applied by means of a proper syringe to the parts affected. The preparation should at first be made weak, and gradually strengthened as the parts become accustomed to its application. The mildest preparation is frequently disagreeable, and sometimes painful, but these sensations are only momentary.

We have used it varying in strength from one to four grains of iodine with double the quantity of the iodide of potash to an ounce of water. It may be applied once or twice a day, or once every second or third day, as occasion may require. In some of the severer forms of this complaint, attended with considerable abrasion and ulceration, the diluted tincture may be used with great advantage.

Its curative powers are far greater than the nitrate of silver—which, in our hands, often seemed to exasperate the complaint—or any other remedy with which we are acquainted.

Law of Cholera. (Med. News and Library.) In our number for November, 1853, p. 189, we noticed a very important law in reference to the influence of the altitude of places on the mortality of this dis-

Duncan, of Liverpool, has, at the request of the former gentleman, examined the influence of the elevation of soil on the increments of mortality from cholera in that city, and confirms Mr. Farr's law so far as to show that "when the districts of approximating elevations are grouped together, and the groups so formed contrasted, the results distinctly point to a relation between the elevation of the soil and the mortality from cholera." More general, though far less exact (mathematically speaking), observation fully confirms the truth of Mr. Farr's law. In whatever part of the globe the history of the cholera there raging is investigated, it will be found as the rule that the disease affects most fatally the low-lying seaports and deltas of rivers, sparing the high grounds, even round river sources. In India, we know it began in the delta of the Ganges, ravaged all low-lying places; while sepoy lines, placed from sixty to a hundred feet above the general level of the country, had scarcely any cases, excepting such as occurred in persons on guard at the different outposts. The higher classes of natives and Europeans generally inhabiting the better raised and more airy parts of the towns suffered proportionably less than the lower ranks, and indeed the general result was such as to lead Mr. Jameson, in his report of the Bengal cholera, in 1817-'19, to say that "there is abundant proof that in high, dry, and generally salubrious spots, it was both less frequent in its appearance, and less general and fatal in its attacks than in those that were low and manifestly unwholesome." In illustration of the law (established at any rate for the great "London Basin") that *the mortality from cholera is in the inverse ratio of the elevation*, we may remark, that if the districts of London are classified according to their elevation above the level of the Thames, in those whose elevation is not on an average twenty feet above its water line, it will be found that "on the bottom of the London Basin the mortality was at the average rate of 102 in 10,000; while in a district, 100 feet high, the mortality was 17 in 10,000; and in another (Hampstead), of an altitude of 350 feet, it was 8, or, deducting a stranger infected at Wandsworth, but who died there, 7 in 10,000." Now, if the mortality from cholera, according to the data here involved, and as occurring in relation to the elevation and mortality of other groups of districts included between those of less than 20 and above 350 feet of altitude be closely examined, the relation between elevation and death will be expressed as in the following table, on the one hand, as the facts actually occurred and on the other, as they should occur

It was found by Mr. Farr, on comparing the numbers of his series with the mean mortality observed in districts of eight different elevations, that the only considerable discrepancy was at the mean elevation (20—40) assumed to be thirty feet. The relation thus indicated being so important, it was thought proper by its discoverer to submit the principle to another test—viz: to compare the elevation and mortality from cholera of each *sub-district*, and this comparison, though it made the mortality on the lowest level less, and became deranged somewhat by the deaths in hospitals and workhouses, entirely confirmed the announced law. (*Report on Cholera*, p. lxvii.) Whilst the *inverse ratio* is, we think, generally amply sustained, it yet suffers, it must be confessed, considerable perturbation; so much so, in some cases, as to have led to but little appreciation of its value. "Instances of this kind," remarks Dr. Baly (College of Phys. Report, 18), "which are found to be very numerous, when the relation between the mortality from cholera and the elevation of the surface is examined in the cases of the sub-districts of the metropolis, go far to prove that, even in London, the connection between lowness of level and the prevalence of cholera, depends, at least, in great part on the character of the inhabitants and their dwellings in the lower districts, and on the foulness, as distinguished from mere dampness of the atmosphere in their localities." Dr. Duncan found also in Liverpool, that taking the districts singly, where the difference of elevation is only two or three feet, the law was not carried out, *being apparently overpowered by disturbing elements*. Notwithstanding all this, and much more which might be adduced if our space permitted, we accord in the opinion that, in considering the law thus announced, it is necessary to view it in connection with certain circumstances impeding its operation, and producing some deviation from strict mathematical accuracy, and that, such being yielded, as should be done in all complicated questions of hygiene, we shall find "that this *inverse ratio* is maintained as accurately as is requisite for the theory."

Chloroform as a local Anæsthetic. (Condensed from the Journal de Médecine et de Chirurgie.) It is well known that certain neuralgias yield readily to the vapour of chloroform, as if by enchantment; and that also when a painful part is enveloped by the vapours, it loses its sensibility. Dr. Hardy, vice-president of the Obstetrical Society at Dublin, invented an instrument destined to direct the vapours of chloroform upon the painful parts, and he obtained remarkable results. M. Hardy used this instrument specially against uterine pains accompanying cancer. The patients could employ for themselves, these vapour douches, and as soon as the neck of the uterus became enveloped, the pains ceased, as if by enchantment. The pains also ceased in the back and loins, and the relief continued for some time. When the pain reappears, it suffices to re-apply the means referred to dissipate it a second time. M. Hardy also applied effectually the same means for the relief of painful swelling of the breasts, applying the canule of the instrument under the bit of linen which covered the diseased organ. M. Charrière has introduced this instrument into France, and it is now widely used. They have gone further still, though not always with complete success,

in endeavouring to extend its use to producing insensibility previous to the employment of the knife. Nelaton, Dubois and Michon have employed it successfully. At Val-de-grâce also, M. Larrey applied this little instrument successfully, in relieving the pain in the wound following amputation of the thigh. M. Charrière has modelled it by the addition of a flexible tube, by means of which the vapour can be applied to various portions of the body.

EDITORIAL AND MISCELLANEOUS.

Yellow Fever.

It is with great regret that we record the prevalence, in this city, of the Yellow Fever. Whilst the "Hæmagastric Pestilence" devastated, last year, the cities of New Orleans and Mobile and numerous minor towns in the South West, our city enjoyed a complete exemption from it. This summer the destroyer has laid his heavy hand upon Savannah and Charleston.

As far as our information goes, the appearance of the Fever in Savannah ante-dated its appearance in our city, and the mortality in the former has been truly frightful. It seems to be accompanied, in that city, by a very fatal form of Congestive Fever.

With us, the approach of the Yellow Fever was heralded by some cases of mild Remittent and Intermittent Fever.

The subjoined extract from the Report of the City Register, of the 22d August, will exhibit the dates and order of succession of the cases that have occurred up to that period :

On the 11th of May the Isabel arrived from Havana and Key West. Three days after her arrival a steerage passenger who had been taken in at Key West, was sent to the Marine Hospital with well marked symptoms of Yellow Fever—he recovered, but threw up black vomit freely.

On the 11th of July the Isabel again arrived from the same ports. One of her passengers, an Irish nurse, went to an hotel, complaining

tain was forced to run her ashore near Georgetown. She was brought to Quarantine in tow of a steamer in a sinking condition, the sick sent to Lazaretto, and after leaving Quarantine she lay for two days at Commercial wharf, and then went up to Union wharves.

The Spanish polacres Columbus and Concha, from Havana, arrived at Commercial wharves on the 22d of July from Quarantine. On the 28th, one man from each of these vessels was taken sick and sent to the Lazaretto—one died with black vomit, the other recovered speedily, and I believe was not considered as a case of Yellow Fever. The vessels were ordered back to Quarantine.

The ship Sullivan arrived at the same dock from New York on the 21st of July. On the 7th of August a seaman was sent to the Marine Hospital with Yellow Fever. On the 9th he was transferred with one other from the same ship to the Lazaretto—one died on the 10th, throwing up black vomit—the other recovered.

The Vesta arrived on the 16th of July from Boston—lay at Union wharf next the Aquatic. On the 7th of August one of her seamen was sent to the Marine Hospital, on the 9th transferred to Lazaretto, and recovered. On the 11th another, from same vessel, was sent into the Marine Hospital, and died on the 15th with black vomit.

The brig Iris arrived on the 22d July, from Maine, and lay at the Custom House wharf, (new.) On the first of August she hauled round to Potter's Mills in Ashley River; on the 7th the Steward was sent to the Marine Hospital with malarial fever, and recovered. On the 10th the Captain, after several days sickness, entered the Hospital, and is still under treatment; the case is regarded by the Physician of the House as yellow fever—no black vomit.

On the 12th of August, a man who had been at work on board of the Aquatic, and residing in Pinckney-street, sickened with fever and died on the 14th without throwing up black vomit.

On the 18th of August, an Irish woman residing on East Bay, near the Battery, was seized with yellow fever, and died on the 15th, throwing up black vomit freely.

The barque Aura arrived at Brown's wharf on the 2d of August, from New Orleans—after discharging cargo she was hauled round to Ashley river on the 12th; on the 14th one of her men was sent to the Marine Hospital with fever and recovered.

The schooner Monterey, from Philadelphia, arrived at Central wharf on the 7th August; on the 17th one man was sent to the Marine Hospital, and is now convalescent.

The Susan Cannon, from Baltimore, arrived at the same wharf on the 8th August; on the 17th one man was sent to Marine Hospital, who is still under treatment.

The schooner Maryland, from Baltimore, arrived at Central wharf on the 9th of August. On the 17th the Captain was sent to the Hospital and died on the 21st. He had no black vomit, and had been two days in Ashley river prior to entering the Hospital.

The J. P. Brown, from Philadelphia, arrived at Central wharf on the 8th of August. On the 15th went round to Ashley river. On the 17th

one of the seamen was sent into the Marine Hospital, and died on the 20th with black vomit.

On the 17th of August a clerk on Central wharf was attacked with yellow fever, and is still under treatment.

The brig Emily arrived at Accommodation wharf from Boston on the 8th of August. A seaman was sent to the Marine Hospital on the 18th, having been three days sick before admission, and died on the 20th with black vomit.

On the 18th of August, the wife of an Irishman residing in Calhoun-street, near Anson, died with black vomit, having been sick two days before calling in a physician.

On the 18th of August, a man residing at 33 Broad-street, was taken sick, and is still under treatment.

On the 19th, two Germans residing at the foot of Hasel street, near Dry Dock, were attacked, and one at the corner of State and Queen on the 21st. These are all under treatment.

It will appear from the above facts that the total number of cases have been twenty-three, and the number of deaths nine. Of the 23 cases, eight occurred in the city, and fifteen among sailors on shipboard, 4 of the fifteen being imported.

Of the nine deaths, six occurred in the city and three at the Lazaretto.

We have no intention, at this time and in this place, of entering upon a consideration of the origin of the fever; whether caused by the foul air contained in the holds of the Spanish vessels, or whether proceeding from local causes.

This question we expect to discuss at length in our report to the next meeting of the American Medical Association. Our object is to lay before our readers a plain statement of facts, from which they may draw their own conclusions. We shall only say, further, that the disease, although at its commencement, (not being yet epidemic) exhibits a degree of malignancy, perhaps unsurpassed in the history of its prevalence in Charleston; and that, since the report, above quoted, was made, it would appear that the *proportion of recoveries is about 1 in 2½ or 3*; a number of the cases mentioned in the report, as being then under treatment, having subsequently terminated fatally.

Death of Dr. Burnett.

We regret to record the death, in Boston, of Consumption, of Dr. W. J. Burnett, of that city, whose fate is mourned by the Medical Profession of the United States. Dr. B., though young, was not only a man of great promise, but was already one of the most distinguished Microscopists of his age in this country, and his observations and papers were

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TO READERS, CORRESPONDENTS, PUBLISHERS, &c.

THE following books and pamphlets have been received :

Traité Therapeutique de Quinquina et de ses Preparations, par P. Briquet Médecin de l'Hôpital de la Charité, Agrégé Hon. de la Faculté de Médecine de Paris. Chev. de la Legion d'Honneur. Paris Libraire de Victor Masson. 1854. (Imported for Charleston Medical Journal and Review. To be reviewed.)

Traité des Clinique et Pratique des Malades des Enfants, par M. M. E. Barthez, Médecin de l'Hôpital, Sainte Marguerite de Paris. Chev. de la Legion d'Honneur, et F. Rilliet Méd. en chef de l'Hôpital de Gêve; Memb. of Acad. of St. Petersburg, Turin, Edinburgh, Ancient Internes de l'Hôpital des Enfants Malades de Paris. Ouvrage couronnée, par l'Acad. de Méd. et de Sciences, autorisée par le Conseil de l'Instruction publique pour les facultés et les écoles préparatoires de Méd. Deuxième Ed. entièrement refondue et considérablement augmentée. Paris: Germer Gaillière. 1853. (Imported for Charleston Medical Journal. To be reviewed.)

A Universal Formulary : containing the methods of preparing and administering officinal and other medicines. The whole adapted to Physicians and Pharmacutists. By R. Eglesfield Griffith, M.D. A new edition, carefully revised and much extended; by R. P. Thomas, M.D., with illustrations. Philadelphia: Blanchard & Lea. 1854. (From the publishers.)

A Clinical Introduction to the practice of Auscultation, and other modes of Physical Diagnosis, in diseases of the lungs and heart. By H. M. Hugos, M.D., Fellow of the Royal College of Phys., Assistant Phys. to Guy's Hos. Second Am. from second and revised English edition. Philadelphia: Blanchard & Lea. 1854. (From the publishers.)

An examination of the Practice of Blood-letting in Mental Disorders. By Pliny Earle, M.D., author of a visit to Thirteen Asylums for the Insane in Europe; Hist., Descrip. and Statistics of the Bloomingdale Asylum for the Insane; and Institutions for the Insane in Prussia, Austria and Germany. New-York: S. & W. Wood. 1854. (From the publishers.)

Observations Sur L'Osteogénie. Par Charles Robin: Prof. Agrégé à la Faculté de Médecine de Paris. (From the author.)

Considerations sur les Principes immédiates des Corps organisés. Par M. Hifelsheim, Membre de la Soc. de Biol. Paris. (From Ch. Robin, Paris.)

Auscultation and Percussion, by Dr. Joseph Skoda. Translated from the fourth edition. By W. O. Markham, M.D., Assistant Physician to St. Mary's Hospital. Philadelphia: Lindsay & Blakiston. (From publishers through Mr. John Russell.)

Recherches sur plusieurs Points d'Anatomie. Par le Docteur J. B., Froment-Prosecteur des Hôpitaux, Membre de la Soc. Chirurg. de Paris, &c. Paris: 1853. (From the author.)

[This gentleman is the able Prosector at Claymart's, near Paris, whose practical courses and demonstrations on Operative Surgery, are taken by many of our countrymen.]—*Eds. Charleston Medical Journal*.

By-Laws of the Elliott Society of Natural History. Charleston, S. C.: 1853.

Outline of the Principles and Practice of the Osteopathic Treatment of

several well known professional men of the city of New York.]—*Eds. Charleston Medical Journal.*

On the Subject of Priority in the Medication of the Larynx and Trachea. By Horace Green, M.D. New-York: Baker, Godwin & Co. 1854.

Observations on Exhaustion from the Effects of Heat. (Coup de Soliel.) By H. S. Swift, M.D., Resident Physician of the New-York Hospital. (From the New-York Journal of Medicine.) New-York: Holman, Gray & Co. 1854.

Transactions of the Fifth Annual Meeting of the Medical Society of the State of North-Carolina, held at Raleigh, N. C., May, 1854. Wilmington, N. C.: Fulton & Price. 1854.

The Transactions of the Iowa State Medical Society, Third and Fourth Sessions, held at Fairfield, May, 1852, and Davenport, June, 1853. Burlington: 1854.

Constitution and By-Laws of the Medical Association of Texas; proceedings of the last Annual Meeting, together with the Anniversary Address, delivered by Geo. Cupples, M.D., November, 1853. Austin: 1854.

Observations on the Cause, Nature and Treatment of Epidemic Cholera. By A. B. Palmer, M.D., Professor of Anatomy in the University of Michigan. Detroit: G. E. Pomeroy & Co. 1854.

We are constantly in receipt of an increased number of British, French and Italian exchanges, recently effected, from which translations and condensations are furnished at some pains, as our subscribers perceive, in each number of the Journal. We receive these Journals from our foreign correspondents, and from New-York, through Adams' Express, at no inconsiderable expense. Want of space prevents us from furnishing the list.

ERRATA.

In a previous issue in the European correspondence from Italy, the words *Juni-perus Virginiana* were written by mistake for "*Diocypus Virginia*."

*Meteorological Observations made at Aiken, in South-Carolina,
for the month of June, 1854, by H. W. RAVENEL.*

Days.	Rain and Melted Snow.	Psychrometer.						Daily Remarks on the Weather.
		Dry bulb.			Wet bulb.			
	Amount inches.	7 A. M.	9 P. M.	7 A. M.	9 P. M.	7 A. M.	9 P. M.	
1	0.318	61	58	55	57	54	53	heavy & overcast all day, with light showers.
2		56	67	62	52	57	56	overcast all day.
3	0.335	55	67	65	53	62	61	rain all morn'g; heavy & overcast in after'n.
4		64	75	74	59	68	70	variable.
5		71	82	77	68	73	72	clear, with passing clouds. [afternoon.
6		72	84	81	71	73	76	clear morning; clouds with distant thunder in
7	0.251	75	77	76	72	73	71	shower in morning; variable.
8		73	86	67	69	79	63	clear all day.
9		67	83	78	62	67	70	clear all day.
10	0.425	73	84	77	69	77	71	clear morn'g with thunder & rain at mid-day.
11	0.200	78	81	76	75	75	74	ditto.
12		74	87	79	71	75	69	clear all day.
13		71	80	76	66	70	71	cloudy morning, clear in the afternoon.
14		73	84	80	70	71	71	clear all day.
15		73	86	81	69	72	72	clear all day.
16	0.158	76	87	75	71	77	72	clear morning, slight shower in afternoon.
17	0.779	74	78	70	73	73	68	cloudy morning; rain all afternoon and night.
18	0.135	69	79	74	67	73	71	rain in morning; cloudy the rest of the day.
19		71	80	79	69	73	74	variable.
20		73	84	81	71	74	75	clear all day.
21		75	87	80	71	76	76	clear morning; cloudy afternoon.
22		75	85	79	73	76	75	clear in morning; distant clouds in afternoon.
23		77	88	83	74	65	76	clear.
24		77	91	85	73	73	72	clear all day; fine breeze.
25		78	94	86	71	74	76	do. very dry atmosphere.
26		78	93	86	73	74	77	do. do.
27		81	94	88	77	77	79	do. do.
28		82	96	90	77	80	80	do. 99° at 4 P. M. [in afternoon.
29		84	96	92	77	77	80	do. 99 at 5½ P. M.; a few floating clds.
30		86	97	90	79	80	81	clear m'g. 98 at P.M.; clds. with thun. & light.

2.601

Meteorological Tables.

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Meteorological Observations made at Aiken, in South-Carolina, for the month of July, 1854, by H. W. RAVENEL.

Days.	Rain and Melted Snow.			Psychrometer.						Daily Remarks on the Weather.
				Dry bulb.			Wet bulb.			
	Hour began.	Hour ended.	Am't. inches.	7 A. M.	9 P. M.	7 A. M.	9 P. M.	9 P. M.		
1	P. M. 2.30	P. M. 3.50	1.300	83 92 80	78 81 77				close & hot m'g; thun. stm. & rain in aft'n.	
2			1.752	78 86 76	75 78 74				Thun. stm. & rain before daylight & in aft'n	
3				75 86 82	73 76 78				variable.	
4				79 88 86	76 79 78				clear.	
5				80 90 88	75 78 79				clear.	
6				81 92 87	76 80 78				clear; distant thunder in afternoon.	
7	P. M. 6.25		0.793	81 93 81	76 80 77				clear morning; rain in after'n. and evening.	
8	P. M. 6.30	P. M. 7.20	2.724	80 90 78	77 80 75				[rain.	
9			0.016	79 90 85	76 79 80				clear morning; violent thunder storm and	
10			0.045	80 91 80	76 79 76				variable; light shower about 3 P. M.	
11				77 81 79	74 75 74				clear morning; light shower in afternoon.	
12				76 85 79	71 71 71				overcast most of the day.	
13				73 85 80	66 70 71				clear.	
14				75 88 82	68 73 74				clear.	
15				78 88 85	73 76 76				clear.	
16				78 91 85	72 76 74				clear.	
17				79 93 86	75 76 78				clear.	
18	P. M. 4.85	P. M. 5.00	0.140	81 92 82	75 80 77				[in afternoon.	
19				79 91 86	74 78 76				clear morning; thunder storm and shower	
20				79 91 86	75 78 79				clear.	
21	P. M. 4.35		0.830	80 91 77	75 76 74				[afternoon.	
22				75 84 79	73 78 75				clear morning; thunder storm and rain in	
23				75 88 79	73 76 73				overcast and foggy in morning; clears off.	
24				77 83 78	75 78 75				variable; cloudy, with distant thun. in aft'n.	
25				76 85 82	74 78 76				variable; light showers.	
									cloudy morning; clears off.	
26	P. M. 8.25		1.552	79 88 77	75 76 74				[night.	
									clear morning; thunder storm and rain at	
27	P. M. 4.00		1.689	75 85 74	73 76 73				[all the afternoon.	
									morning variable; thunder storm and rain	

SAVANNAH MEDICAL COLLEGE.

SESSION 1854-'5.

R. D. ARNOLD, M.D., Professor Theory and Practice of Medicine.

P. M. KOLLOCK, M.D., Professor of Obstetrics.

W. G. BULLOCK, M.D., Professor of Surgery.

C. W. WEST, M.D., Professor of Medical Chemistry.

H. L. BYRD, M.D., Professor Materia Medica and Therapeutics.

E. H. MARTIN, M.D., Professor of Institutes of Medicine.

J. G. HOWARD, M.D., Professor of Anatomy.

J. B. READ, M.D., Professor of Pathological Anatomy and Demonstrator of Anatomy.

J. J. WEST, M.D., Assistant Demonstrator of Anatomy.

The regular course of LECTURES in this Institution, will commence on the *first Monday in November*, and terminate in the following March. Preliminary Lectures will be delivered from Monday, the 16th day of October, until the commencement of the regular course, for which no additional fee will be required. Students have free access to the Medical and Surgical Wards of the Savannah Hospital. This extensive charity is under the medical control of two members of the Faculty, by whom clinical lectures are delivered. In addition to this, they have ample opportunity for learning *practical* medicine at the College Clinique which is open daily to patients.

During the past session upwards of two hundred cases were prescribed for, and over thirty surgical operations performed in the presence of the students at this clinique. *Material* for anatomical investigations will be abundantly supplied.

FEES—Matriculation, \$5; full Course of Lectures, \$105; Demonstration, \$10; Graduation, \$30. For further information, address (post paid),
Sept. 1 H. L. BYRD, M.D., Dean.

UNIVERSITY OF LOUISVILLE.

MEDICAL DEPARTMENT.

The Eighteenth annual course of Lectures in this Department, will commence on the 30th of October next, and terminate on the last of February, under the following arrangement:

BENJAMIN R. PALMER, M.D., Professor of Descriptive and Surgical Anatomy.

LUNSFORD P. YANDELL, M.D., Professor of Physiology and Pathological Anatomy.

SAMUEL D. GROSS, M.D., Professor of the Principles and Practice of Surgery.

HENRY MILLER, M.D., Professor of Obstetric Medicine.

LEWIS ROGERS, M.D., Professor of Materia Medica and Therapeutics.

J. LAWRENCE SMITH, M.D., Professor of Medical Chemistry and Toxicology.

ARTHUR FLINT, M.D., Professor of the Theory and Practice of Medicine.

CHARLESTON

MEDICAL JOURNAL AND REVIEW.

Vol. IX.] CHARLESTON, S. C., NOVEMBER, 1854.

[No. 6.]

ART. I.—*An Inquiry into some of the general and local causes to which the endemic origin of Yellow Fever has been attributed by myself and others.* By WM. HUME, M.D., Professor of Experimental Science in the State Military Academy, Charleston.

It must appear strange to professional gentlemen, not engaged in mercantile pursuits, that there should be so much difficulty in determining so simple a problem, as to whether the yellow fever, as it occasionally occurs in Charleston, is the result of foreign intercourse, or of domestic production. A prudent man would admit that both sides of the question are true, if a satisfactory proof was given that at one time it was proved to be introduced, and at another it was proved not to be introduced. When I came into this world, I found the doctrine to be that it was of domestic origin; what the proof was, I never learned. On the occasion of cases of fever, the simple expression was, that "the yellow fever was in Charleston." Where it came from, or how it arose, was never taught. I was left to conjecture whether it came out of the earth, or out of the people, or to seek the first cause, and believe it came from heaven. In this sea of doubt and ignorance I began to have suspicions, and to hope that if I could only determine which of these natural causes produced the disease, the prevention was possible. In 1827, I observed a complete saturation of the earth with water, the disease then prevailing. I then remembered that a different condition of the earth prevailed the preceding summer when the city was healthy; I thought it possible that these different conditions of the earth might have some connexion with the origin of the fever, and published my observations, and as much learning as I could bring to bear on the subject in the *Charleston Mercury*, under the signature of "Makrobiasis." In 1829, the following allusion to this article was made by the Hon.

Stephen Elliott, in his Report to Council, as Chairman of the Board of Health.

"Among the inducements which may have influence with the Council and the public to deepen the drains, if they shall be permitted to continue, it may not be improper to notice an opinion, which has lately been advanced by one of our physicians of much observation and research, that the malignant state of fever has been found to exist in a violent degree only when from the quantity of rain which has fallen the earth has become completely saturated, so that the water appears to rise nearly, or quite to the surface. Persons have indeed supposed themselves able to foretel the appearance of fever in this city, from the height of the water in their wells."

"Our observations on this theory have not yet been sufficient to test its accuracy, and as rainy summers are usually unhealthy, particularly after the rains have ceased, the height of water in our wells may only be the index, not the cause of an unwholesome state of the atmosphere. Still the probabilities are in favour of this opinion, and it would certainly be desirable, if while attending to another object, we could remove the inconveniences which result from a soil saturated with moisture, and so construct our drains as to adopt them not only to carry off the water from the streets and yards of the city, but to act as drains to the earth, and by their constant action to keep the surface of our soil at all times dry." Subsequent observations on this matter induced me to abandon it as an established fact, for I occasionally observed the saturation of the earth and no fever, and at other times I observed the prevalence of the fever, and no saturation of the earth; therefore, as sole cause of the effect of fever, the assumption was untenable, but as an adjuvant to its extension, I have no reason to doubt its efficiency.

About the year 1838, it was established that citizens could visit the country with impunity provided they returned to the city before dark, and steam-boat excursions were frequent to the plantations on Cooper river. A new fact was discovered, and new matter for reflection was presented to my mind. The simple question for solution was, why

remarkable coincidence of character between the four healthy years, and an equally marked difference between the four healthy years and the one unhealthy. I rejoiced in my success, I fancied that I had the key to open the future, and that the origin of the yellow fever in the city was governed by the same laws as that of the country, and both by the laws of vapour, as I had ventured to anticipate in my publication in 1838. The return of yellow fever in 1852, gave me another opportunity to compare healthy and unhealthy years, and I was somewhat provoked to observe that my former results were not exactly repeated. Discrepancies were now apparent. The fever did not appear when it should, and did appear when it should not; 1850 was a malarial year, but very dry; 1852 was an equally malarial year, but very wet; we had fever in 1852 and not in 1850, and I was forced to attribute its presence to the quantity of rain, other circumstances being the same, and I accordingly published this explanation of the matter in this Journal of January, 1853. A reference to the article will exhibit the difficulties I had to encounter to make my first position tenable, and it will also exhibit the doubts that I entertained of ultimate success in establishing the fact, that the origin of yellow fever was dependent upon meteorological phenomena. The following extract will suffice:

"In preparing this article, I wish it to appear that I am seeking for truth and beneficial practical results, not proselytes. I will avoid all logic that tends to make probabilities appear to be truths, and all possibilities realities. Did I wish to obtain proselytes alone I would stop my investigations here. My tables speak for themselves, their accuracy can be tested if doubted, but the results and conclusions cannot be denied. Cool nights, warm days and a high dew point are the meteorological characteristics of a yellow fever summer. Warm nights, warm or cool days and a low dew point, are the concomitants of a non-yellow fever summer. These facts are fully exhibited by the comparisons made for six years. Such constancy is not common to contingencies, and the generality of persons, after six years of careful observation, would suppose that they had the truth and the whole truth. So thought I, until I examined the two healthy years of 1851 and 1850, and was surprised and disappointed to find that they bore a stronger resemblance to the unhealthy year of 1852, than to the healthy years of 1845, '46, '47 and '48: and although I had the truth, I found I had not the whole

and the result bore out the truth of the original supposition. Thus to produce a yellow fever summer, we must have a combination of malarial and pluvial phenomena, filth and population remaining the same. The summer of 1853 was carefully studied as it advanced, and exhibited all the necessary causes in perfection. In its details it was a worse summer than 1849, and if there was any truth in preceding observations, I had a right to expect that similar results should follow similar causes; in other words, if the fever of 1849 had been produced by meteorological causes, the same meteorological causes should produce a fever in 1853. The malarial phenomena were more intense than in 1849. I patiently waited the development of the pluvial which occurred about the first of September. On the first of August I called on the Mayor, and expressed my apprehensions of the dangerous condition of the season and advised care in cleanliness, and the distribution of lime and chloride of lime. That so long as the season was dry there was little danger, but should rains commence, previous observations had demonstrated that the fever was to be apprehended; at any rate it would be prudent to do all that could be done in the way of prophylactics, and diminish the danger at least to that extent. In September I called again, and announced that, in accordance to previous observations, the fever was to be expected, all the conditions were fulfilled, and draining the cellars, &c., was to be accomplished as soon as possible, which was done. This advice was voluntary, and founded upon the similarity of the seasons of 1849 and 1853. If there was any reliance to be placed upon the meteorological condition of the atmosphere in 1849, as conclusive to the development of yellow fever, the same reliance should be placed upon identical phenomena producing the same result in 1853. The summer of 1853 presented worse appearances than 1849 or 1852, and became the test of all my preceding observations. Had the fever been produced, I might have passed for a true prophet, but as it was not I was a false one; and it was not the first time, for I predicted health in 1852, and was disappointed. I am always ready and willing "to pursue with eagerness the phantoms of hope." Others allured by similar phantoms have detected the hidden laws of nature, and on the success of others I ventured to institute a series of observations and comparisons of different years, to determine how far the predominant doctrine was consistent with truth. I had

men. If the local and meteorological condition of the atmosphere in 1849 was capable of producing the yellow fever, the same condition should have produced the same effect in 1853, but it did not, hence we cannot attribute the origin of it to atmospheric causes in 1849. If the season of 1849 produced the fever, the season of 1852, from their great dissimilitude, should not have produced the fever, but the fever did appear, hence another reason for rejecting the productive effect of climate alone. Similarity and dissimilarity of seasons are not accompanied with any fixed result, which can be predetermined by careful and exact observation. The same phenomena produce different results in different years, and different phenomena produce the same results in different years, conditions so opposed to all of our ideas relative to cause and effect, that it would be absolute folly to express a belief that identity of cause produced identity of effect. Who would venture to assert that the light on the earth was produced by the sun, if there was not a constant concurrence between the appearance of the sun and the illumination of the earth? A constant concurrence is indispensable to cause and effect, and where those constant and invariable concurrences do not exist there is no reason to assume that the phenomena stand in the relation of cause and effect, on the contrary we are bound to assume that they do not stand in such relationship. Did the yellow fever prevail every summer in Charleston, as the country fever does every summer in the country, there would be no dispute about its endemicity, as there is no question of the endemicity of country fever, and we would be justified in attributing its origin to the emanations of the soil, organic matter, moisture and high temperature, but its occasional absence confutes this common origin to both diseases, for the common causes are always present in the city while the specific disease is frequently absent, and when present there is no established proportion between the intensity of the disease and the intensity of its hitherto presumed causes. It is a fact well established that if a case of country fever is brought to the city during the prevalence of yellow fever, that the country fever runs right into it, and the patient dies of black vomit and other signs of city fever, while it is well known that no such result takes place if he remains in the country, showing that there is a vitiation of the air in the city different from that of the country. To produce this change in the disease it is by no means necessary that there should be a case of yellow fever in the same room, nor the same house, hence what is called contagion plays no part in this metamorphosis. In the summer of 1830, L. W. S., a native of Charleston, aged 22 years, ventured to pass the season in Christ Church Parish, and took the fever; he was brought to the

city for the benefit of good nursing. Four days after, yellow fever predominating, he died of the latter disease. He resided in Cumberland-street, opposite the Church, and no case of yellow fever was known to be in the neighbourhood, and certainly he had never come in contact with one. I mention this case to show that a more powerful malaria existed in the city than in the country. More powerful, I repeat, because it was capable of transforming another disease into its own nature. That its influence was ærial we have no reason to doubt, but that it was contagious we have every reason to deny. If ærial and not contagion, was there a local cause adequate to produce the transformation? In other words, was there a portion of earth in this vicinity capable of eliminating an ærial poison sufficiently virulent to produce such a result? If there was it was not discovered, for no extraordinary exposure of the earth was made by fires, digging or pulling down houses, things remained as they had been during the preceding summer when it was healthy.

The origin of yellow fever is generally admitted by all parties to be through the medium of the air. I trust that I have shown that it is not the result of meteorological causes operating upon it so as to change its nature from a healthy to an unhealthy condition. Neither excessive nor long-continued heat alone is capable of producing epidemic fever, as may be proved by reference to past years, when the city was healthy during the hottest summers. Excessively wet summers have, occasionally, been healthy; in fact, the strictest comparison of healthy and unhealthy summers, on the most extended scale, have failed to point out the constant existence of those occurrences on which we have a right to presume a priori, that its origin depends. I cannot conceive that the original intent of the then Secretary of War, the Hon. John C. Calhoun, when he ordered meteorological observations to be kept and preserved at all the army posts of the United States, was to confirm preconceived opinions of the origin of disease, but it was rather to furnish the means for future comparison, and establish the truth whatever it might be. The recommendation implied a doubt in his mind of the accuracy of the then prevailing opinions, and the surest way to arrive at the truth was by more extended observations, and more minute comparisons, as well as to determine the sanatory condition of new posts. The establishment of the office of City Register in Charleston, was "to prepare and publish the weekly bills of mortality—make and preserve the meteorological and thermometrical observations—keep a public register of all marriages, births and deaths, which take place within the city—inquire into the cause of all epidemical diseases, and

report the same to the Mayor or Board of Health whenever required." This ordinance was ratified in 1842, and went fully into effect in 1845. We now possess valuable meteorological and thermometrical observations for ten years, and by careful study of them we may or may not, be enabled to establish the truth or falsity of any hypothesis that the human mind may conceive. I have applied these details to my hypothesis, which was the hypothesis that was voted to be the only true one in 1800, and has continued to be admitted to the present time by many intelligent and worthy physicians. I applied them in hope and doubt, alternately both feelings have predominated, but, finally, I had to yield to the power of truth. My pride of opinion was first elevated in 1850, was rendered doubtful in 1852, and humbled to the ground in 1853. In 1854 I was convinced of my error and embraced the opposite side, for the only alternatives of faith were domestic origin or foreign introduction. I laboured to prove the domestic origin of yellow fever as long as the current of observations led me on successfully, but when it turned I also turned, and followed it to wherever it should lead. It has led me to the doctrine of contingent contagion or infection, a doctrine full of comfort and replete with the happy prospect of ultimate success in its practical applications. My change of opinion was based upon the impossibility of reconciling the observed facts with the meteorological conditions. My cherished hypothesis was seen to be erroneous, and I conceived it to be not only folly, but vicious obstinacy, to persist in advocating that doctrine which I could no longer believe. I admit an impossibility in mind to regulate its belief, and if I am an apostate, it was not that I loved my colleagues less, but that I loved truth more. I feel as much indebted to the meteorological records for correcting my erroneous suppositions as I would have done for confirming them and establishing their truth.

When advocating the doctrine of the domestic origin of epidemic yellow fever, we were accustomed to set down as admitted truths several local causes, as fully adequate to produce the effect; among these were disturbances of the surface of the earth by digging, exposure of the surface by destruction of houses by fire, accumulation of stagnant water in low places, popular excitement at elections, crowded population, exposure to the heat of the sun by day and the dews by night, accumulations of filth and bad odors proceeding therefrom, &c. However active and efficient these several causes may be for propagating the disease, they do not appear, on minute examination, to be adequate primarily to engender it; in other words, they do not seem to be sufficiently active and efficient as to be considered endemic causes.

The cotemporaneous occurrence of two events ordinarily suggests to the mind the relation of cause and effect, but that this should be conclusive, the same concurrence should always be observed. If disturbances of our soil is capable of engendering fever at a certain season, we may infer that fever should occur whenever the soil is disturbed at the same season in different years, and that fever should not occur when the soil is not disturbed. Founded upon this view of the origin of yellow fever, there is an ordinance prohibiting all excavations from May to October. This law was based upon observations of the last few years, especially the fever of 1852, being cotemporaneous with the excavations for the custom house. The most prominent instance of a similar coincidence in our country, was the digging of the Carondelet Canal in New-Orleans in the year 1796, and the first appearance of yellow fever in that city.—(*See Thomas and Picornelle.*)

But the fever has continued to appear in that city and no cotemporaneous canals have been dug. In our city we have had several extensive excavations, and no constant concurrence of fever. Commencing with the Custom House foundation of 1852, in connexion with the development of the fever of that year, I have obtained the following details from the records of the progress of the work, which are undoubtedly authentic and may be relied upon. The first excavation on the north wing was commenced on the 8th of January, 1852, forming a pit seventy feet long, thirty wide and ten deep—thus removing 21,000 cubic feet of earth, logs and mud. On the 5th of August, the digging, piling and concrete was finished and the masons were at work, salt-water percolating into the pit all the time, which was removed by a trunk and pumps. In July, inundated for three weeks with salt-water. On the 5th of October, there were 211 persons employed; of these 196 were Irishmen, 3 Englishmen, 1 Pole and 11 Americans. Up to this period fifteen had been sick and five had died—a mortality certainly not indicative of any local cause of disease, especially when we recollect that these labourers slept in other parts of the city, and were exposed to general infection as well as local. If this pit had been the cause of the disease, we should have expected the first cases would have been among them, but we all remember that the first case was a lady on Brown's wharf, foot of Queen-street, and subsequently the disease pro-

the Clara S. Bell, with Mrs. Cole on board, being the first case, was between these two sources of possible infection. The first case of fever among the labourers at the Custom House, was on the 14th day of September, a period too remote to be considered the origin of the epidemic, when Mrs. Cole had died on the 16th of August.

If the excavation of 21,000 cubic feet of earth in 1852 could produce such a calamitous result to the city as has been supposed, we have a right to infer that the same result should have occurred by the continuation of the work in 1853. During the same period of 1853, 143,000 feet of earth were excavated and heaped up on the premises. 202 Irish labourers were employed in August and September, yet not a case of fever appeared. The exemption from disease of the workmen in 1853, is conclusive that the excavation of 1852 had no "lot or part" in the production of the disease of 1852, and the labourers in 1852 only suffered in common with other citizens, from a cause unconnected with their particular occupation, and we have every reason to conclude that the same calamity would have occurred, if the foundation of the New Custom House had never been commenced in 1852.

The disturbances of the earth, as an original cause of endemic fever, is further weakened by the history of the various gas works. In May, 1847, the gasometer pit was commenced in Church-street, and completed in December, 1848. During the summer of 1847, 125,000 cubic feet of earth, wood, shells and clay, were removed and distributed over the city. How many Irishmen were employed in the operation it is impossible to determine as it was a contract, and no record to this effect is to be found in the office. The number was not less than one hundred. In November, 1847, the laying of pipes commenced, and was continued all winter and next summer; and in April, 1848, the city was lighted with gas. Nearly every street was dug into in order to lay these pipes to the extent of miles, yet strange to relate no fever was produced by this profound and extensive excavation. The August and September sun poured its hottest rays upon the heads of at least one hundred Irishmen, strangers to our climate, emerged in a pit ten to twenty-six feet deep, without a current of air to fan their burning bodies; notwithstanding the supposed poisonous exhalations from the earth, not a case of fever was developed. With such an example as this, how can we cling

... .. making an endemic origin to fevers

The gas pit in King-street, near Vanderhorst, was begun in May, 1849, and finished in November of the same year. The work of excavation progressed during the fatal summer of 1849, yet there was no remarkable fatality in the immediate neighbourhood. The nearest case was a German apothecary, Mr. Meyer, who probably took the disease in the lower portions of the city, and brought the germ of it to his home. I say probably, for the following reason:—Mr. M. learned that his pastor, residing in Society-street, had yellow fever, and he determined to visit him. He called at Mr. Paeknin's and induced one of the young men to accompany him. On their return about 9 o'clock, P. M., they were overtaken by a shower of rain and took refuge in the market, where they were detained about one hour. They, finally, returned to their respective homes, viz.: Meeting and King streets. Both of these persons took sick and were buried within twenty-four hours of each other; and, if I dare add another probability, by a common infection taken at the same time and place, that is in the market during the rain. I have elsewhere shown that the fever of 1849 took its origin on Union wharves, and subsequently extended to other portions of the city.

During the summer of 1853, a third gas pit was dug in Cannon-st., and as no fever was observed in the neighbourhood, it is only mentioned confirmatory of the simple fact, that extensive excavations of the earth in Charleston may be executed without involving the neighbours in one common calamity of disease.

I had written thus far, when it was announced that the yellow fever had appeared in our city, and was distinctly traced to importation. If such should be established to be true, my labours in writing will have ended. I need not now examine the other local and general causes to which it has been referred. One cause to account for an effect is sufficient for me, provided that cause be true and sufficient to explain the appearance. The demonstration of the truth of that cause I leave to others. I have "set the watch and the enemy has been taken." By the religious the divine hand will be seen in this simple development of

LARTIGUE on *Lunar Influence*.

ART. II.—*Lunar Influence*. By G. B. LARTIGUE, M.D., of Barnwell District, S. C.

Messrs. EDITORS:—In the July number of your Journal, Art. IV, my attention has been recalled to a subject discussed in my Thesis on “Periodic Vital Phenomena,” presented to the Faculty of the College in your city, in March 1852. In my preparation for the essay, I was much at a loss for statistics, bearing on the alleged connection between the Phases of the Moon and the changes of weather; the table presented by Dr. Johnson would have been very valuable to me.

I design in this article to make some extracts from my Thesis, and probably some additional remarks, confirmatory of the want of the alleged “connection;” followed by a brief discussion relative to the effects of the moon on the tides.

“The doctrines of critical days, septenary periods and lunar influence having in more or less degree their origin in the mysterious philosophy of the Ancient Egyptians and Chaldeans; having been in part re-developed by Pythagoras, and cherished by Hippocrates, they have been alike powerful on the popular mind, and in the studies of philosophers. Even at this day, many who are ashamed to acknowledge the mystic influence of the number seven, count with confidence the septenary periods, and calculate critical days. Dr. Laycock found great assistance in applying the “law of seven” to the explanation of periodic vital changes in the interval between menstrual evacuations. He ascertained that these intervals were “not constantly a lunar month; but two weeks, three weeks, five weeks, and even six weeks.*” Hence he determined to count by *weeks*. It is but natural, when speaking of periods of time less than a month, that the next smaller division should be used. So those authors who write of menstruation, say the interval is *about* four weeks, or a lunar month. But it cannot be supposed that they used the week as a unit of measurement, simply because it contained *seven* days. Velpeau† speaks of the term of 28 or 29 days as most common, but says, ‘in an infinity of people, they (the courses) are observed to recur at nearer or remote periods, sometimes only 22, 20, 18, or even 15 days elapse’ &c., again he speaks of ‘one who is never more than 12 days,’ and ‘others are regular every 32d, 34th or 40th day, and even every 2 or 3 months,’ &c. Dewees, quoted by Ryan,‡ reports a case in which the flow returned every 10th day, from the 12th year to the 40th.

* Vide London Lancet, for 1843 and 43. vol. i. p. 34. † On Midwifery, p. 87.

‡ Ryan's Midwifery, p. 43.

"Davis* in his *Obstetric Medicine*, says "Haller considers the period as being equal to a solar month," also, that 'it is a statement that M. Desormaux (*Dict. de Med.* Tom. xiv, p. 181,) that he has known many ladies who noted in their almanacs, each return of the menses, and that their returns coincided with the corresponding day of the solar month,' but in a great many instances they anticipated it by two or three days. Desormaux also 'believes that women menstruate quite indifferently of all artificial duration of time, as well as any supposed planetary influence with which the function may have been theoretically connected.' Denman and Blundell speak of cases which were not hebdomedal, and Gall found in Vienna women who were regular at intervals of 21, 25, or 26 days.

"These cases are considered to be well authenticated, and give intervals of 10, 12, 15, 18, 20, 22, 25, 26, 29, 30, 31 and 40 days. Neither of these numbers is an exact multiple of seven, and this fact is of itself sufficient to satisfy the mind that seven is not supreme. But popular opinion still says, a *lunar month*, is the proper term, because the most proper one. Granting that this is the most common term, does not establish that it is more proper than any other, but the facts do not warrant any such acknowledgement. Dr. Laycock found that he could not take four weeks as a unit of measure, but had recourse to one week as the unit, which he found better in establishing his general law. There are a greater number of cases, definitely detailed, having intervals of days divisible by seven, than those so divisible, for in the 28 day interval, there is on every side room for confusion. "The common idea is, that the menstrual discharge returns every twenty-eight days, or in other words, that there is this time 'between the end of one menstrual period and the beginning of another.' A practitioner of midwifery, in London, in a communication to Dr. Lyall, asserts that this is a mistake, 'that 28 days include both the period and the interval.'"[†] Now when the flow lasts from 4 to 6 days or more, how if the "common idea" governs, is the truth to be determined? The inquiries of Desormaux alone, on this point, were conducted in a satisfactory manner, and the

..... month. In these

nite and invariable number of days for the interval, and no definite period, a multiple of seven days, at the expiration of which these discharges universally appear.

"The term of Utero Gestation in the human female, was considered by Dr. Laycock to present strong evidence in favor of his law, for determining periodic revolutions. He says that this term, usually forty weeks, has numerous exceptions, but is lengthened or shortened by weeks.* He arrived at this conclusion by considering as inaccurate, the observation of those cases which did not accord with his hypothesis. The determination of the period of utero-gestation, is of great importance as connected with Medical Jurisprudence, and has received much attention. The most eminent physicians of Europe and America, have been called on for their opinions before the courts of law, and on those opinions, the laws of the land connected with the legitimacy of children, have been framed. The opinion advanced have been very contradictory, as the great differences of the laws all nations demonstrate. These differences arise from the great difficulty in determining the moment of conception.

"Four circumstances have influence in determining the reckoning in general, and of these four, one or more must have bearing to make the observation of any case useful in establishing the term. †"They are, 1, certain peculiar sensations experienced by some females at the time of conception, or within a few hours, or a day, or two or more days, after the fruitful coitus. 2. The cessation of the catamenia. 3. The period of quickening. 4. A single coitus. If we review these circumstances, we shall find a certain degree of uncertainty to attach to all. There are some females not conscious of ever experiencing the first; the last is not applicable to married females; while the period of quickening varies sufficiently to render it perfectly nugatory in a calculation to be made like the present. There remains then only the cessation of the catamenia, and this is the point from which most females date the period of conception. The exact time generally taken is the middle period between the last appearance of the menses, and that in which they would have recurred if pregnancy had not supervened. Some, however, calculate from the first week after the cessation.' At a glance, we perceive that observations connected with the catamenial period are worthless, in determining the period, being itself inconstant. But granting that it may be infallible observation has not established the exact number of

between 39 and 40. To point out definitely these variations, reference is made to Dr. Merriman's table.* Very few cases in comparison with the whole number, terminated in an exact number of weeks.

"The solution of the question must then depend on those few instances where only a single coitus took place. The investigation is here to be made among a mass of statements, the truth of which are uncertain. In order to establish the legitimacy of an heir, or to obtain redress for an injury in most cases, they are probably very often false. But in the very few available cases, none can be found in which the period of days were exactly divided by seven."

(The impression now generally received, and very probably the correct one, is that impregnation does not occur at or after connection, but as an accident. The spermatozoa may not be brought in contact with the ovum to impregnate it, until an indefinite period has elapsed after the freeing of the ovum from the ovary, and a like indefinite term after the coitus. But when the impregnation has taken place, the parturient effort, in a due course of nature, will take place at what would otherwise have been the time for a menstrual discharge. This impression, if it should prove correct, will tend to substantiate the preceding and following matter, relative to the uncertainty of a definite period of time, a multiple of seven days, for a natural course of utero-gestation.)

"Uncertainty in regard to the exact period, caused the author of *"Vital Proleptis,"* to extend his study to animals lower than man in the scale of being. Studying a subject, the explanation of which he believed to rest on the science of numbers, it is very strange that he should have been (as he confesses himself,) not particular as to rigid accuracy. He speaks of the terms of utero-gestation, and incubation in one hundred and twenty nine species of mammals and birds as data. Sixty-seven of the whole number exhibiting 'a definite number of weeks or months.' Those alone, the terms of which were weeks, can be used to substantiate his doctrine, for the term 'month' unless otherwise specified, is used always to point out the solar month, which is no exact multiple of weeks. Twenty-four exhibited periods within a day of the definite number, and thirty-nine had the periods so loosely stated, that they were unavailable to him. Hence, of the 129 species, 63 have terms not definite multiples of a seven day period. Of the remaining 66, (67?) those measured by months, bear against his position.

"From this it is evident that the only reliable statement that

the periods of the hatching of ova, he says, 'the shortest is half a week, or seven lunar days, as in the wasp, the common bee and ichneumon; in some as the *Cecidomia tritici*, the period is one week; in others it is a week and a half, &c. In making further statements, he says, 'in the common bee it is six and a half, in the bumble bee seven days exactly.' As regards the common bee the statements are contradictory.

"From such data he concludes, 'that in animals, changes occur every $3\frac{1}{2}$, 7, 14, 21 or 28 days, or some definite number of weeks.' As he found in those instances a period of three and a half days, (which he termed seven lunar days), he resolved the septennial period into 48 hours, instead of 168, as has been generally considered. This in accordance with his opinion, should be considered the unit of time for all calculations regarding periodic vital phenomena. To show that this would be incorrect even granting his premises, we need not refer to the periods of $6\frac{1}{2}$ and 15 days quoted by him which are not multiples of $3\frac{1}{2}$.

"Observations have been made with regard to the period of gestation, definite to a day. Of these, M. Tessier* made reports to the National Institute of such facts as came under his observation for 40 years. He specified the days which 28 cows went with their calves, and of that number five only were delivered at the end of an exact number of weeks. Of eight mares, none foaled in a definite term of weeks, and a similar result obtained in the case of eight cows, reported to Dr. Beck, from very reliable authority.

"These facts lead us to conclude, that neither the week of seven days nor the half of this period, has any important connection with the periodic changes in living beings. Even should it be established, that a *majority* of these changes can be connected with a $3\frac{1}{2}$ or 7 day period, it would be a mere coincidence, and as a scientific fact, would be altogether unavailable.

"The doctrine of Critical Days, as taught extensively by Hippocrates, has already been often discussed. 'Crisis is defined as a sudden and notable change, occurring spontaneously in the course of fever, and exhibiting a remarkable influence on its character and termination.'† This is very generally acknowledged, but the doctrine of critical days is based on a classification of these crises, whence data are supposed to be found for the calculable periods of these changes in general. 'These days, if we collate the writings of the principal and most zealous supporters of the doctrine, are 3d. 4th. 5th. 6th. 7th. 9th. 11th, 13th, 14th.

critical days are the 8th, 10th, 12th, 16th, 18th and 29; six in number, We cannot wonder, then, if as is asserted, a majority of crises do occur on the so-called critical days.*

"In the opinion of those authors who advocate the opinion of critical days, it is evident that it is closely connected with that of septenary periods; moreover, the lunar influence is closely connected with the explanation of both doctrines. This is acknowledged by Dr. Laycock, who says† 'this idea, (that of lunar influence) was conjoined with the doctrine of septenaries, and necessarily so, because the observed vital period of seven days was conterminous with the lunar period of seven days or one week. In the second century, we find Galen discussing this connection between the moon's influence and critical days *in extenso*, and with great ingenuity; and his doctrines revived, but not improved by Actuarius in the 12th.'

"If septenary periods depend on lunar influence, and the conclusions arrived at relative to the former be acknowledged, then the doctrine of specific lunar influence has been successfully assailed; for any theory incapable of practical application, is worthless. But should it be considered that these conclusions are not legitimate, and that the numerous exceptions, to which allusions have been made, could be reconciled if lunar changes, were accurately studied and applied, then it must be shown that they cannot be so reconciled, or a failure made evident in establishing any thing affirmatively or negatively.

"The impossibility of any such reconciliation, is apparent in an examination of the length of a lunar month. Those authors who have advocated these doctrines have been careless in their premises, and have considered (as is done in general,) the lunar month to be twenty-eight days of twenty-four hours each. But truly there are two periods of time, each called a lunar month, with a difference in their length of 2.21 days of 23h. 56m. and 4.09 sec. One of these months, the sidereal, marks the revolution of the moon, on its own axis, and is 27.32 days; while the other, the synodical, marks the time from one conjunction or new moon, to another, and is 29.53 days. As the phases of the moon have no reference to the sidereal, and are the natural divisions of the synodical month, the latter is the one on which the law of septenary periods is based. But neither of these terms is divisible by seven, nor do the phases of the moon recur at exact seven day periods."

should be closely examined, and their truth well tested. To the scientific man, a supposition that meteorological phenomena are governed by some power of the moon, is absurd; yet with a subject bearing with so much importance on an act dependent on observation for its most useful facts, the investigation must be carried a step further, and the results of observation analysed. In discussing* the 'influence of the moon on fevers, and on the spread and duration of epidemics,' after fortifying his position with the opinions of different authors, Dr. Laycock produces a table prepared especially to demonstrate the truth of that influence on Cholera. This table framed from 'the results of 46 instances,' shows that Dr. Orton, in India, had sixteen cases of Cholera, to make their appearance in two periods, each of three days; the middle day of the three being that of the changes of the moon. Thus on the six choleraic days he found 16 of the 59 cases; but in two periods of two days each, which were the second days before and after the full and change, he had 15 cases. These four days were removed further from the syzygies than the the six choice days; and although the proportion of cases that appeared in the four days was much the greatest, it was overlooked. But on the days, the third before and after the full and change, the cases became less in number; this causes the inference 'that the moon's syzygies have a very marked influence in producing disease, and the quarters in removing it.' The obscurity of the table and its explanations, are evident on a slight examination, as is also the absurdity of forming conclusions on such data.

† "Dr. Laycock gives account also of the progress of Cholera in different cities of Great Britain, which he says '*do not exactly corroborate*' Mr. Orton's. The cases from which these tables were made, were 1940 or more in number, conclusions drawn from which were not strong enough in Dr. L's opinion, to cancel those drawn from forty-six cases. These tables are without explanation, and are very obscure. But from over 1100 cases which occurred in York and Glasgow, we perceive that 7.36 cases per diem appeared at the quadratures, the new and the full of the moon, allowing three instead of one day at each phase to make the average; while 852 cases per diem appeared on all days not so distinguished. (The cases at 'quarters' are not considered, for the author must have meant the 'octants' as the moon quarters at the 'quadratures.' If he calculated from the octants, with three days at each, as at the other phases, he would have had twenty-four days of the twenty eight, as days of specific lunar action.) The progress of the epidemic

* London Lancet, 1842 and 43, vol. ii. p. 440.

† Lon. Lancet, loc. cit.

at Manchester, also bears heavily against Mr. Orton's conclusions. The results from 1940 cases must entirely cancel those of 40 cases which occurred in India.

"In considering the influence of the moon on nervous affections and insanity, Dr. Laycock brings forth the force of popular opinion to support his position. The following extract from '*Des Malades Mentales*, par E. Esquirol,'* will be sufficient on this point to negative his conclusions.

"'Has the moon any influence on the insane? The Germans and the Italians believe in such influence; the English and nearly all modern people give the name of *lunatics* to madmen. Daquin de Chamberi, after some observations, concludes that the moon influences these maladies. Some insulated facts, the phenomena exhibited in many nervous diseases, would seem to justify the opinion. I have been unable to examine whether this influence is real, notwithstanding the care that I have taken to assure myself of it. It is true that the insane are more agitated at the full of the moon, in the same manner that all of them are at the break of day. But is it not the light of the moon, that excites them, as that of the day excites them every morning? Does not this light produce in their habitations an illumination which affrights one, which rejoices another, which agitates all. I am myself convinced of this last effect, by closing carefully the casements of some insane persons who had been given over to me as lunatics. Dr. Hutchinson has never perceived this influence during the many years he has remained at the Pennsylvania Hospital, in the quality of resident physician. Haslem has not been more happy at the Bedlam of London; at the hospice of Salpêtrière, where practical truths have become in some measure popular among the inmates of the house, they do not yet suspect the influence of the moon. I am able to say as much of the Bicêtre, and of some particular establishments of the capital.' 'M. M. Leuret and Mitiré have examined whether the moon influences the pulse of the insane, and they pronounce in the negative.'

in all particulars. Changes of some kind can perhaps always be detected. But in that table are recorded many occasions on which no change occurred. On the proportion of changes and no changes, he based his conclusions, but those conclusions would have been more satisfactory, had they been based on an accurate observation of the degree of change.

"M. Arago, very high authority, made observations for 27 years at Munich, Stuttgart, Augsburg, and Vienna, and his conclusions are that *it rains more frequently during the increase, than during the wane of the moon, and the chances of rain are the greatest when the moon is in perigee, or nearest the earth; but the popular notion, older than the christian era, that changes of weather take place more frequently at, or near the change of the moon has not been verified."

"If the moon has an influence so special and powerful, it must be evident through changes in light, heat, electricity, diversity or moisture of the atmosphere. The examinations of these matters must here be most cursory. That the light and heat of the moon are so powerful, cannot be supposed, when brought into contact with solar influence in the same particulars. But few experiments have been directed especially to the elucidation of the point. One, however, has yielded a satisfactory conclusion, viz. 'chloride of silver, the color of which suffers the greatest and most rapid changes by an exposure to light, is not at all affected by the lunar beams, condensed in the focus of a powerful burning glass.'† Electricity has never been in any way shown to have any relation with lunar changes, nor have barometric changes been proved to have such a relationship. The atmospheric tides, as spoken of by Humboldt and others, are only definite in tropical regions, and even granting that they are as marked as the tides of the ocean," and that the moon has a similar influence over them to that over the latter, still neither has observation connected any effects of atmospheric tides with vital changes, nor is there any plausible theory that such connection should exist. La Place says, the changes depending on the ærial tide are too inconsiderable to be detected in the barometer, unless by the most refined observations. Hence it is concluded that the fluctuations produced by this cause are too light, to affect meteorological phenomena in any appreciable degree.‡ "The moisture of the atmosphere is connected with barometric changes, and has much to do with some vital actions. The dew point will answer as a criterion in this matter." (Accompanying the thesis was a table, prepared by comparing the average dew point at

2 o'clock, P. M., on every day of the year 1850, with the dew point of the days on which occurred the new and full moon, the quadratures, apogees and perigee.)" "The conclusions exhibited, show very plainly that the moisture of the atmosphere is independent of the phases of the moon. The changes observable are inconstant, as were those noted by M. M. Toaldo and Arago. Such inconstancy demonstrates the non-existence of a recurring series of atmospheric changes within our cognizance.

"In dismissing the preceding discussion, the conclusions arrived at, are, that the doctrines of critical days and septenary periods, are not legitimate deductions from such influence as the moon appears to possess over natural changes, either in organized or unorganized existences; and that this influence has been much exaggerated in the minds of men."

Though we are satisfied that the moon has had a power ascribed to it, much greater than is its due; owing to the imperfect observations and the superstitions of preceding ages, still where it has been proved to have an influence, that influence should be acknowledged. In the applications of the laws of gravitation, there are none more complete and satisfactory, than "the theory of the tides," as explained by that law.

Your contributor on the "alleged connection," has been evidently led astray in regard to the theories concerning the tides. The work by Keith, of London, in 1815, must be much behind the age in regard to the theories alluded to, and Dr. Johnson has cause to congratulate himself, that the results of his personal observation, tend to correct the errors of the scientific men of past days, and to confirm the more enlightened views of the present time. I trust however, that a presentation of the state of the theories regarding the tides, as they are at this time received, will not be inappropriate.

In treating the matter, I will take it up in the order presented by Dr. Johnson. The uniform recurrence of spring tides, at the new and full moons is to be expected, and pretily exemplifies the propriety of the present theory relative to it. (I introduce some extracts from the latest edition of Olmsted's Astronomy, to which work I will also be indebted

drop of water in its descent to the ground. But if one part fell faster than another, the different portions would evidently be separated from each other. Now this is precisely what takes place with respect to the earth in its fall towards the moon. The portions of the earth in the hemisphere next to the moon, on account of being near to the centre of attraction, fell faster than those in the opposite hemisphere, and consequently left them behind. The solid earth, on account of its cohesion, cannot obey this impulse, since all its different portions constitute one mass, which is acted on in the same manner as though it were all collected in the centre; but the waters on the surface moving freely under this impulse, endeavor to desert the solid mass, and fall towards the moon. For a similar reason the waters in the opposite hemisphere falling less towards the moon than the solid earth, are left behind, and appear to rise from the centre of the earth."

"The spring tides, or those which rise to an unusual height twice a month, are produced by the sun and moon's acting together; and the neap tides, or those which are unusually low twice a month, are produced by the sun and moon's acting in opposition to each other." (Not when the sun and moon are in *opposition*.) "The spring tides occur at the syzygies; the neap tides at the quadratures. At the time of the new moon, the sun and the moon being both on the same side of the earth, and acting upon it in the same line, their acting conspire, and the sun may be considered as adding so much to the force of the moon. We have already explained how the moon contributes to raise a tide on the opposite side of the earth. But the sun as well as the moon raises its own tide-wave, which, at new moon, coincides with the lunar tide-wave. At full moon, also, the two luminaries conspire to raise the tide; for we must recollect that each body contributes to raise the tide on the opposite side of the earth as well as on the side nearest to it. At both the conjunctions and oppositions, therefore, that is, at the syzygies, we have unusually high tides. But here also the maximum effect is not at the moment of the syzygies, but thirty-six hours afterwards.

"At the quadratures, the solar wave is lowest when the lunar wave is highest; hence the low tide produced by the sun is subtracted from the high water, and produces the neap tides. Moreover, at the quadratures the solar wave is highest when the lunar wave is lowest, and hence is to be added to the height of low water at the time of neap tides. Hence the difference between high and low water is only about half as great at neap tide as at spring tide."

With regard to the highest rise of spring tides, Prof. Olmstead says:

"The tides which happen when the moon is in perigee are considerably greater than when she is in apogee; and if she happens to be in perigee at the time of the syzygies, the spring tide is unusually high¹. When this happens at the equinox, the highest tides of the year are produced." The equinoxes occur on the 20th March and the 23d September;—in the spring and in the fall, during which periods the highest spring tides occur, instead of during the winter. That the melting of the snow and ice in both the polar regions has much or any effect in producing these tides, is another question. At the occurrence of the vernal equinox, about the time of the highest tide in the spring, the "summer has not sufficiently advanced in polar regions to produce any effect through the melting of the snow and ice, and some time before the autumnal equinox, about which the other period of the highest tide occurs, the daily efflux of waters from the polar seas must have reached its maximum. But as the efflux is daily, it cannot concentrate its bulk to influence the tides particularly on one day in the spring, nor on one in the fall.

The consideration of the tides of the torrid zone contrasted with those of the temperate zones, is very interesting, though in some degree complex. If the earth was every where covered with a stratum of water of uniform depth, and the ecliptic was coincident with the equator, the plane of the lunar orbit being at the same time coincident with the plane of the ecliptic and equator, then we would truly find the highest tides under the equator and in the torrid zone. But these circumstances do not exist. Though for a moment granting only the existence of the uniform stratum of water and knowing that the sun's path is in the ecliptic, and that the lunar orbit makes a greater or less angle with the equator, it is evident that the highest average tides would be frequently at a great distance from the equator, at the point where the greatest attraction is made by the moon; and that they would be still higher when the attraction of the sun acts in assistance. In Prof. Olmstead's words: "*the declinations of the sun and moon have a considerable influence in the height of the tides.* When the moon, for example, has no declination, or is on the equator, the rotation of the earth on its axis will make the two tides exactly equal on opposite sides of the earth." "The tides are in this case greater at the equator and diminish gradually to the poles, when it will be low water during the

inferior meridian. Therefore all places north of the equator will have the highest tide when the moon is above the horizon, and the lowest when she is below it; the difference of the tides diminishing towards the equator, where they are equal. In like manner, places south of the equator have the highest tides when the moon is below the horizon, and the lowest when she is above it. When the moon is at her greatest declination, the highest tides will take place towards the tropics. The circumstances are all reversed when the moon is south of the equator. The Professor speaks here of the relation between the relative heights of the tides and the position of the moon as regards the horizon, as if the great tide wave fathomed just beneath the moon, which is not the case, as he points out in the proper place.

But as the uniform stratum of water on the earth is only a supposition, it is plain that the influence of the sun and moon cannot be a *complete controlling* power, and that the various depths of the ocean, its currents and counter currents, the barriers formed by the land, and the prevalent winds must be also influential in making the tides as they are. Also it is evident that a large body of water like an ocean, being acted on by an accumulating, and then diminishing force, repeatedly applied, must take time to receive the whole impression of the force, or in other words, to acquire its momentum, and also take a like time to revert through the gradual diminution of the force, to a quiescent state. This is exemplified in the fact that the extra force applied in the production of the spring tides, takes about thirty hours to develope itself.

By the consideration of the influences enumerated as modifying the action of the sun and moon in producing tidal phenomena, one might with a tolerable degree of accuracy shew how the great variety in the time of occurrence, and the height of the tides is produced, and why they appear sometimes in advance of, and sometimes after the moon. But it is needless to carry the discussion further, as any one desiring more full information on the subject, can procure it at the expense of a few hours study.

ART. III.—*Case of Poisoning by Stramonium Seeds, (Datura Stramonium.)* By C. B. FAUST, M.D., Buford's Bridge, S. C.

Messrs. Editors,—I submit to you the following Case of Poisoning, by *Stramonium Seeds*, and if you consider it worthy of a place in your valuable Journal, you are at liberty to insert it:

On the 23d of June I was sent for in haste to see a child of Mr. C———'s, aged one year and eight months; the distance being about a mile, I was not long in reaching the spot.

On my arrival, I found the child laboring under the following symptoms: the whole body of the child was of a scarlet red, skin hot and dry, tongue redder at the apex than at the base, and dry, fauces almost white and dry, respiration somewhat accelerated, pulse full, but not preternaturally frequent, face somewhat swollen, pupil enormously dilated and insensible to light, the muscles of the whole body convulsed, and the child screaming almost incessantly.

At about half past one o'clock (a few minutes after my arrival) I administered the following: calomel grs. v, jallap grs. v, to be followed in a few hours by a dose of ol. ricini; I then had warm pediluvia ordered, and also cold water poured continuously upon the head; after using these means for twenty minutes, the child dropped into an apparent sweet slumber, and slept about twenty minutes.

During its repose my suspicions were excited as to the cause of all the distressing symptoms my little patient was laboring under, and I began making inquiries what it had been doing, where it had been, what it had been eating, &c.; the parents informed me that it had been out with two little negroes at play, in a patch of Jamestown weed. I then informed them of my belief as to the cause of all the distressing symptoms in the little sufferer.

In about twenty minutes from the time the child went to sleep, it awoke, and was like a mad child; it would hold out its hands, as though it wanted a glass of water, then bring them to its mouth and sup as if it was drinking water or other fluid; it would put its fingers into its mouth and even bite them, and also its mothers fingers whenever she put them about its mouth, also screaming incessantly. I had the cold water continuously applied to the head, and administered ipecac grs. v, (as I had no sulph. zinc with me) followed by warm drinkse very few minutes.

I continued this for about one hour, but could not produce emesis. I then went to my office, a distance of a mile and a half, to obtain sulph. zinc, thinking that I could produce free emesis sooner than with the ipecac; I was not long in going and returning, and when I returned I found that no vomiting had ensued, although grs. x had been given it, grs. v having been given during my absence.

I then administered sulph zinc grs. v, followed by draughts of warm water, for fifteen or twenty minutes, and again administered sulph zinc grs. v, in the course of fifteen or twenty minutes longer; vomiting com-

menced, and there was thrown up about twenty Stramonium seeds, intermixed with some indigestible matter of the stomach; I endeavored to keep up vomiting by warm drinks (which the child took very reluctantly) and irritating the fauces, until it had thrown up fifty-five seeds.

At this time, (which was about 5 o'clock, P. M.) the vomiting had ceased, although ineffectual efforts were made, and my efforts in reproducing it, seemed to be unavailing. I had a mustard sinapism to the epigastrium, and the cold water assiduously applied to the head, which seemed to compose the little sufferer more than any thing else.

At 9 o'clock, P. M., my little patient went to sleep again; the pulse at this time natural, respiration natural, skin warm, and not so red as before, pupils still dilated very much; but previous to going to sleep the patient was much more composed.

I then left the following powder and took my leave; calomel grs. v, jallap grs. v, to be followed at 5 A. M. by a dose of ol. ricini, thinking the child would wake in two or three hours, with a promise to return the next morning.

At 7 o'clock in the morning on my return I found my little patient better than I anticipated, and was informed by the parents that they had to wake it to give the medicine I had left; and when they had given it to him he soon went to sleep again, and did not wake until morning, when they administered the castor oil, but as it had had no effect on the bowels, when I arrived, I ordered another dose, and another, if it did not operate by 9 o'clock.

As I have remarked above, I found my patient much better than I anticipated, although the pupils were very much dilated yet, and it could not yet walk; in attempting to walk it would stagger like an intoxicated man and would fall, although rational; at 8 o'clock, I took my leave, with a promise to return at 2 o'clock, P. M., When I returned I found my little patient running about, and was informed by the mother that the medicine had operated well (I may say here that it took three table spoonfuls of oil to move the bowels,) and had brought away at least a table spoonful of stramonium seeds.

The child improved very rapidly indeed from the first operation on the bowels. In twenty-four hours from the time he was first taken, almost every symptom had disappeared, and the little fellow was at his usual puerile occupation.

It is strange to say that neither of the two little negroes that were

with this little boy got poisoned, one of them being a little older, the other a little younger.

This case is not without interest, (at least so to me.) In the first place, the scarlet redness of the skin, which I have never seen mentioned by any author whose writings I have perused. In the next place the recovery after such an enormous quantity of seeds had been taken into the stomach; and thirdly, the very great insusceptibility both to emetics and to cathartics.

Taylor mentions the case of a little boy two years old, poisoned by sixteen stramonium seeds, (Taylor's Medical Jurisprudence.)

Barton tells of two British soldiers who ate of the plant by mistake, one of whom became furious and ran about as if he was mad; the other died with symptoms of tetanus, (Barton's Medical and Physical Journal, vol. 1.)

ART. IV.—*Cases of Inflammation of the Cervix Uteri, (concluded from page 311 of the volume.)* By JAMES M. GREEN, M.D., Maccon, Geo.

A LADY aged thirty, the mother of several children, had been treated by me for inflammatory disease of the cervix and cured; after which pregnancy occurred and delivery at full term. Two years later she was attacked with the same disease, marked, however, by a very different train of symptoms. In the meantime she had been the subject of a severe cardiac inflammation, which was supposed by her physicians to have been subdued without the production of any perceptible organic changes in the heart or its appendages. At least she recovered a good share of health, and the heart seemed to have regained its natural tone and action until she met with an abortion, accompanied by pretty severe flooding, and followed by a rapid deterioration of health. In the first attack she had slight lumbar and right ovarian pains, with some feeling of pelvic pressure, a great deal of urinary irritation and white and yellow vaginal discharges. In the second, although there was a greater amount of disease, there was no urinary irritation, pelvic pressure, ovarian or lumbar pains, or any other evidence of local irritation, save a moderate leucorrhœal discharge, occasionally streaked with blood; but her heart gave evidence of its sympathetic connection with the uterine

organ in a very marked and decided manner. The patient had frequent attacks of disordered feeling about the heart, more commonly manifested by a sense of impending dissolution, at other times by a sinking, trembling, and other disordered feelings in the cardiac region, and occasionally instead of these she had palpitation, but never very violent. These symptoms several times assumed a very grave aspect, but were generally promptly relieved by subjecting the patient to the influence of opium, and creating some counter-irritation over the chest. Upon making the necessary examinations, the anterior lip of the cervix was discovered to be a good deal enlarged and indurated, while its posterior surface was the seat of a very irritable and disagreeable looking ulceration, which extended some distance up the cervical canal, and in a lesser degree upon the internal surface of the posterior lip. The os was enlarged into a long transverse slit, from which there exuded a glairy mucus mixed with purulent matter.

When this patient was subjected to treatment, she did not improve with any thing like the same regularity and rapidity that she did in her previous attack. The first two or three weeks of treatment the ulceration went on improving favorably, and with this improvement in the cervical disease a similar one took place in the cardiac symptoms, only far more rapidly, as she had no bad feelings about her heart after the first two applications of nitrate argt. until the ulcer began to grow worse again. Consentaneously with the increased inflammation and irritation of the cervix the heart symptoms re-appeared, and were productive of the greatest suffering and distress. So convinced was the patient of the intimate connection between the uterine disease and the cardiac symptoms that she anxiously awaited the weekly cauterizations, declaring that she became aware of their beneficial effect in a few hours by the increased tone and vigor of the heart's pulsations, and by the subsidence of the sinking and dying feelings about that organ. This happened so often and so regularly that she said there could be no mistake about it. After several severe returns of the cardiac symptoms, this patient recovered her health under proper local and general treatment, periodical cauterizations, opiates, tonics, counter-irritants, &c. Her strength was, however, not completely re-established until after a trip to the North, where a cooler climate and the thousand and one revulsive and curative influences connected with a change of climate and surrounding associations, dissipated all the nervous remainders of her disease.

Some writer has compared the effect of a new discovery in science to

that of the introduction of a candle into a dark corner of a partially illuminated room, in dissipating the shadow and obscurities that surround the various objects in it; in bringing into proper relief their different parts, and in showing their just relation to each other. Eminently has this been so with the great discoveries of Bell and Hall in neuralogy, of Lænnec in chest diseases, and of Recamier in disorders of the womb. In none of these instances has the practical effect been greater, or the illumination so brilliant, as in the invention of the uterine speculum, for we use the splendid beams of the great day-god himself to alleviate and cure the pangs of suffering humanity. What floods of light have been thrown upon the innumerable pains, aches, debilities, perversions, abortions, discharges and hæmorrhages, that afflict the female sex? How plainly we can see the connection between the cause and the effect, and what is of still greater consequence, relieve them?

The following observations contain, it is believed, a brief bird's-eye view of the most prominent points in the nature and treatment of inflammation and ulceration of the uterine neck. They are thrown together without much regularity or connection, and though with little pretensions to originality, have, with the exception of a remark on bilious congestion, been drawn from my own experience. As the true nature and proper treatment of these diseases is still a subject of controversy, both in Europe and this country, it is considered not improper to throw upon it the light of any additional observations that may corroborate or correct those of other and preceding writers. The great work of Dr. Henry Bennett, of London, will, no doubt, long continue to be the leading authority upon this branch of medical practice, and although it is difficult to discover any thing new in a field which has been so thoroughly explored by so able an observer, yet I do not recollect that he has noticed the influence of catarrh in producing or in increasing cervical inflammation, or given a due degree of prominence to the powerful agency that diseased cervix sometimes exerts in exciting functional disease of the heart.

It will be seen that I have made frequent use of the much controverted term *ulceration*, I shall continue to do so, believing that real ulcerations frequently occur on the cervix uteri, and that the minute histological distinction that some have attempted to draw between the irritating, bleeding, pus-discharging granulations of the so-called abrasions and true ulcerations of that body, have very little or no practical application to the subject.

Of Inflammation and Ulceration of the Cervix Uteri.

From a variety of causes, the principal of which are the periodical congestion which precedes, accompanies and follows the menstrual flow, and the contusion, laceration and rough usage its tissues are subjected to during parturition, and the inflammatory and acrid discharges that succeed it, the *cervix uteri* from the os to the internal coarctation or stricture, seems to be more liable to inflammation resulting in abrasion, granulation and ulceration, than any other part of the body, the pharynx excepted.

Ulcerative or ulceroid disease of the uterine neck produces the same effects upon the adjacent tissues as an ulcer situated upon any other part of the body, namely—irritation, tumefaction, induration, diseased discharges, &c. These conditions may be conjoined in every degree and variety of combination. In a certain number of cases there will be tumefaction without hardening; in others, induration and tumefaction together; and, in other cases, induration without apparent increase in size.

In a great majority of cases the disease commences in the mucous membrane about the os, and extends upon the exterior of the cervix, or up the cervical canal, or both, &c. But, in a certain number, interstitial inflammation and hypertrophy at some point precedes, which when it reaches the surface is accompanied by ulceration.

Ulcerations of the cervix uteri present every variety of appearance, from a small bright abrasion to the large purple fungous ulcer of pregnancy, in which the granulations are larger than a pepper-corn and suspended by a pedicle. I have frequently seen small sized ulcers depressed below the surface of the mucous membrane, precisely like those of the tongue and lips.

Inflammation of the uterine neck, accompanied or not by hypertrophic development of its texture, very seldom extends to the body of the organ, &c. When this does take place, my observation would corroborate that of Dr. Bennett, that the posterior half is the one most generally affected.

In a hundred cases of inflamed and tumefied cervix, the enlargement would probably be much greater in the anterior lip in sixty; in the remaining forty, there might be thirty of enlargement of the posterior lip, and the rest might be equally divided between equable and lateral

Simple inflammatory tumefaction may be very rapidly developed; thus at one time it may be slightly marked, and a few days subsequently the swelling of one lip may be so great as completely to hide the os and other lip, and entirely prevent an exploration. This sudden tumefaction may be caused by injudicious exercise, as a long ride or walk, particularly if immediately after cauterization, and sometimes seems to come from no appreciable cause.

Dilation of the os is almost an invariable accompaniment of ulceration of the cervix, because it is almost always indicated, but occasionally a case will be met with where the disease is situated upon the mamillary portion at a distance from that orifice, or at the upper part of the canal, and where the os will not be dilated. Also, rare cases of very great and chronic hypertrophy and induration, in which the hypertrophic development takes a centrifugal as well as a centripetal direction, and closes up the orifice entirely.

In almost all instances, as the case progresses towards a cure, the the os and cervical tube grow smaller and smaller, until they reach the natural size, but in such a case as is referred to in the last observation, os dilates as the hypertrophy is removed, but afterwards diminishes again.

The ulcerative inflammation and the dilatation of the cervical tube, seems to be always bounded in its upward extension by the coarction, which does not dilate in these cases. But in acute inflammation of the mucous lining of the cervical and uterine cavities, this contraction or os internum does not dilate, and will admit the passage of a large sound.

Leucorrhœa or whites, which is a generic name for all vaginal discharges except blood, among females, is an almost invariable attendant upon inflammatory disease of the cervix.

A thick, creamy or butter-milk looking secretion, which seldom passes externally, but may often be with on the parts by separating the labia, is merely an exaggeration of the natural vaginal mucus.

A thick, ropy, transparent string, sometimes rendered opaque on the outside, by contact with the vaginal mucus; either protruding from the vagina, or broken off in lumps, on the protecting napkin, may be at once attributed to inflammatory disease of the os and cervical canal. It is an exaggerated secretion of the mucous follicles of the cervical canal.

When fluid pus is met with exteriorly, or on introducing the speculum, we may confidently expect to find ulceration of the uterine neck.

Blood, at various times and occasions,—after undue exercise, coitus, &c.—is a very common ingredient in these discharges.

These different secretions and discharges are met with in various degrees of intermixture and combination.

They are sometimes not seen exteriorly, in consequence of the small amount secreted, or its absorption in its progress through the vaginal canal. They are also, occasionally, mechanically retained for a long time, in the parts, by great enlargement of the anterior lip combined with prolapsus. This is also apt to be the case in women who have very strong, well-developed sphincters vaginal, and in these instances, and in those last referred to, the discharges become putrid and offensive. Disease—producing emanations radiating from the inflamed cervix, develop symptoms in the most distant parts of the body, and those that would seem least likely to be sympathetically affected from this point.

Various perverted states of the intellectual manifestations, from a marked degree of eccentricity to complete aberration of the mind—both in the married and virgin relations—depending upon inflammatory disease of the cervix uteri, are met with. The relation between the two conditions, is shown by the mind getting well, when the local disease was cured: *mens sana in corpore sano*. Some of the most astonishing delusions of the senses occur in these cases.

Every form of head-ache may be found in this disease;—as the sensation of an iron band round the head; intense pain in one spot, &c., &c.

Many disordered and impressible states of the cardiac nervous system, rendering the heart liable to palpitation, feelings of dissolution, sense of sinking, &c., &c., of an extraordinary and alarming character.

Among the most frequent forms of functional disorder, excited by diseased states of the cervix, are the various forms of gastric embarrassment and dyspepsia, anorexia, &c.

Congestion of the liver, with bilious vomiting or jaundice, is, sometimes, met with in this disease.

Constipation of the bowels is a very frequent attendant, and, also, alternate states of constipation and diarrhoea, and sympathetic states of the abdomen.

Disordered states of the kidneys, accompanied by weight and heaviness of the loins and by an acrid and irritating urine, loaded with lithates (and less frequently with the alkaline) deposits, are frequently met with in diseased conditions of the cervix uteri. It is difficult sometimes, in such cases, to say, whether the disorder springs from the general disturbance of the genito-urinating system, or from the secondary

by frequent, difficult, or painful irritation, is very commonly experienced at some stage of the disease.

Inflammation of the mucous membrane, as well as the spongy substance of the urethra, are sometimes seen, in these cases, forming an intensely sensitive oblong tumour in the upper part of the vagina, obstructing the passage of the speculum.

Pains and debility in the loins; pains in the hypogastric, inguinal, and ovarian regions, are the most common sympathetic pains of inflamed cervix, but they are frequently met with in the epigastric, sternal, cardiac, and sternal regions, and in the thighs.

In a good many instances the passage of the foetal mass through the rectum, produces great increase in the pelvic uneasiness, and nausea. This may proceed from functional disorder of the rectum itself, but more frequently from pressure upon the irritable and inflamed cervix.

In some instances there are some of the characteristic symptoms, (the leucorrhoeal discharge, which is least seldom absent, being absolved in the vagina), and we recognize the disease solely by its influence on the general health, debility, sallow complexion, emaciation, anorexia, cardiac trembling, anemia.

The functionally disordered organs react upon each other, and upon other organs, thus increasing the general disturbance of the system. In functional disorder of the heart, proceeding from inflamed cervix, the writer has several times met with well-marked asthma. Functional disorder of the stomach, proceeding from the same cause, mostly expends its pathological reaction upon the head. As, in disordered states of the kidneys, it will be difficult to decide between a secondary pathological reaction, and a direct influence proceeding from the diseased part itself.

While inflammation and ulceration of the cervix seldom, of itself, brings about an immediate fatal result, it certainly prolongs, renders more violent, and occasionally renders fatal, other diseases arising in the system.

The cervix uteri, when in a diseased state, occasionally becomes subject to the influence of catarrh,—the inflammation and tumefaction, with all the dependent irritations, increasing, in a very notable manner, whenever the patient catches cold.

Inflammation, ulceration and induration, are among the most common causes of sterility.

Chronic hypertrophy, with induration of the cervix, is an effectual barrier to pregnancy. On the contrary, pregnancy frequently takes place in severe cases of ulceration, without hypertrophic hardening; but,

if left without treatment, always produces abortion or premature labour, with dangerous hæmorrhage.

Occasionally, after the ulceration is cured, and the part is reduced nearly to its natural size, and is of a pale colour, the cervix will continue, for months or years, in a chronic indurated state, with a small os and discharging very little mucus. In these cases pregnancy will not take place, until the induration is removed.

Inflammatory disease of the cervix has a powerful effect in aggravating the nausea, and other distresses of pregnancy.

In non-menstruating females, (either from pregnancy, or past age,) adhesion of the os is a most uncommon result of the cure of ulceration, and is not a very unfrequent cause of tedious labour.

The greatest differences are found in the sensibility of the cervix, when diseased, in different persons. In the major number little suffering is experienced from the application of caustics, in others, the acutest sensibility is manifested, and, in other cases, it seems to have none at all, bearing the roughest usage with impunity. In the pregnant state the sensibility is very slight.

A relaxed and flabby state of the vaginal tube is, almost always (to a greater or less degree) associated with chronic disease of the neck of the womb. Sometimes only the upper half of the canal loses its tone and dilates, which, then, is constantly filled with muco-pus and other secretions, bathing the prolapsed cervix.

A certain degree of prolapsus is a not unfrequent accompaniment of inflamed and ulcerated cervix, but by no means seems to run any parallel course with the amount of disease existing in that organ—great degrees of inflammation and swelling frequently being present—without prolapsus and lesser degrees associated with it. Different degrees of flexion, both forwards and backwards, are occasionally associated with diseased cervix. The latter, more frequently, being present.

The menstrual flux is modified, in various ways, by inflammatory disease of the cervix. Sometimes it is increased in frequency and quantity; sometimes absent for one or more periods, then very profuse, and occasionally it is all the time very scanty. Almost always the local and general symptoms are exaggerated, during the entire period of menstrual congestion.

Treatment.

There is no treatment whereby confirmed ulceration of the cervix uteri can be cured, except the local application of cauterizing agents to the diseased parts.

The most generally useful of these agents is the nitrate of silver, which of itself is sufficient for the cure of many cases. In a large number, however, its attractive effect is not great enough, and we have to substitute the acid nitrate of mercury. A good general rule is to apply the solid nitrate to the ulcerated surface every four to seven days, as long as it seems to be healing. When the healing process stops then make one application of the acid nitrate of mercury, and notice the result; if favourable, return to the weekly touchings with nitrate of silver, until a similar necessity should arise. In cases of a little greater obstinacy make alternate weekly application of the two agents.

In some cases the eschar is thrown off sooner than in others, and an earlier re-application of the caustic is consequently required. Again, in some a longer interval is required for the production of the best effects. When the ulceration ceases healing under the periodical cauterizations, it is advisable to stop treatment for two or three weeks.

In old chronic cases of indurated hypertrophy, the disease will occasionally be found to resist the influence of these milder remedies. In these we shall be obliged cautiously to resort to the caustic, lime or soda. The resolute effect of the potential and actual cauteries, in removing chronic uterine indurations that bid defiance to all other methods of treatment, is among the most brilliant results of Recamier's discovery.

The only mode of using these remedies safely is through a speculum. Among the best and most convenient forms of this instrument is the common conical glass speculum, covered with amalgam and painted black. If made tolerably thick there is no danger of their breaking in the vagina, and they expose the parts very perfectly to view.

Ulceration of the cervix uteri may be treated with more certainty and success during the pregnant state, from a disposition in nature to cure the disease at this time and remove the inflammatory hardening, thus preparing the way for delivery.

In treating cases of ulcerated cervix with hypertrophy, we should never cease treatment until every trace of induration is removed.

One or two cauterizations frequently produce a very marked improvement in the feelings of the patient, when little apparent change has taken place in the ulcer itself. Though, of course, some vital changes must have occurred in its condition modifying the decreased emanations.

Sometimes during the progress of a case the anterior lip, which may have been more inflamed and ulcerated than the posterior, suddenly heals up and the posterior lip becomes swollen and inflamed, and vice versa.

Sometimes when the case is apparently getting well under treatment, the ulcer will suddenly assume a redder hue and more irritable look, and increase in size, with a corresponding increase of the surrounding tumefaction and all the dependant irritations. On careful examination, this may, at times, be traced to the irritation of rough treatment, or too much exercise.

Some writers have repudiated the use of vaginal injections, but their inefficient use in washing away the impure collections of acrid secretions, in the upper part of the canal, must have a healing and healthy influence. The free contact of cold water with the inflamed and irritable cervix and vaginal mucous membrane, must exercise a similar influence.

The writer has not found cases of ulceration associated with any ordinary degree of flexion, backwards or forwards, or prolapsus, to require any other treatment than that adapted to cure the cervical disease, and to restore the contractile power and strength of the vaginal tube.

In some instances, where the general health and the functional disturbances did not give way from the removal of the local disease, excellent results have been realized from the use of Hyd. Potass. nad Sarsaparilla Syrup, and still more from that admirable eutrophic Cod Liver Oil.

In irritable and inflamed conditions of the vagina, which frequently accompany cervical inflammations, laudanum and morphine injections, allowed to remain sometime in the canal, are very useful.

In cases attended by much pelvic suffering, a laudanum enema, administered every night, has the best effect in relieving the local suffering, as well as the pains in the loins and other parts excited by it.

Confinement to bed should only be ordered when the ulcer is in a very irritable condition, and motion seems to aggravate the disease. Continuance in the recumbent posture rapidly impairs the appetite and the general strength.

Where the urine is red, scanty and loaded with red deposits, great improvement will result from the use of alkaline remedies, (an excellent prescription is Carb. Potass. gr. 20 ; Calcined Magnes. gr. 10 ; Pulverized Rhubarb gr. 5, every night,) followed, after some weeks' use, by some of the terebinthinate remedies.

Where the neuralgic pains continue lurking about the loins and pelvic viscera after the ulceration is cured, they may frequently be entirely removed by one or more decided vesications to the loins.

When the urinary deposits are white and alkaline, the patient will be benefited by the use of the acid alteratives, nitric or muriatic acids in proper doses.

When the stomach is very much impaired in tone, and deranged from the long continuance of the functional irritation, it will be benefited by one or more blisters to the epigastrium.

In closing these remarks upon inflammation and ulceration of the neck of the womb, the writer thinks proper to observe that although the treatment necessary for the removal of these grave lesions does not require any peculiar and inscrutable *tactus eruditus*, yet it does require a great deal of care and caution, and some experience in the treatment of uterine disease. The application of strong reagents to this delicate and important part, necessarily involves delicacy of touch and discretion, particularly the use of the potential caustics, for the removal of chronic indurations. The latter should not be attempted by the tyro in uterine disease.

I subjoin a tabular statement similar to the one appended to Dr. Bennett's treatise, showing the age, menstrual peculiarities, social state condition of the cervix, prominent symptoms and result, in each of the cases in which I have taken notes.

No.	Age.	Menstrual Peculiarities.	Social State.	Condition of Cervix and Vagina.	Prominent Symptoms.	Result and remarks.
1	23	Natural.	Married at 18; one labour.	Small ulcer passing into dilated canal; cervix not tumefied or indurated.	Menses disordered; sometimes twice a month and sometimes absent; leucorrhœa, pains in loins and pelvic bearing down.	Cure by one cauterization.
2	24	Uterine disease commenced at 1st menstruation in fifteenth year. Painful and attended by sp'ns.	Married in 19th year; two labours.	Ulceration; anterior lip enlarged, indurated; os dilated vaginitis.	Anemic, debility, cardiac, hypogastric, lumbar and right ovarian pains, pelvic bearing down, urine intolerably offensive, frequent hysterical fits.	Cure after five months treatment. This patient has had two gonorrhœas.
3	20	Natural.	Married in 17th year; three abortions; pregnant.	Tumefact'n, large rough ulcer dilated os.	Epigastric, hypogastric and lumbar pains, pelvic weight, leucorrhœa, sars and floodings.	Cured in 3½ ms. treatment before delivery. Patient became pregnant again and had a healthy child.
4	35	Commenced in 14th year. Profuse and painful before marriage; after, profuse but not painful.	Married in 15th year; eight labours.	Ulceration, tumefaction and induration.	Emaciation, debility, lumbar, ovarian and hypogastric pains, menstrual draining for two or three weeks, leucorrhœa.	Cured after a yrs. treatment; twice salivated by acid nitrate.
5	32	Commenced in 15th year; natural.	Married in 19th year; five labours.	Great tumefaction, induration, ulceration, dilated os.	Debility, emaciation, intense nausea, profuse floodings, rapid enlargement of abdomen, <i>absolutely none</i> of the usual uterine or reflected pains.	Cure in 2½ mos.; of hydatids.

No.	Age.	Menstrual Peculiarities.	Social State.	Condition of Cervix and Vagina.	Prominent Symptoms.	Result and remarks.
626		Menstruated at 14; always profuse, more so after marriage in 15th year; one abortion.	Married in 15th year, one abortion.	Hypertrophy, induration, ulceration; polypus of cervical canal.	Anemia, profuse floodings, cephalic, lumbar, ovarian and hypogastric pains, leucorrhœa, dyspepsia.	Removal of polypus and scion of ulceration, but no benefit to flooding or improvement in health.
718		Always profuse, commenced at 15.	Married, one birth.	Cervix ulcerated, tumefied, os dilated.	Debility, lumbar and hypogastric pains, leucorrhœa.	Cure in two mos. with adhesion.
830		Natural.	Never been pregnant.	Some tumefaction, ulcer entirely intra-cervical.	Leucorrhœa, debility, anemic hæmorrhages, slight lumbar pains.	Rapid cure.
936		Natural.	Married, three births.	Ulceration, great tumefaction and embarrasment, induration, os contracted.	Debility, gastric embarrasment, headache, lumbar and left ovarian pains, pelvic pressure, increase of pains and nausea in defecation.	Perfect cure after a year's treatment.
1030		Natural.	Married at 18, one birth in 20th year.	Tumefaction, induration, ulceration. os dilated.	<i>Never had any</i> of the usual p's., symptoms entirely cardiac, some debility.	Cure in seven months.
1138		Natural, commenced at 19.	Married, four births.	Ulceration, tumefaction, induration, dilated os.	Profuse menstruation, leucorrhœa, lumbar pains, hypogastric pressure.	Cure in six months treatment.
1219		Natural.	Married, two births.	Ulceration of cervix, but little or no hypertrophy.	Nausea, headache, feeble, emaciated, leucorrhœa, lumbar pains, pelvic bearing down.	Cure in three months, became pregnant after.

No.	Age.	Menstrual Peculiarities.	Social State.	Condition of Cervix and Vagina.	Prominent Symptoms.	Result and Remarks.
13	20	Natural.	Married, two births, six mos. pregnant.	Cervix tumefied, ulcerated; os dilated, ulcerated, some vaginitis.	Threatened abortion, lumbar p's., no vaginal discharges.	Cure of ulceration without abortion & with occlusion; child born at the time but dead; large vaginal tumour.
14	34	Natural.	Married, four births.	Cervix tumefied, indurated and ulcerated, os dilated.	Cephalic, lumbar and hypogastric pains, pelvic bearing down, leucorrhœa, gastric embarrassment, great debility.	Cure in five months, pregnancy afterwards.
15	25	Natural.	Married, four births, two abortions.	Ulceration.	Leucorrhœa, lumbar pains, pelvic bearing down.	Spontaneous cure with adhesion to latter end of pregnancy.
16	20	Natural.	Married in 19th year.	Ulceration, dilated os, indurated.	Leucorrhœa, lumbar, hypogastric and ovarian pains.	Cure in two mos. pregnancy afterwards; consumption fatal after delivery.
17	38	Natural.	Married, never has been pregnant.	Ulceration, tumefaction.	Leucorrhœa, lumbar, right ovarian pains, debility.	Cure, tedious tumour in right iliac.
19	25	Natural.	Married, three labours, pregnant.	Ulceration, tumefaction, dilated os, vaginitis and euthritis.	Leucorrhœa, intense pain in right loin and iliac region, vesical irritation, fetid.	Cure prevented miscarriages.
20	45	Natural.	Married, fifteen	Ulceration, tu-	Leucorrhœa,	Cure, with oc-

No.	Age.	Menstrual Peculiarities.	Social State.	Condition of Cervix and Vagina.	Prominent Symptoms.	Result and remarks.
21	25	Natural.	Widow, one birth.	Cervix ulcerated, tumefied, indurated, os dilated.	Leucorrhœa, lumbar, hypogastric and right ovarian pains, palpitations and other cardiac symptoms.	Cure rapid.
22	48	Natural.	Married, sixteen births, five abortions and miscarriages.	Cervical canal ulcerated, anterior lip tumefied, os dilated.	Emaciated, feeble, gastric embarrassment, profuse hæmorrhages; none of the cervical p'ns.	Cure in a month and a half.
23	26	Natural, except lumbar pains.	Married, four births.	Ulceration, some tumefaction and induration, os dilated.	Vesical irritation, leucorrhœa, right ovarian pain and slight lumbar.	Cure in a month and a half.
24	30	Natural.	Married, three births, youngest ten years since.	Ulcerated, indurated and tumefied.	Irregular three years and painful, leucorrhœa, left ovarian and lumbar pains.	Cure.
25	33	Natural.	Married, one child at 28.	Ulceration of cervical canal, tumefaction and induration.	Debility, emaciation, leucorrhœa, constant left ovarian pain.	Cure, disease returned in one year and now under treatment.
26	40	Natural.	Married, eight births.	Ulceration, tumefaction and induration, os dilated.	Debility, emaciation, leucorrhœa, mental delusions, menstruation profuse and painful.	
27	40	Natural.	Married, several children.	Ulceration, tumefaction and induration.	Menstruation early und painful, leucorrhœa.	Cure.
28	47	Natural.	Married, seven	Ulceration of	Pains in the ova-	Rapid cure.

No.	Age.	Menstrual Peculiarities.	Social State.	Condition of Cervix and Vagina.	Prominent Symptoms.	Result and remarks.
29	20	Natural.	Married, two births.	Cervix tumefied and ulcerated, os dilated, vaginitis.	Pain in loins, hypogastrium and right ovarian discharges streaked with blood.	Cure.
30	17	Natural.	Married, one birth and one abortion.	Cervix ulcerati'n, tumefaction, vaginitis, swelling and tenderness of whole uterine body, os dilated.	Lumbar, hypogastric and right iliac pains, leucorrhœa.	Rapid cure.
31	43	Always irregular; stop two or three months at a time.	Married, several children.	Cervix ulcerati'n, tumefaction and induration, vaginitis.	Dysuria, leucorrhœa, lumbar and right iliac pains, pelvic pressure.	Cure.
32	35	Natural.	Married, two abortions.	Cervix ulcerated, tumefied, indurated, os dilated, but filled with granulations.	Pain in loins, both ovarian region and in lower abdomen.	Benefitted by a short treatment, but would not continue so.
33	32	Natural.	Married, three births.	Cervix ulcerated, tumefied and indurated, os dilated.	Lumbar, hypogastric and left ovarian pains.	Cure.
34	27	Commenced in 14th year, always painful.	Married, one birth.	Cervix ulcerated, tumefied, indurated, os dilated.	Leucorrhœa, lumbar, hypogastric and left ovarian pain, and pelvic pressure ever since menses commenced; constant headache.	Rapid cure.
35	21	Natural.	Married, two births.	Cervix ulcerated, tumefied and indurated.	Anemia, debility, leucorrhœa, left ovarian pain.	Cure in six months.
			Married, eight	Cervix ulcerated.	Anemia, debili-	

No.	Age.	Menstrual Peculiarities.	Social State.	Condition of Cervix and Vagina.	Prominent Symptoms.	Result and remarks.
37	17	Natural.	Married, one birth.	Ulceration of cervix, some tumefaction and induration, os dilated.	Lumbar, hypogastric and ovarian pains, leucorrhœa.	Cure rapid.
38	24	Natural.	Married, two births.	Ulceration, tumefaction and induration, os dilated.	Lumbar pains, hypogastric pns. and soreness, leucorrhœa.	
39	38	Natural.	Married, eight births.	Cervix ulcerated, tumefied, indurated, os dilated.	Lumbar and hypogastric pains, pelvic pressure, dysuria, leucorrhœa, hæmorrhages.	Cure after four months treatment.
40	23	Natural.	Married, three births.	Ulceration, tumefaction, dilated os.	Lumbar and hypogastric pains, soreness right ovarian, leucorrhœa.	Cure in one mo. treatment.
41	35	Natural.	Married, three births.	Ulceration, tumefaction, dilated os.	Lumbar and hypogastric pains, pelvic pressure, leucorrhœa.	Cure after three months treatment.
42	30	Natural.	Married, three births.	Ulceration, tumefaction, leucorrhœa.	Lumbar pains, leucorrhœa, hallucinations of the mind.	Cure.
43	38	Natural.	Married, one birth 2d year of marriage.	Ulceration, tumefaction, induration, dilated os, vaginitis.	Lumbar and hypogastric pains, debility, anemia, leucorrhœa, hæmorrhages, disordered mind.	Cure.
44	25	Natural.	Married, two births.	Ulceration, hypertrophy, induration, dilated os.	Lumbar and hypogastric pains, leucorrhœa.	Cure in four mo. treatment.
45	18	Natural.	Married, one birth.	Ulceration, tumefaction and induration, dilated os.	Lumbar, hypogastric and left ovarian pains, leucorrhœa.	Cure rapid.
46	24	Natural.	Married, one birth.	Ulceration, tumefaction and induration, dilated os.	Lumbar and hypogastric pains, pelvic pressure, leucorrhœa.	Cure rapid.

No.	Age.	Menstrual Peculiarities.	Social State.	Condition of Cervix and Vagina.	Prominent Symptoms.	Result and remarks.
47	50	Natural before disease commenced.	Widow, four births.	Ulceration, tumefaction, induration.	Lumbar and hypogastric pains, profuse leucorrhœa and bleeding hemorrhoids.	Irregular treatment, improvement.
48	23	Natural.	Married, two births; pregnant.	Ulceration, tumefaction, dilated os.	Lumbar and hypogastric pains, pelvic bearing down.	Treatment irregular, but improved and went full term.
49	26	Natural.	Married, three births.	Ulceration, tumefaction, dilated os.	Lumbar, ovarian and hypogastric pains, leucorrhœa.	Cure in two mos. treatment.
50	30	Natural.	Married, two births.	Ulceration, tumefaction, dilated os.	Lumbar and left ovarian pains, nausea, debility, anemia, leucorrhœa.	Getting well.
51	28	Natural.	Married, five births, one abortion.	Ulceration, tumefaction, dilated os.	Cardiac disturbance, emaciation, debility, leucorrhœa.	
52	30	Natural.	Eight births, three abortions.	Ulceration, induration, tumefaction, dilated os.	Occasional lumbar pains, various mucous & mental disturbances, very sallow skin, debility, abscesses about vagina and round, &c.	
53	40	Natural.	Married, never pregnant.	Cervix inflamed and ulcerated internally, patches on its exterior surface, but not near os, which was <i>not dilated</i> , but small and contracted.	Lumbar & right iliac pains, gastric embarrassment, emaciation and debility, very little leucorrhœa.	Relapse from previous attack; cure slow.
54	26	Natural.	Negress, married, five births two abortions.	Cervix inflamed and ulcerated extending up canal, os dilated.	Lumbar, iliac, hypogastric and right ovarian pains, free leucorrhœa.	Cure rapid.

No.	Age.	Menstrual Peculiarities.	Social State.	Condition of Cervix and Vagina.	Prominent Symptoms.	Result and Remarks.
55	35	Natural.	Mulatto, married, several births and several abortions.	Cervix hypertrophied and inflamed.	Lumbar pains & weakness, great vaginal weakness & pressure, prolapsus, leucorrhœa and cough, general debility.	
56	20	Natural.	Married, one birth.	Cervix not tumefied, but somewhat indurated and ulcerated round the os and internally to the constriction.	Commenced with urinary irritat'n, had pelvic pressure & iliac pain, has great cardiac embarrassm't & leucorrhœa.	General nervous excitement and debility.
57	22	Natural.	Negress, married, two births.	Cervix enlarged, soft, spongy, ulcerated and exciting up, os dilated.	Lumbar pains & debility, hyp. & iliac pains, menstruation painful & irregular, leucorrhœa, costiveness, anemia.	Rapid cure.
53	20	Natural.	Negress, married, one birth.	Enlarged, ulcerated, os dilated.	Hyp. soreness & bearing down, intense pain, extending from right lumbar to iliac region, epigastric pain, prolapsus, hæmorrhages and leucorrhœa.	Rapid cure.
59	28		Married, six births, two abortions, with great flooding.	Cervix hypertrophied, ulcerated, os dilated	Lumbar, hypogastric and left iliac pains, pelvic bearing down, dysuria, pains inord. in defecation and nausea, constipation, debility, cardiac	

ART. V.—*Medical News in Paris. Letter to the Editors from R. A. KINLOCH, M.D., of Charleston, S. C.*

PARIS, September 3d, 1854.

Drs. Cain and Porcher :

Messrs. EDITORS—The two months which have passed by, since the date of my last letter, have been unusually dull in Paris. I mean, of course, in a Medical way. Very little, if any thing of interest, has transpired in the hospitals, so that, although anxious to oblige you with a letter, I am afraid you may find the material not worth having. It is true that that fearful, and now, I may say, common enemy, the cholera, has been with us for several months, creating that thrilling interest which it has ever done and which it must ever do, until we shall learn something more of its pathology and treatment, for there is nothing developed, in connection with it of sufficient importance to dwell upon. Indeed the only agitation has been, as you must have seen by the journals, in reference to the use of the sulphate of strychnine in the treatment of the disease. This, as you are aware, is no novelty, but, like many of the boasted remedies, has "gone to the tomb of the Capulets." Some one has seen proper to imagine that he has discovered in it curative powers not previously recognized. Such agitations, perhaps, do no harm; they only lead to more rigid analysis, and force us to see more clearly that knowledge of primary importance—the knowledge of our impotency and ignorance. From the report of the last seven days, ending with the thirteenth of August, I see that the cases of cholera at the hospitals and hospices of the Seine, were as follows :—received from without 232, declared within the institution 99—total 331. During this period of seven days, there went out 192 cases—there died 179. The situation, up to the 30th August, since November, is as follows :—cases treated since November, 5,599; went out 2,247; died, 2,868; resting under treatment, 514. The disease has diminished, in a very marked degree, several times, and then again increased, without there being observed any hydrometrical or thermometrical changes. This is all, I think, worthy to be said in reference to the fearful scourge which seems to march, with equal pace, all over the world. Perhaps, even while I write, you yourselves, may be seeing as much of the disease as I may see here. It is not reasonable to expect that one little town is always to escape. God grant, however, that the evil day may continue long to be put off.

In alluding, in my last letter, to novelties met with at the hospitals, I neglected, for want of space, to refer to a little matter of surgical prac-

tice which I conceive to be of some importance. I allude to the very frequent use made of the actual cautery, in the Parisian hospitals. I think it is Miller, in his *Practice of Surgery*, who remarks upon the frequency of the employment of the heated iron, by the old surgeons, when compared with the modern. He notices how rare a thing it is to see the fire and the irons at the door of the ward of a hospital of our day, and consequently congratulates the profession upon the banishment of a vestige of a by-gone, barbarous age. If Mr. M. should visit Paris at the present time, he would have good reason to alter the opinion expressed in his book, both, I think, as to the rarity of the employment of the cautery, and as to its use being justly considered the vestige of a barbarous age. The glowing furnace and the reddened irons may have been seen almost any day, in the surgical wards of almost any Parisian hospital. It would really seem that we were coming back to the old Hippocratic maxim, that what medicine would not cure, iron would cure, and what iron could not cure, fire would. I think that I see, every day, great advantage from the use of the hot iron in cases of chronic glandular swellings and diseased joints. In the service of M. Ricord, especially, have I seen it applied to the first class of cases. In all indolent buboes, whether syphilitic or scrofulous, and even to indolent lymphatic swellings of the neck. M. R. is quite carried away with the conceived advantage of the practice; he ever applies the iron to cases of scrofulous enlarged testicle, not, of course, here, with any hopes of the absorption of the tubercular deposit, but merely to relieve the accompanying vascular engorgement. In all of these cases, the iron used has only the thickness of a quill, so that the cauterized points are very small, but always numerous. The application, in this way, of the iron, gives very little pain, and in four days the patients seem always ready and willing enough to be touched up again: consecutive applications continue to be made, of from four days to a week, until the result is obtained. In the treatment of diseased joints, the cauterizing iron is large, generally, and is more painful. But now, chloroform is administered, or is brought to the relief of the

anesthetic, however, every day makes me more and more satisfied with its employment, more and more thankful that surgery has now so rich a boon. It is given here as a general rule, with much more effect than when used by the majority of surgeons with us. Indeed it has for a long time been, with me, a matter of melancholy reflection that, even now, so few of the surgeons that I have seen in America, really know any thing about procuring the full advantage to be derived from this potent agent. They are, most of them, either so fearful of the effects of the drug, or else so impatient, when once they have the knife in hand, that they seem, for a time, to forget that the poor creature, over whom they have the momentary controul, is, like themselves, possessed of a nervous system. I shall contend that the surgeon, under these circumstances, is morally responsible for the suffering he causes the fellow-being who has come to him, in full confidence, for relief:—But to digress no further, let me say that I have seen the cautery used here, both upon children and adults, for the relief of very troublesome "*prolapsus ani*." I have not as yet, though, kept these cases sufficiently under my observation, after the operations, to speak positively as to the beneficial results produced. Let me say, before leaving the subject of the cautery, that I trust it will begin to be more used with us; I believe it less painful, and, infinitely preferable, in many cases, to the practice of repeated blistering, to the use of the tincture of iodine and other irritants, from which we obtain very little result. It will no doubt, be somewhat difficult to bring it into general use as it is here, for I do not believe the American will stand the hot iron as well as the Frenchman; but, at all events, let us begin to put him in train. The Frenchman really seems to object as little to this as he does to a blister, and it is truly amusing to hear him before he has ceased to wince from the torture, and as the surgeon is about leaving him, give utterance to the two expressive words always upon the joint of his tongue—" *Merci Monsieur*."

It is, no doubt, true that in our profession points of doctrine have occasionally assumed a disposition as periodic as the tides. Rules of practice deemed excellent yesterday are considered worse than useless to-day; while to-morrow, the same, after careful consideration, may come to be a little modified, and then once more approved and practiced by the worthiest members of the profession. To a casual observer this may appear an evidence of the stationary condition of our science; but, actually, it may be indicative of sure progress. Like the influx of the tide upon the shore, a passing glance at its advancing and receding billows may mislead us as to the direction of the flow, time and observation alone make us certain of its mighty march. It would really seem that French

surgery was, at present, undergoing one of these remarkable reactionary movements. Whether this is a movement, however, in which true progress is to be recognized, cannot at present be determined. What I have shown above, in reference to the use of the cautery, may be considered as one of the signs of such a movement; but there are others, one or two of which I shall notice.

In my last letter, I referred to an operation for the radical cure of "inguinal hernia," practiced by Mons. Maisonneuve. I have since discovered that there was somewhat of a rage for such operations. Mons. Jobert has figured most conspicuously in this line, and has reported several cases of cure, following operation, to the Academy of Medicine, which, this body has verified. I have had the pleasure of seeing two or three cases. The operative procedure is the old one of injecting the sack with tincture of iodine. The method, originating with the distinguished veteran of La Charité, is modified by Mons. Jobert. Instead of exposing the sack, by a previous incision of the superjacent tissues, and thus becoming certain of the injection finding its proper place, Mons. J. thrusts a canula, *as he thinks*, directly into the sack, and throws in the fluid, an assistant pressing upon the upper part of the canal with his finger, in order to prevent the iodine entering the abdominal cavity. I say that Mon. J. *thinks* that his instrument, and the injection, enters the sack, because, to me, it appears very doubtful where the instrument or the fluid does actually go, in most cases. Mons. Velpeau, in his "*Médecine Opératoire*," seems to be little satisfied with his own success, even after using the precaution above referred to, in order to be sure that the injection enters the sack; and he has well remarked that:—"Un sac herniaire vide n'est pas comme un kyste quelconque rempli de liquide, facile à ouvrir au moyen du trois-quarts." Mons. Jobert relies upon two circumstances as proof that the canula is in the sack, viz: the ease with which the instrument can be moved about, and the capability of throwing in the injection. Now, in the name of common sense, has Mons. J. never heard of the attempt on the part of surgeons to trace the direction of fistulous passages with a probe, having been followed by the formation of other passages, from the ease with which the cellular tissue yields before the pressure of even a blunt-pointed instrument! Or again, has he never known of the injections of iodine being forced into the cellular tis-

been thrown? If there be any certainty about the matter, it is, that the iodine is injected just where the operator intended it should not be. The best proof of this is Mon. Jobert's own report of the condition of the parts, after the operation. He dwells particularly upon the amount of "*plastic effusion*" which follows the operation, as evidenced by the swelling and induration of the strictures about the chord and inguinal canal. This is undoubtedly the result. I have seen it with my eyes, and felt it with my hands. But, is it usual to see such a result after injecting iodine into other serous sacs? Undoubtedly not! Pertinent to this very point, I notice, since beginning this letter, some similar remarks by a critic, in one of the weekly gazettes—"On ne voit point," remarks the writer, "*ces résultats locaux après l'injection, iodée des kystes du cordon spermatique sont au contraire, ce cordon dense, présentant la consistance du bois, nous rappellerait plutôt ce qui se passe quand on a porté le liquide iodé, non à la surface d'une sereuse, mais dans un trajet creux au milieu des tissus normaux; lorsque, par exemple, on a enflammé, à l'aide d'injections réitérées, le foyer sinneux d'une fistule anale.*"

But, admitting that, in the above named operation, the iodine can be thrown satisfactorily into the sack, and that, in Mon. Jobert's cases, the plastic effusion and obliteration of the cavity has been the result, can hernia, generally, be thus cured? It is true the descent of the viscera into the same sack may be prevented, but is the inguinal canal obliterated? Is the tampon, formed in the now closed sack, sufficient for this end? Is the abdominal cavity of the strictures so overcome, that the patient, in following his usual occupations, will not be liable to another hernial formation? One month, two months, or three months, is not sufficient to give a satisfactory conclusion upon this point. A narrowing or partial closing of the canal may, for a time, prevent any observable visceral descent, but a considerable period of time must elapse before a reasonable surgeon should conclude, that the case was radically cured. And again, could a conscientious surgeon, after these operations, advise his patient to dispense, altogether, with that excellent safe-guard against hernial protrusion, a well-fitting truss? If he could not, in how much has the patient's condition been benefitted? Is this really an improvement, with the risk of peritonitis, and other dangers which certainly are attached to all these operations for the radical cure of hernia?

I am, then, certainly of the opinion, as expressed in my last letter, that, as a general rule, all operations for the cure of hernia, must ever be scarcely less than failures.

But, Messrs. Editors, I am not, of course, writing an argument against such operations. This whole matter has, again and again, been discuss-

ed by many far more competent than myself, so that it would be vanity and supererogation in me to say more. I did not expect to say half as much, when I began; the remarks offered have been almost unconsciously produced by wishing, simply, to call your attention to the report of Mon. Jobert's cases. Whether this mania for the revival, I may say, of these kinds of operations, is to extend from the wards of the surgeon of "Hotel Dieu," I cannot pretend to determine; but should such be the case, the change must appear to me as rather a step backwards in French surgery.

I have yet to refer to another novelty which, I may say, takes the same direction as the matters already dwelt upon, or, in other words, which looks like an energetic innovation upon surgical practice. The scene, this time, is taken from the ward of M. Malgaigne, at the hospital St. Louis, at which I have, latterly, given an occasional glance. You are aware that one of the subjects, towards which Mon. M. has more especially directed his great abilities, is the management of fractures. The accidents are, perhaps, better understood and more carefully managed in his wards, than in those of any other Parisian surgeon. Yet, permit me here to digress and say, that this may not be according to Mon. M. a great deal of praise; for, as regards the treatment of this important class of injuries, the French are vastly behind us in America. I feel fully confident that a student, from a New York, Philadelphia, or Boston hospital, (alas! that I cannot speak of a Charleston hospital,) has nothing (except it be by accident,) to learn of fractures in a surgical ward at Paris. I have scarcely seen here a bandage properly applied, and the greater proportion of the apparatus employed is incapable of meeting the plainest indications. The use of adhesive plaster in the management of fractures, one of the greatest modern improvements, has not yet been introduced into Paris; and, indeed, as regards the whole matter, from what I have as yet been able to see, the French appear as ignorant, as they are of the proper administration of calomel in the treatment of Pleurisy. But, to return more properly to my subject. Mon. M. claims some originality in his treatment of fractures; and in his ward, some weeks since, I certainly saw what, to me, was a perfectly novel procedure, in the management of a compound fracture of the leg. The case was one of fracture of both bones, at the most common point, a little below the middle, the fracture of the tibia being oblique. You are aware of the difficulty generally met with in these cases, particularly in managing the upper fragment of the tibia, so as to overcome the anterior angular displacement. The case in point I saw, for the first time, two or three days after the adjustment, which was as follows:—The

limb rested upon a double inclined plane, the foot was attached to the foot board, and there was also lateral splints and pads; no particular counter extension was made use of. But the novel feature in the dressing was the presence of an iron band, which, arching over the member a little above the seat of fracture, passed exterior to the lateral splints, and was secured firmly upon either side of the inclined plane. From the centre of this arch passed a firm steel pin, which could be depressed or elevated at pleasure, its upper two-thirds being wormed, and corresponding with the size of the hole in the band through which it passed. The lower and sharp end of the pin had been made to penetrate the soft tissues, over the flat anterior and inner surface of the upper fragment of the tibia, and was there *firmly fixed in the bony tissue itself*. Thus was this upper fragment kept firmly down in its position, and the angular displacement overcome. There was no evidence of swelling, or other appearances of inflammatory action about the point where the pin penetrated; and the patient assured us that it gave him no particular inconvenience. I watched the case with interest for some time after, but could discern no unfavourable result from the apparently heroic practice; the bone was kept admirably in position, and every thing seemed progressing admirably. The instrument of Mon. Malgaigne, for retaining in apposition the fragments of a fractured patella, has been long before the profession. It was natural to have supposed, *à priori*, that serious inflammatory action of the fibrous tissues covering the bone, and of the bone itself, might result from its use; yet experience has not only failed to confirm such an opinion, but has assured us that, in many cases, this instrument may be very successfully employed. It may be the same, with the means above described, for the management of fractured tibia. I apprehend that the cases are few in which such practice need be brought into requisition, still the method may be a good one to have in reserve. Pertinent to this question, and as an indication of sentiment in regard to this energetic surgery, I would mention having seen at Charriere's, a day or two since, a newly invented instrument for retaining in apposition the fragments of a fractured clavicle, the principle being precisely the same as that of Mon. Malgaigne's, for fractured patella. And now, Messrs. Editors, I must draw to a close. I have complied with your request, and written you a second letter; whether there be any thing in it, you and your readers must determine—at all events, it is the best I can at present offer.

Respectfully and truly yours,

R. A. KINLOCH, M.D.

ART. VI.—*Prolonged Gestation.* Reported by J. J. CHISOLM, M.D.,
Of Charleston, S. C.

TOWARDS the close of July, 1843, I was requested to see a healthy, robust servant girl, aged eighteen, of good constitution, whose abdomen was rapidly enlarging, unaccompanied by pain or even uneasiness, caused from sudden cessation of her menses, five months previously. She considered herself dropsical, which alarmed her owner. Suspecting pregnancy, no treatment was instituted except mild laxatives to obviate costiveness, from which she was suffering.

Leaving the city for the summer months, I heard no more of the case, till my return, when, I was requested to attend the girl in her delivery, she having confessed her condition when it could no longer be concealed. Towards the end of November, she having, as she supposed, arrived at full term, was attacked with cramps in the abdomen and pain in her back, which lasted an entire day, when they disappeared. In December, the pains and cramps returned with greater severity—so much so that the patient was crying all the day of the pain. These, also, disappeared, and an interval of a fortnight elapsed, when, in the early part of January, a return of the pains brought on a tedious labor. The enormous size of the child induced me to weigh it, on the second day after birth, when, to my surprise, it weighed fourteen pounds—duplicates of its clothing weighed eight ounces, leaving thirteen and a half pounds as the net weight of the infant. Had it been weighed immediately after birth, it would have been heavier, as it had several copious evacuations and had taken no nourishment.

The case is interesting, from the circumstance of gestation having been prolonged nearly two months over the time, dating from the first appearance of cramps and pain in her back, in November. This is also corroborated by her previous report of suspension of menses, in March, which she attributed to taking cold, and still strengthened by the large size of the child—the parent being rather below the medium stature.

with you, but, I assure you that, I have found the time passed by me in silence, only sufficiently long to enable me to draw just and determinate conclusions of things seen; and as regards the report of individual cases, I must say that I can really recall very few invested with a sufficient number of points, either new or interesting, to entitle them to a place in your journal. In surgery cases of rare interest and replete with points instructive in practice, are of every day occurrence in Paris, but to me, devoting myself as I have done, almost exclusively, to the more monotonous and less exciting science of medicine, the choice of cases, worthy of record, becomes at once more difficult and more embarrassing. The French practice, too, is so different in many points from any which I have seen in America, and their means for the relief of certain pathological conditions so different, and, in some cases, so diametrically opposite, that it has required some time for me to overcome prejudices, and judge frankly of the advantages and results of the two methods. One of the first and most prominent of these points which drew my attention was, the much more frequent use of the lancet than with us, and, to illustrate it, I will give you a hasty notice of certain cases in the wards of Monsieur Gendrin, or perhaps, Valleix, in whose services I have chiefly occupied my time. M. Gendrin is evidently of the old school, holds firmly to maxims which, by many with us, would be thought worse than useless, and uses the lancet with an energy which shows how much less than we, he appreciates the importance of the vital fluid. A case of pneumonia, in a person of ordinary strength, for instance, enters his service, as is almost always the case in hospital practice, not earlier than the commencement of the second stage; unless some strong counter indication exists, the treatment is invariably commenced by venisection; does the inflammatory state continue, it is invariably repeated a second or a third time, until the desired effect is produced. Some days ago, there entered his service a patient labouring under double pneumonia; she was emaciated, of a sallow, unhealthy complexion, and presented the appearance of one imperfectly nourished; the pulse was frequent and resistant, almost wiry, the skin warm, though by no means proportionately so, the respiration hurried and laborious, and, as shown by the violet hue of the lips, insufficient; in a word, she presented one of those cases of the disease so often met with in aged persons, debilitated by in-

possible, and, accordingly, general blood-letting was employed : the case resulted in death, which, however, I do not mention in connection with the treatment, as it would most probably have resulted under any treatment, the patient, as I have stated, being debilitated, and both lungs being involved.

Another case, of the same kind, occurred shortly afterwards, in a young man convalescent from an attack of confluent small pox ; he had not, as yet, left his bed, when he was detained by a very slow convalescence, when attacked by the new affliction, to which the care of the physician was called in its very commencement. After a close examination he remarked, that the disease being confined to the upper lobe of the left lung, probably depended upon tuberculous deposit, and that the forces of the patient, already worn out by the previous disease, were not capable of meeting the pneumonic attack ; therefore, he considered it the wiser course to ward it off by energetic means, of which none seemed to him to likely to succeed as blood-letting, which was accordingly tried, though without success.

Of cholera, which has existed in Paris to some extent, his service has received a large number of cases, and he has largely experimented with blood-letting ; as soon as a patient is brought into the wards, (as far as I can learn, without any regard to the stage of the disease,) general blood-letting, to the amount of ten or twelve ounces, is performed, and the usual course of internal stimulants and friction employed. If in the stage of almost complete collapse, still is the lancet resorted to, the desire being not to diminish the amount of the circulating mass, but to "give a start" to the circulation, and render the blood more fluid. His success, in the treatment of the disease, is not particularly flattering, though he still continues the same treatment.

In ordinary typhoid fever, the treatment, adopted by Gendrin, is quite active, when compared with ours ; if, in the commencement, the febrile excitement is great, the lancet is resorted to, and repeated blood-letting practiced to counteract the condition. If, on the other hand, the grade of excitement is not sufficiently high to call for so powerful an agent, or if some contraindication exists, he prescribes a douche bath of five or ten

face flushed; complains of violent pain in the head and a general uneasiness which prevents sleep;—ordered a douche bath, to be continued for five minutes. July 3d, patient feels much relieved and slept well after the bath: no marked change in external symptoms. July 4th—did not sleep last night and feels again restless and weary; repeat bath and continue it for ten minutes. And thus, throughout the disease, he uses the bath as an efficient means of diminishing the violence of the febrile action. The patient invariably seems, and expresses himself as, relieved after its exhibition, and although, by it, the length of the disease may not be materially influenced, the suffering of the patient may be, to some extent, alleviated.

I was much interested by an experiment, instituted by Gendrin, a short time ago, for the relief of the immediate results, and thus a prolongation of life, in a case of pneumo-thorax entering his service. The result of the case did not furnish a test of its efficacy, but I mention it merely as an idea worthy of consideration, and which might be made useful. The patient, a man of forty-eight years of age, had, for a length of time, suffered from phthisis, presenting all the signs of cavities in the summit of the left lung. About seven days ago he was suddenly attacked by an acute, lancinating pain in the side, and suffered violently from dyspnoea and orthopnoea, in which situation he entered the hospital. Here, a physical examination revealed the existence of pneumo-thorax, and the commencement of the formation of fluid in the cavity; this went on gradually increasing, respiration becoming more and more impeded and his forces failing, when M. Gendrin reviewed the case thus. Whoever, says he, has seen one of these unfortunate patients sitting constantly in his bed, unable to assume the recumbent position, struggling violently for breath, and, at short intervals, racked by prolonged and excessive paroxysms of cough, cannot doubt that his death is being caused, 1st, by consumption of the lung, 2d., by the exhausting cough, and 3d., by the want of rest thus produced. The result of these cases is death, which approaches, ordinarily, rapidly, except he had observed where, at the same time with the internal fistula, there existed, from any cause, an external one, and here the patient not only suffered much less, but life could become prolonged. In accordance with this view he determined to imitate nature, and cause a fistulous orifice by means of caustic potash; the first application being sufficiently powerful to destroy the skin and cellular tissue, the second, the intercostal muscles, and the third to enter the crest; this was put into practice, but before the third application had fully accomplished the perforation, the patient died.

The treatment, adopted in the same service for lead poisoning, is one

that I have never met with, and I will briefly state it. The administration of opium or purgatives (unless the exceeding violence of the early symptoms should demand the former) is only tampering with a dangerous malady, says he, for all the symptoms, for which these medicines are administered, are caused by the slow and gradual introduction of the salts of lead into the system, how, then, can we hope to cure by giving remedies which do not cause an elimination of the poison; all that we can possibly do is to alleviate the immediate suffering, and leave time to accomplish, with risk to the patient, that which we have left undone. The treatment which he employs is the administration of baths of the sulphuret of potassium, and the old treatment of sulphuric acid internally; by the baths the sulphate of lead is formed upon the skin, and he regards them as excellent adjuncts of the internal treatment. I have frequently examined patients after the baths, and observed the skin of different parts of the body, but particularly of the inner surface of the arms and legs, stained of a dark, shining hue, which soon disappears by a slight disquamation; this deposit is regarded as the sulphate of lead.

A point which cannot fail to strike one forcibly, both in the medical and surgical practice of the French is, the exceeding boldness with which they invade cavities which, but a few years since, were hermetically sealed to the application of all medicaments, and, even at present, are very rarely encroached upon, for that purpose, by the instruments of the American practitioner. I allude to the puncture and injection of tincture of iodine into the cavities of the pleura, peritoneum, knee joint, and cold abscesses of all kinds, whether from caries or other cause.

Some time since, there entered the service of M. Valleix, a case of pleuretic effusion which, during a period of seven months, resisted all the means of causing absorption, blisters, local blood-letting, diuretics, &c., &c. The patient's constitution becoming affected, M. Valleix determined upon evacuating the fluid by puncture with a trocar, and modifying the chronic inflammation of the walls of the cavity, by injection

M. Nelaton had lately in his wards a patient suffering from a large abscess of the liver, which he had contracted in Algiers; his constitution was good, and he suffered neither from jaundice nor hectic fever. The surgeon related several cases of success which he had met with in this affliction, by the injection of the tincture of iodine, and unfavorable progress, in case no active interference was resorted to, resolved him at once to put it into practice. Accordingly, adhesion between the capsule of the liver and the abdominal walls was obtained, by the application of caustic potash, and through the centre of the eschar thus formed, a trocar was passed, which gave issue to a quantity of pus and disintegrated hydatids. The tincture of iodine, two-thirds diluted, was then injected and a gum elastic catheter kept constantly in the wound, its end kept closed by a small cork, to be removed at intervals by the patient. The discharge was mingled with bile in large quantities, and the feces showed absence of it, leading to the supposition that a large branch of the biliary duct had been severed by the instrument; it, however, went on diminishing, the bile reappeared in the evacuations, and after one or two repetitions of the injection the patient left the hospital cured.

The only case in which I have seen the injection of this substance into the peritoneum, was not attended by such happy results, its performance, in fact, was only accidental.

One of the first surgeons here passed a trocar into a large ovarian cyst and followed it, as he thought, by injection of tincture of iodine, but, either from escape of the fluid or maldirection of the canula, the larger part, or, perhaps, all of it was injected into the peritoneum. The next day the symptoms of peritonitis developed themselves, and subsequently destroyed the life of the patient.

It is, however, in the cure of large chronic abscesses, that this treatment appears to exert a peculiarly good effect, modifying the walls in such manner as to remove the danger resulting from putrid absorption or pyæmia. In the abscesses attending caries it is universally employed, even before the disease of the bone has ceased, for it is believed that iodine, thus applied, not only has an influence over the abscess, the result of the disease, but upon the disease itself. This view is, of course, not new, for all know how exaggerated were the statements of Lugol, in 1830, with regard to the external application of iodine, in the treatment of affections of this nature.

A report has just been addressed to the director of public assistance, upon the treatment of porrigo favosa, (favus of cazenave,) by M. Bazin, physician of Hospital Saint Louis, in which he considers that he has made, in the treatment of that class of diseases, as marked an ameliora-

tion, as was made in that of the itch, some years ago. Believing, with Messrs. Gruby, Lebert, and Ch. Robin, that the morbid agent in the disease is a parasite, he has, for a length of time, been experimenting with the intent of discovering some substance capable of destroying its existence, and it is to declare his success that his report is made. The following is the process:—1st The head is cleaned and crusts removed by cutting the hair at about an inch from the scalp, and applying emollient cataplasms; twenty-four hours after this the first application (styled by him parasiticide) is made, it consists of from 25 to 45 grains of the acetate of copper or of corrosive sublimate, to 10 ounces of vehicle. The intent of this is to destroy any parasites existing in the hair, or in the depressions of the scalp left by removal of the crusts. 2d. The hairs are all removed by jerking them out with pincers. 3d. One of the above mentioned solutions is rubbed over the scalp with a brush, to destroy the parasites of the deep layers of the cutaneous texture. He declares that he has never found a case to resist this treatment, and gives a report of 201 cases, treated during the years 1852, '53, '54, of which he maintains 74 as being permanently cured. Whether this treatment stands as well in the eyes of the profession as in those of its originator, I am not at present, prepared to say; should it do so, it will certainly be a cause of congratulation to those called upon to treat this rebellious and deforming malady. His treatment, in the first and second steps, does not differ from that of M. Cazenave; it is the employment of the lotion of copper, in mercury that he hopes to effect the death of the parasite, about whose existence there has been and still is, so much doubt. The same doubt, however, for a long period, existed with regard to the acarus in the itch, and we may hope that in the certainty of this new discovery, the microscope has added one more to its many valuable contributions.

I saw a short time ago in the service of M. Nelaton, a case of more interest, and presenting some difficulties of diagnosis, and which, not being very rare in practice, a short notice of it may not be amiss. The patient, a young girl from one of the provinces, had observed, for a length of time a tumour about the centre of the abdomen, from which she suffered slight dragging pains in the loins and a sense of weight at the

for relief. M. Nelaton entered into a close and comprehensive diagnosis, and arrived, by exclusion, at the certainty of the existence of a moveable kidney.

A short time after, a case of similar character entered the service of Velpeau; the displaced organ had taken up its position in the abdomen, below the umbilicus, about three years ago, and although active counter irritation, caustics, and issues, whose marks were indelibly written, had been employed by the physicians to whom she had applied, and who had diagnosticated abscesses and tumours of various kinds, it appeared in no wise inclined to return to its normal place, unlike that just mentioned, being very slightly moveable. Velpeau had met with seven or eight cases, and mentioned one, in which the health of a gentleman had been nearly destroyed by treatment, (consequent upon erroneous diagnosis,) when he was consulted; immediately recognizing the trouble, as an abdominal condition, and not a disease, he checked all treatment, and allaying the mental anxiety aroused by sundry hints of abdominal aneurisms, cancerous growths, &c.. soon saw the patient recover his entire health, and continue his business without hindrance.

In the study of diseases of the eye, there is much employed here a small ophthalmoscope, which appears to combine, in an eminent degree, the greatest simplicity and efficiency. The common employment of most of the ophthalmoscopes in use, has hitherto been interfered with by their complicateness and the time necessary to examine with them, both of which objections, by this new modification, have been entirely done away with. The instrument is one, introduced only a short time since, by M. Anagnostakis, a physician of the faculty of Athens, and consists of a small concave mirror about the size of a dollar piece, and resembling, in form, the common sunglass; one of its surfaces is covered by foil, the other by metal; its centre is pierced by a small hole which, the instrument being held by a candle of three inches in length, is placed before the eye, the mirror turned towards the patient, and the metallic surface towards the examiner. The room is darkened slightly, and a candle, which is held over the shoulder of the patient, throws its rays upon the polished surface where they are collected and thrown upon the ball of the eye, and render distinctly visible, (to the practiced eye,) the interior structure of the organ, the lens, vitreous humour, retina, and choroid. M. Desmarres considers the modification one of real value, and in his *chiquette* makes constant use of it; I heard him remark, a few days ago, that in his private practice, never did he pronounce fully upon a case in which physiological symptoms existed, without corresponding anatomical ones, without bringing this instrument to his aid.

Of course, like all other means of physical diagnosis, auscultation, the speculum, &c., its revelations are of little value, except after some experience in its use. For the information of any one desirous of experimenting with it, I will state that its price is from thirteen to fifteen francs, and the entire apparatus capable of being carried, without inconvenience, in the vest pocket.

Very truly yours,

T. GAILLARD THOMAS, M.D.

PARIS, September 14th, 1854.

MESSES. EDITORS.—A lengthy discussion in the Academy of Medicine, commencing May the 18th, was concluded about a month ago, which, taking as point of departure an isolated question, soon involved almost the entire field of uterine pathology, and has elicited the views of most of the highest authorities in Paris. The origin of it was this: M. Valleix, physician of Hospital La Pitié, in imitation of Simpson, of Edinburgh, has lately employed here the practice of introducing into the uterus an inflexible instrument, for the purposes of reducing this organ to, and maintaining it in its normal position, when affected by deviation or inflexion. His apparatus consists in a stem of ivory, from one to one and a half inches in length, which passes into the uterus, the instrument being straight; as soon as introduced a spring is touched, and the intra-uterine portion flexes itself at a right angle; from this passes out of the vagina a metallic piece which joins another, made to fit against the pubis, where it is secured by bands. The practice was not new, for, as early as the year 1826, Recamier commenced the employment of uterine catheterism, and was soon followed by Amussat, who, becoming bolder, left instruments remaining in the cavity as intra-uterine pessaries. Grave and mortal accidents, however, caused the means to be laid aside for a

that a committee, of which M. Depaul was chairman, was appointed, and on the 18th of May its report, of which the following are the chief points, was made before the meeting. The report of M. Depaul accuses Valleix of having been so blinded by a desire to prove the utility of a useless and dangerous instrument which he had invented, that he attributes to deviations of the uterus, troubles which they never, or, at least, rarely produce, and examining twenty cases reported by him as cured, endeavoured to prove that in each one of the number, the general symptoms, regarded as dependent upon deviations, in fact, had for cause, some complication as ulceration, hypertrophy, &c., upon which he has laid no stress, or passed by in silence. He then takes three important positions: 1st. That out of women in general, attending regularly and without inconvenience to their duties, a certain number will always be found with deviations. 2d. That out of a very large number of cases affected by deviations, and accompanied by other troubles of the organ, the first, together with most or all of the results attributed to it, will immediately disappear upon cure of the second. 3d. That uterine diseases uncomplicated by deviation produce symptoms which, by erroneous diagnosis, are regarded as dependent upon that lesion. In support of these, he proceeds to cite the opinions and experience of the following gentlemen. The opinion of M. Dubois is, that deviations, anteversion and retroversion, and prolapse of uterus, are perfectly harmless, except when much exaggerated, and he agrees with Lispauc, that their chief cause of trouble arises from the fact of their often owning, as cause, a state of chronic inflammation. Of twenty-seven cases of deviations, anteversion, retroversion and prolapse, reported to the Society of surgery, by M. Gosselin, not one suffered any inconvenience in consequence, and his large experience in the Hospital "Lourcine," of this city, furnishes him a like result. Of twenty-seven cases of various deviations, coming under the notice of himself, he has found them without any other appreciable lesion of the organ; out of ten with retroversion, one complained of a frequent desire to urinate and a sense of weight at the pubis after exertion; out of three with prolapsus, only one, and that a very exaggerated case, complained of pains in the lips and pelvis, and of the rest, none suffered any inconvenience. M. Valleix, says he, has discovered a number of cases in which neuralgic and hysteric symptoms existed, either as independent diseases or as results of inflammatory disease of the uterus, and which, being coincident with deviations, have misled him into the belief that they were thus caused, and when in the course of time, these symptoms have disappeared (if hysteric voluntarily, if from inflammation from the rest, and cauterization, which he has used,) he glorifies himself with the

belief that an intra-uterine pessary, which he has used, with the real result of retarding the cure, has effected it in toto. He next takes into consideration the extreme risk to the patient, from metritis and metro-peritonitis, and quotes, in its support, the opinion of Churchill, who regards it as "a dangerous tentative of mechanical intervention;" of Ashwell, of London, who "has likewise seen grave accidents result from the pessaries;" of Robert Lee, who has seen death almost produced; and of Montgomery, who "is convinced of its powerlessness and grave dangers to which it exposes woman;" he also gives an extract of a correspondence with Golding Bird, in which he describes an autopsy of a case, in which death resulted from metro-peritonitis, set up by the introduction of an instrument into the uterus, for the cure of sterility, and mentions one of the same character coming under the notice of Sir Astley Cooper. Furthermore, to show the extreme danger of leaving substances in the body of the uterus, he mentions two cases of death and four of abortion, all well authenticated, occurring as consequences of simple introduction of the uterine sound in examination of the organ, one of the latter of which occurred in the service of Valleix himself.

Valleix responds to this, explains some of the cases of death as, according to him, arising merely from incidental causes, and regards the views taken by M. Depaul, as a limited and imperfect interpretation of facts. That this (like so many other means in daily use) is sometimes attended with evil results, he does not deny; but his adversary, gathering up all these, and mentioning none of the numerous cases of success, conveys an erroneous impression of the danger, he does not mention, on the other hand, that women, with the redresser in the uterus, have gone from Paris to St. Germain, to Sille and to Versailles, distances varying from ten to twenty miles, without inconvenience, and that the majority of the cases have borne it in long and fatiguing walks through the city. Some years ago, says he, "the happy idea of treating articular rheumatism by quinine, was suggested: the doses were, at first, carried too high and mortal accidents resulted. Did they, on this account, renounce the precious remedy which, even when it does not cure the disease, renders it at least more supportable? No; they diminished the doses; they divided them; they learned, in a word, to manage the remedy, and the accidents ceased, leaving therapeutics richer." At the next session of the Academy the discussion of the subject commenced, Maligne opening it with that eloquence and clearness of style which so distinguishes him; although not himself approving the use of the redresser, he characterised as too exaggerated a part of the report in which it was stated as "repugnant to reason," for, says he, "a treatment which Amussat, Val-

peau, Simpson, Huguier, and Valleix have tried, cannot, should not be considered as repugnant to common sense." M. Valleix considers deviations as graver than they really are, disregarding other symptoms, as, ulceration, &c., while M. Depaul robs them of that importance which they deserve; for his part, he allows that women often have deviations without suffering from them, that frequently the cure of an ulceration or hypertrophy leaves the patient well, notwithstanding the persistence of the deviation, but he is convinced that out of the numerous cases daily treated by him, at St. Louis, a large number suffer violent pains and great uneasiness from deviations, uncomplicated by other disease. Five years ago a discussion similar to this took place in the academy, when the contradictory and opposite opinions sustained, showed how little certainty existed with regard to this class of diseases; M. Beau contended that the local trouble depended upon a general state which thus reacted; M. Hervez de Chegoin, that in the large number of cases deviations were the causes of trouble; M. Huguier, that it was due to engorgement, and M. Robert, to granulations; M. Velpeau, denied the existence of engorgement, and regarded deviations as the cause of disease, while M. Dubois considered both granulations and engorgements of as little importance as he did deviations, and made inflammation of the mucus membrane to play the principal rôle. What uncertainty, then, do we see among the best and most reliable authorities! the more fortunate in consequence is it, that the present discussion has arisen to enlighten us. For his part he does not think that the redressment of the uterus should be thrown aside, although it has proved dangerous in some cases, (probably in those when uterine inflammation exists,) it has done much good in others. In some instances immediate relief has been produced, and these he regards as cases of neuralgia of the neck of the uterus, where relief is produced by titillation. Have they not, says he, "cured facial neuralgias by titillating the tympanum, and dental neuralgia by pinching or burning the lobe of the ear?" Possessed by this idea, he tested it by passing a probe into the organ and found, in almost all cases, the pain relieved; in many it returned again, but with less violence. He concludes, then, that we should not reject the means, but that it should be reserved for cases where no inflammation exists, and that the time that it rests in the organ should be short. M. Hervez de Chegoin then took the argument and concludes thus: "all these considerations lead me to conclude that many of the displacements of the uterus require means for their relief; that those which act upon the inner surface of the organ are dangerous; that the means applied by the vagina are efficacious and without danger, when properly used;" he was, in fine, of the opinion that the redresser

should be rejected. M. Dubois, being then called upon, gave as his opinion: "that the pessaries, applied by the vagina, just now said to be efficacious, by M. Hervez, are not so, being, of course, able to act only on the inferior part of the organ and relieving, permanently, neither deviation nor inflexion." He has used the instrument of Valleix in some twenty cases, and has found it in some to cause violent pain and menorrhagia, though in others no inconvenience; in all, however, there has been one observation clearly determined, namely, that at the end of six months the patients were all in statu quo; he has also had opportunities of examining cases treated by Valleix and Simpson, and in all he has found a continuation of deviation quite marked. He, therefore, determines, 1st. That the uterus is never, as regards either inflexion or deviation, rectified by the intra uterine pessary. 2d. That its action, in some cases, is dependent upon its substituting an acute inflammation, which terminates, for a chronic one which would last long, in others, upon its modifying the sensibility of the organ, having been used in a nervous or hyperæsthetic state, of which he thus expresses himself.

"I am not aware that they have any where described a very common malady, which I, for my part, have had occasion to meet with quite often, and of which, nevertheless, nothing is said in our classical treatises. It is not rare to see women who suffer very acute pains in the genital parts; ordinarily in these cases slight redness is observed in the vulvo-vaginal region. This hyperæsthesia may go farther, and in some cases it proceeds to the point of rendering sexual intercourse impossible, and thus becoming a cause of sterility. Here the pain extends to the uterus, and upon any point of the vagina where the finger is placed the patient experiences acute pain. This affection produces in certain women exactly the same phenomena as those of which I have just now spoken—pains in the loins, pains in the pelvis and sensation of weight. These pains have sometimes been relieved by the application of the intra urine pessary."

M. Cazeaux now rose, the chief points of whose argument are the following:—1st. Of the position of M. Depaul, that of women in gene-

patient! 2d. That out of a very large number of cases affected by deviations, accompanied by other troubles of the organ, the first will disappear upon the cure of the second. If this, (which appears very astonishing) says Cazeaux, has been his happy experience, it is directly contradicted by that of M. Hervez, himself and a host of others, "who have seen engorgements and ulcerations, offered before by the neck of the uterus, disappear after the reduction of the deviated uterus." "I have reduced the uterus to its normal position; after the reduction all the symptoms which you have imparted to ulcerations, engorgements, etc., have disappeared, could it then be concluded that the deviation is only a trivial circumstance, and that the other lesions were the sole cause of the pains, which, nevertheless, have disappeared, notwithstanding the persistence of these same lesions." 3d. That uterine affections uncomplicated with deviations produce the same symptoms as these troubles. This point he allows and believes, that a clear differential diagnosis cannot be made without the speculum and the touch; though taking it for granted that M. Valleix's powers of diagnosis fully equal those of M. Depaul, he could not see what strength was thus added to his argument. He took this occasion to differ from M. Dubois with regard to the utility of the vaginal pessary of Hervez. "I am convinced," says he, "like M. Hervez," that in retroversion in particular, his pessaries have cured women which nothing else was able to relieve. This conviction I have adopted from facts in my own practice. He then concludes thus:—1st. That the intra uterine pessary is generally a useless and often a dangerous instrument. 2d. That simple deviations sometimes are often the cause of serious accidents. 3d. That the science possesses the means, sometimes to cure and always to relieve them. 4th. That accidental inflections when not hindering conception and menstruation, are almost never accompanied by accidents, and do not demand treatment. The meeting now closed, and in the next the question fell into the able hands of Velpeau, whose argument, had I not already encroached too much upon your space, right willingly would I give in full, so replete was it with maxims of importance in uterine pathology; but as that cannot be, I will endeavour to condense, as much as possible, his views in a few words:—1st. He denied ever having entertained, in 1849, the views of the non-existence of engorgement of the uterine neck.

ances. The first case which opened his eyes to this highly important fact, he thus relates.

"A lady, an acquaintance of Prof. Pellaton, and who consulted me in 1828, had been treated during ten or twelve years by a number of physicians of the capital. She suffered according to some from an organic disease of the womb; according to others, it was a simple engorgement. The larger number said that she was affected with an engorgement of the uterus and neuralgia. I myself believed her suffering from a marked uterine engorgement, because I had found in her in touching by the vagina and by the rectum a notable tumor behind the neck of the uterus. This lady, cheerful and of lively temperament, finally died. In the autopsy we found no lesion of the viscera, and instead of engorgement, it was impossible to determine in the uterus any alteration whatever, except a complete retroversion." "At that time the Academy showed a marked incredulity, when I announced that inflexions of the uterus were very frequent; that it was they which had imposed upon us, and which had served during so many years as the basis of the entire doctrine of Lisfrance. Malgaigne, among the first surprised at my position, said that never had he seen an example of these inflexions." He then takes into consideration whether deviations of the uterus do really cause the accidents attributed to them. "Deviations of the uterus can give rise to accidents. The manner is evident and can be explained without difficulty; besides, observation proves it every day. But what is there so astonishing that a lesion such as this should be the point of departure of untoward symptoms in the innervation of the woman in the digestive functions, and the different organs of the abdomen. We can then with perfect propriety say that uterine diseases cause, of themselves, a series of accidents, and that at length they may produce real dangers; accidents and dangers, which vary according to a number of circumstances; according to the susceptibility, the constitution more or less nervous of the woman, etc. As regards the treatment in uterine diseases, M. Velpeau considers the profession poor indeed; vaginal pessaries he regards as unsatisfactory palliatives: first.

the very results of which the instrument of Valleix is now accused. While searching over an old closet of "La Charité" some days ago, the very instrument had been by accident found, (and here the old veteran produced an apparatus differing in principle in no essential degree from that under discussion.) As regards the use of the redresser in practice, he thinks that the small number of deaths reported (three or four out of one hundred and eight in the practice of Valleix) is too small to exclude it entirely, and while for his part he had many fears in its application, he would gladly see them dissipated by the results of experience. In practice he always uses at present the girdle, which passing around the abdomen supported the viscera, and thus relieved pressure from the uterus, at the same time aiding somewhat in the support of the latter; its utility is sanctioned by the experience of years. The meeting now adjourned, and the next was opened by M. Amussat; he had been one of the first to essay the intra-uterine pessary many years ago, but was forced to reject it, accidents of grave character occurring under its use, among which he mentions the case of a young lady suffering from antiversion, where he had several times practiced uterine catheterism with impunity. "In her case," says he, "I determined to introduce and leave in the uterus a stem of ivory. I allowed her to walk with this redressing instrument. The distance was rather long. Upon reaching home she was greatly fatigued and suffered much pain in the pelvis; inflammatory accidents developed themselves, and in spite of all the cures by which she was surrounded, she died promptly." He has seen more good result from applying the actual cautery to the anterior or posterior lip of the uterus, and causing adhesion with the vagina, than from any other means. The old treatment by vaginal pessaries, destined to redress the uterus, he continues, "tends to augment the deviation, and the new, by means of an intra-uterine instrument can give nothing but a temporary result in the greater number of cases, and is dangerous in some. M. Ricord now rose and continued the argument thus:—It was not his intention to dwell upon points already so well and thoroughly discussed, he would only say a few words of a certain hyperæsthetic state of the uterus, more or less localized to its neck and to the vagina, and which had already been touched upon in the discourse of M. Dubois. In these cases a vaginal pessary or an abdominal girdle will often give no relief, while the mere change of position in lying will do so; he is inclined to the belief that the redresser (which he has seen of benefit in this class of cases,) acts by removing the uterus from its painful position, and in isolating it without pressing upon its sensitive points. In conclusion, he gives us as his opinion "that the

method of Simpson and Valleix can render service when applied to cases calling for its use, and that consequently it should not be rejected from practice." At the next and last meeting, M. Depaul again took up the discussion; after ably meeting the arguments and criticisms made against his report, he observed that if, as was now contended, this means should not be rejected, it was a little singular that Amussat, one of its first employers, and Velpeau, a man who does not soon become discouraged, should have done so, and that Valleix should at present employ it much more rarely than formerly, giving the preference to the inflated pessary of Gariel. In conclusion, he offered the following resolutions to the consideration of the Academy:—

1st. "That the observations communicated to the Academy by Messrs. Broca and Cruveilhier, joined to those numerous ones of which the science is possessed, prove that the application of the intra-uterine pessary, may often give rise to serious accidents and even sometimes to death."

2d. "In the rare cases where this instrument has appeared to have produced advantageous results, it is not proved that it has always acted by redressing the uterus."

3d. "In some exceptional cases where deviations of the uterus determine serious functional troubles, and have resisted all known therapeutic means, the application of the intra-uterine pessary may be tried."

4th. "The committee respectfully proposes a return of thanks to Messrs. Broca and Cruveilhier, for the interesting communications which have been the origin of these debates."

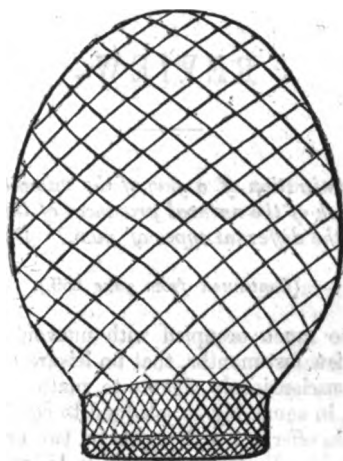
After a discussion, the Academy adopted the first resolution with suppression of the word "often;" the second without modification; the third was rejected and the fourth accepted, with addition of a return of thanks to Messrs. Valleix and other physicians, who had addressed to the Academy communications on the subject.

T. GAILLARD THOMAS, M.D.

THE Instrument for producing Compression in Orchitis,

great improvement to the old plan of leeching, fermentations, cold applications, &c.—the bandaging giving immediate relief from pain, and reducing the swelling considerably in a few hours, and this without confining the patient to his bed.

In treating cases of orchitis, I have found the swelling so much reduced the morning after the straps were applied, that they no longer gave any support, to the testicle, and, therefore, to be efficient, had to be reapplied every ten or twelve hours, or oftener; for as soon as the pressure of the plaster is taken off of the gland, by the latter becoming smaller, the straps no longer act beneficially, and the curative process is arrested until a fresh support is given to the swelling by a reapplication of the dressing. Now, this constant reapplication of the straps is troublesome and painful, and if not attended to as soon as they become loose, no longer give any support to the enlarged gland, and the cure is protracted.



To remedy this defect, it seemed to me that an elastic pressure exerted gently on the swelling, would be efficient—a pressure that would be constantly exercised on the swelling, notwithstanding it became reduced in size. For this object I had made an oval bag, about the size of a turkey egg, netted of thin india-rubber thread—the body of this bag was netted with pretty coarse meshes, so as to be open and cool, but the neck was netted much closer and finer. When this is stretched open and the testicle put into it, the pressure, on account of the closer work (around the mouth of the bag) is a little greater than it is elsewhere—it therefore remains on perfectly well, and there is a gradual and gentle pressure exerted over the whole gland, until the swelling is completely reduced.

I have treated four cases with perfect success, and with little inconvenience to the patient. In the process of the treatment the patient can take it off for a little while to bathe, or for any other purpose, and reap-
ply it himself without any difficulty. The plate subjoined gives a very good idea of the instrument which I use, and which, I think, will be found to give much comfort to patients afflicted with orchitis, and be of some service to the profession.

Your obedient servant,

T. L. OGIER.

Charleston, Oct. 12th, 1854.

REVIEW.

ART. IX.—*An examination of a few of the statements of Prof. Agassiz, in his "Sketch of the natural provinces of the animal world, and their relation to the different types of men."* By J. BACHMAN.

(Continued from page 659.)

WE have been so much occupied with unavoidable and melancholy duties, during the few last months, that no leisure hour was afforded us which we could, conscientiously, devote to matters of science. Nevertheless, as we were, in some degree, pledged to continue this subject, we will, on this occasion, offer a few thoughts on two or three of the prominent arguments used by Professor Agassiz, in his article, (Nott and Gliddon's Types, &c. pp. 58, 76,) leaving the more minute examination of his sketch, with the Tableau annexed, to a future occasion.

On p. 74, Prof. Agassiz says: "I am prepared to show that the differences existing between the races of men, are of the same kind as the differences observed between the different families, genera, and species of monkeys or other animals; and that these different species of animals differ in the same degree, one from the other, as the races of men—nay, the differences between distinct races are often greater than those distinguishing species of animals, one from the other. The chimpanzee and Gorilla do not differ more from the other, than the mandingo and the Guinea negro; they, together, do not differ more from the orang than the Malay, or white man, differs from the negro. In proof of this assertion, I need only refer the reader to the description of the anthropoid monkeys, published by Professor Owen, and by Dr. Wyman, and to

such descriptions of the races of men, as notice more important peculiarities than the mere differences in the colour of the skin." * * *

* "I maintain, distinctly, that the differences observed among the races of men, are of the same kind, and even greater, than those upon which the anthropoid monkeys are considered as distinct species."

As Professor Agassiz has merely made an assertion, unaccompanied by either facts or arguments, we may be allowed to produce some facts, and offer some arguments, that are calculated to throw doubts on the accuracy and fairness of the comparisons he has instituted.

1st. He has produced the chimpanzee and gorilla as an evidence, that these species are as closely allied, as the Mandigo or Guinea negro. With the chimpanzee, (*Troglodytes Niger*. Geoff.) naturalists are sufficiently acquainted, and it differs so widely from the orang-outang, or pongo, that it is now arranged under a different genus, (*pithecus satyrus*). If Professor Agassiz had compared these two orangs with each other, or any two species of the fifty-six well-determined species of monkeys, described by Martin, (Hamilton Smith has given 155) the facts might serve as the foundation for a fair argument. But it should be observed that nothing is known of the gorilla but the skull. In the zoological transactions, Feb'y, 1848, and in April 11th, of the same year, we have a description, by Prof. Owen, of the skulls of this animal, sent by the missionaries, from Gambooa river, West Africa; four skulls had also reached Dr. J. Wyman, of Boston; neither the skeleton or the skin were sent, and we, therefore, know nothing of either the form, the outward appendages, or the colour. This, then, is the species which Prof. A. produces for a comparison. If he had selected any other two species of baboon or monkey, there would be no difficulty on the subject, since the species, arranged under several genera, are more distinctly marked than those of the deer, the antelopes, or the hares.

2dly. His Mandingo and the Guinea negro were domesticated men, and hence subject to all the variations, both in skull and countenance, that are exhibited in domesticated animals. The comparison, to be of any real value, should have been instituted, between his two tribes of negroes, which differ from each other, in skulls, about as much as two neighbouring tribes of American Indians, and any species of animal, subjected to ages of domestication. He has admitted that "the hog descends from the common boar, now found wild over the whole temperate zone, in the old world." (p. 87.) He has admitted the origin of our domesticated ducks, geese, tame pigeon, and the turkey. These are well-known domesticated animals and birds, presenting every variety of form of skull, of size, and of colour. We contend that there are no varieties of men on earth, that present so wide a difference in skull—in skeleton—in form—in external appendages and colour, as in those of the hog, or domesticated pigeon, the varieties of which, he admits, have originated from species that are now living in a wild state. In their feral state, they have undergone no change either in form or colour. It is only when subjected to ages of domestication, that they have assumed all sizes, and colours, and shapes of the skull. When we compare the original wild hog with our long-nosed, gaunt formed and long-legged Carolina grunter—with the short-nosed, short legged, sleek, fat and clum-

sy Woburn, or Guinea hog, or any of the improved breeds, differing, not only in skulls, size, form and colour, and especially in the covering of hair and bristles, we will not be much surprised to find, that by a similar process of domestication, together with changes of food and climate, man has undergone, and is still undergoing similar changes. The same, and even greater changes have been effected in the domestication of the pigeon. Let us compare the large runt—the carrier—the fan-tail—the pouter—the nun—the trumpeter—the tumbler—the ruff—and a dozen other striking varieties with the original rock dove, (*Columba livia*), and we will not be surprised at any varieties in the human race.

3d. To render the comparison a fair and truthful one, he should have proved that the chimpanzee and gorilla presented the same differences in skull, in form, and feature, as are seen in the individuals among the African tribes. This, he is unable to do. Baboons and monkeys, of the same species, have been known and described for ages, and no changes have taken place. As the species were, when first described, so they remain at the present day. Not so with the Mandingo and the Guinea negro. We think we could show Prof. Agassiz some genuine skulls, together with those of their descendants, which would puzzle him vastly, in designating the tribe to which they originally belonged.

4th. In basing his arguments on the facts which he presented, to prove that certain species of monkeys were as closely allied to each other as are the tribes among the varieties of men, he should have offered, at least, a conjecture, that they had no antipathies to each other—that they met together lovingly—associated with each other, and produced prolific offspring. Prof. Agassiz will not deny that this is the case with the Mandingo and Guinea negro, and many other varieties of men, and we trust we will be able to show, in good time, from reliable statistics, that this is the case with every variety of the human race, and that there is no more sterility in the mulatto, the half-breed Indian, or the product of the mongul and caucasian, than there is in the white or any other race. The gorilla will, probably, when the whole animal is procured and examined by naturalists, be admitted as a distinct species. Another species, the moreo orang, is also given from a single skull without skin or skeleton. The gentlemen who have described the skulls, are known to possess high scientific attainments. When, however, the other portions of the bodies are obtained, it will, no doubt, be discovered, should they prove new species, that the conformation of their skull has indicated such structures in the animal form, as will distinctly separate them from all other species. In the mean time, it would be advisable to base our theories on well-established facts, since these are abundant and within our reach, and not fly to conjectures, which, on a closer examination, will be found delusive. Africa is the land of antelopes and of monkeys. The chimpanzee and the gorilla are described as inhabiting the same region, whilst the orang-outan is found in Borneo. A number of species of baboon or monkey, of several genera, inhabit the same forests, both in Africa and tropical America. Who has ever known them to associate? Who has ventured to peril his scientific reputation, by asserting that new races have been produced by the union of two species of monkeys—nor can a single hybrid, from their native forests, be found in any collection in the world, either living or dead?

5th. As men are greatly influenced by authorities, we are warranted in placing the conjectures of Prof. Agassiz by the side of the personal examinations, the close investigations and the masterly demonstrations of Owen himself, professor in the Royal College of surgeons. We believe he has, without a dissenting voice, been placed at the head of the school of comparative anatomy and physiology. We do not believe that, in deep penetration—in a thorough knowledge of his subjects—in his powers of analysis—in sound judgment and his long practical experience, he possesses his equal among living men. This opinion is based, not only on the perusal of his voluminous works, on a personal familiar intercourse, and on an attendance on his occasional lectures and his discussions before various scientific associations in Europe, but on the published declarations of the best judges in these sciences. The opinions of such a man, whose independence of mind and conscientious regard for the truths of nature, as they are presented to his mind, will save him from the suspicion of concealing his convictions, to gratify the prejudices of others, are of very great weight in this discussion. Prof. Owen was the first describer of the gorilla, and subsequently assigned the specific name proposed by the gentleman who obtained and forwarded the skulls. He was perfectly familiar with his subject. He had been, for many years, engaged in examinations of the various species of the monkey tribe—arranging them under their several genera, and in pointing out their specific distinctions. We observed, that whenever a new or rare species of the monkey tribe was brought to his notice, his eye lighted up with enthusiasm and he was eager for an examination.

We have an abstract of Prof. Owen's recent lecture on man-like apes, in the Ethnological section of the British scientific association, which, we are informed, "drew together an immense audience." The address, in full, has not yet reached us, and we are compelled to refer to the abstract, which we find in several of our daily journals. His able lecture is well-timed, and will serve to disabuse the American public of several strange errors which it was attempted to palm upon their credulous minds.

1. As an attempt had been made to show that there is a connecting link, between the highest order of the quadrumana and the lowest order of the negro, Sir Richard Owen "determined the true zoological characters of the known orang-outans and chimpanzees, as manifested by adult specimens, pointed out the relative proximation of these caricatures of humanity to the human species, and indicated the leading distinctions which separate the most anthropoid of the apes from man."

It would appear almost unnecessary, with the knowledge we possess of man and the monkeys, to waste words in exposing the folly of regarding intelligent, speaking, reasoning man, as in any-wise linked with even the highest order of the monkey tribes. When men, in order to show the gradations between man, the negro and the monkey, place the figure of the chimpanzee in a range with the Apollo belvidere, and in the central connecting link, a personification of ugliness in the negro visage, whose counterpart it would be difficult to find, it is sufficiently evident they aim rather at effect than truth. Even in his physical conformation, in his osseous frame, and in his attitudes, the monkey is infinitely re-

moved from any variety of the human race. Man walks erect, and were he to attempt the quadrupedal attitude, his head could not be sustained without great inconvenience and pain. On the other hand, the ape tribe, including the orang-outan, chimpanzee and gorilla, are organised as climbing animals—they totter when they assume an erect posture on the ground, and are only at ease among the branches of the trees. The organization of man enables him to be at ease in a standing, sitting, and lying posture, whereas, the ape is obliged to climb, to sit, and to rest, in very different positions. Adrian Spigel says: "*solus homo ex omnibus animalibus commodè sedet. Cui carnosæ et magæ nates contogere, et pro subellio, pulvinarique tomento repleto enserviunt, ut citra molestationem cedendo, cogitationibus rerum divinarum animum rectius applicare possit.*"

Prof. Owen, after having referred to the higher orders of the monkey tribes, then proceeded to consider man and his varieties. He used the following expressive and emphatic language:

"Entering, then, upon the subject of the varieties of the human race, he defined the degree in which the races differed from each other in colour, stature, and modifications of the skeleton. The unity of the human species is demonstrated by the constancy of those osteological and dental characters, to which the attention is more particularly directed in the investigation of the corresponding characters, in the higher quadrumana. *Man is the sole species of his genus, the sole representative of his order.* He has no nearer physical relations with the brute kind, than those which arise out of the characters that link together the great group of placental mammalia called "*unguiculata.*"

Here, then, we have the matured conviction of the greatest anatomist of any age, who was familiar with every bone and muscle in the varieties of the human family—whose extensive museum was crowded with the skulls and skeletons of every tribe of man—of the varieties in domesticated animals and birds, and the skeletons of wild animals. The organization of these had engaged the study of his whole life, and he records the result of his judgment, as similar to the conclusions of Cuvier—of the two Humboldts—of Lepsius and Bunson—of Pritchard—of Martin and Latham—and the most eminent naturalists of every country, that "*Man is the sole species of his genus, the sole representative of his order.*"

Prof. Owen has recorded his opinion on another important subject. Man is represented, in the scriptures, as the latest creation of the Deity. An attempt has recently been made to show that man was not the last, but that his remains were found in the lower strata, evidencing that he existed before the creation of the present races of animals. Prof. Agassiz speaks of a skeleton, in his possession, 10,000 years old—Dr. Usher,

traces of their existence, in the fossil state, by many teeth and skeletons. Wherever man has existed, he has left other evidences than his bones, to mark the period when he lived and acted. He is a predatory animal—he invents expedients to supply himself with food and clothing. He is obliged to have implements, however rude, for agriculture, for culinary purposes, for procuring his game and warring with neighbouring tribes. Man, living on the surface of our earth, however savage, has left many traces of his life and actions in the implements he used. There are a million of arrow-heads, spears, stone hatchets, &c., found, to one human skeleton. The whole surface of the earth is covered with monuments, indicating the existence of its early inhabitants. If man existed in a period anterior to the present races of animals and men, is it not very strange that in those lower formations, where so many hundreds of specimens of its ancient animals are dug up in marl beds and morasses in a single rood of earth, neither the remains of man or any traces of his works have ever been detected, in such a position as to produce conviction, that they were not placed there by accident or design?

Let us now hear the conclusions at which Prof. Owen has arrived on this subject.

“Human bones have been found in doubtful positions, geologically considered, such as deserted mines and caves, in the detritus at the bottom of the cliffs, but never in tranquil, undisturbed deposits, participating in the mineral characters of the undoubted fossils of these deposits. The petrified skeletons in the calcareous concretes of Guadalupe, are of a comparatively recent origin.”

Men, professing to be governed by the laws of science, have often given pain to the believers in the revealed will of God, by the expression of infidel sentiments. This is rather an evidence of their want of knowledge, than of any error in the revelation contained in the scriptures.

It is refreshing to hear the conclusions to which the mind of this, the greatest of comparative anatomists, has been brought, by the study of nature, compared with the revelation given by the Creator of man and the Author of nature.

“Thus, therefore,” concluded the Professor, “in reference, both to the unity of the human species, and to the fact of man being the latest, as he is the highest of all animal forms upon our planet, the interpretations of God’s works coincide with what has been revealed to us, as to our origin and zoological relations in the world.”

Martin, in his recent admirable work entitled “The Natural History of Man and Monkeys,” has faithfully and scientifically described all the different species of monkeys, under their different genera, and given them their true specific names, but when he comes to the bimana—he says: “*This order consists of but one species—Man.*”

We cannot but believe that the attempt of Prof. Agassiz to prove the varieties of men to be of distinct species, by instituting a comparison between the different species of the wild monkey tribe, has not only proved a failure, but is calculated in the end to give a death blow to his whole theory. If he had compared man with the domesticated animals that have accompanied him over the world, even with those only whose wild origin and change of form and colour he admits as the

effect of domestication, his arguments would have been legitimate and we would cheerfully have met him on that ground; as it is, we have only to regret that he has attempted to support an erroneous theory by an illegitimate comparison.

It should be remarked, in this place, that those specific distinctions which separate the species of monkeys from each other, apply to every species of quadruped, bird, fish and reptile. Wild species do not change in their characteristics or multiply with each other, producing hybrid races. The descriptions of Linnæus and of the Roman writers that preceded him are as true to nature now as when they were first penned. We will show on another occasion, if we have not proved this already, that domesticated species partake of the same character—that two species of animals may produce a hybrid which is organically incapable of producing a race—that a horse remains a horse through all his variations—a goat a goat, a sheep a sheep, a hog a hog, and a dog a dog, and that no mixture with an other species can be perpetuated into a race.*

On page 72, Prof. Agassiz, aware of the powerful arguments that may be produced from the affinities of the different languages of the varieties of men—a subject on which philologists, and among the rest Lepsius, we are informed, are now industriously engaged—states what he supposes a fact in the history of the notes of animals and birds. He says: "Among vociferous animals, every species has its peculiar intonations, and the different species of the same family produce sounds as closely allied, and forming as natural combinations as the so-called Indo-Germanic languages compared with one another. Nobody, for instance, would suppose that because the notes of the different species of thrushes, inhabiting different parts of the world, bear the closest affinity to one another, these birds must have a common origin; and yet with reference to man, philologists still look upon the affinities of

* As the internal organization of the gorilla is, as yet, unknown, let us draw a comparison between a few other species that are closely allied to each other.

The orange, men of the woods, or anthropoid apes, are approximating species. The orang-outan has twelve ribs on each side—the chimpanzee, thirteen.

The following are the differences in the vertebrae of several species.

Species.	Dorsal vertebra.	Lumbar do.	Sacral do.	Caudal do.
Orang-Outan,	12	4	5	3
Chimpanzee,	13	4		7 } Sacral & caudal.
Hoolock Gibbon,	13	5	4	8
Capuchin Monkey,	12	7	3	21
Proboscis Monkey,	12	7	3	23
Bonnet Monkey,	12	7	3	20
Mandrill Baboon,	12	7	3	20
Charmack Spider } Monkey.	14	4	3	32

languages as affording direct evidence of such a community of origin among the races," &c.

In a recent favourable review of Nott and Gliddon on *Types of Mankind*, signed A. L., the writer (who if he had not spoken in rather extravagant praise of himself and his own work, one might conjecture from the similarity of thought and style, and the reversed initials to be Agassiz himself,) makes use of the following language :—"There are blackbirds in America, and black birds, but of a totally distinct species, in Europe. The blackbirds in America have not been taught to sing by the blackbirds of Europe, and yet their note is similar, because their throats are alike"—p. 288.

This would be a good argument if the facts were as he has presented them. We have no doubt he has expressed his honest convictions, but we are equally well convinced that if Prof. Agassiz and the reviewer, whoever he may be, had ever listened to the notes of these black birds as they call them, and compared them with each other, they would have hesitated before they committed themselves in making so heedless an assertion. We can only say that if a teacher of music could not instruct his scholars in the imitation of sounds, better than the black bird of Europe has taught, or can teach the black birds of America, he would either be dismissed for incompetency, or his pupils for stupidity. We contend that every species of bird has its own peculiar note, and

the cocoa-nut, are enormously large ; there are, also, differences in size, colour, length of arms, and in the presence or absence of tails and cheek pouches. But we here perceive that those most nearly allied, differ widely in their vertebræ. The above is the result of the examinations made at the Zoological Society of London, and their accuracy may be depended on. We have seen no account of others.

The following is the osteology of the three principal varieties of man, under which Cuvier arranged the human family.

No. of teeth.	True ribs on each side.	False do.	Dorsal vertebra.	Lum-bar do.	Sacral do.	Caudal do.	Ag'gate bones in skeleton.
Above 16	=32	7	5	12	5	5	240
Caucasian, 16							
Below 16							
Above 16	=32	7	5	12	5	5	240
Mongolian, 16							
Below 16							
Above 16	=32	7	5	12	5	5	240
African, 16							
Below 16							

The above organic structure appears in every variety of the human race. How, then, can it be asserted that any two species of monkeys approach as near to each other, in structure, as the varieties of men that are all allied in every characteristic that constitutes a species? The varieties of man differ even less than the varieties of domesticated animals, and are far more uniform in size. There are black, and brown, and white breeds, in all the varieties of domesticated animals, as well as of poultry. There are greater differences in the skulls of the various breeds of domestic cattle, sheep and hogs, than in those of men. There is in the sheep and goats every gradation in pelage—from the coarse, straight hair of the common goat, and of the Nubian sheep—the latter introduced into Cuba, and now by Dr. Davis, into Carolina—to the softest curled wool of the merino sheep and the cashmere goat. The intelligence of the Arabian horse or the English racer is as far superior to the dray-horse, and that of the spaniel to the bull-dog, as that of the white man is to the negro.

that the notes of species of different genera often bear a closer resemblance to each other than those of the nearly allied species. We admit that the peculiarity of voice in birds and animals is dependent on the structure of the throat—the organization of the larynx—and of the inferior ligaments of the glottis. But that these are very differently constituted in birds and animals, even of the same genus, may easily be discovered by the ear without resorting to the delicate and difficult process of anatomical investigations. We are free to admit that we are not a connoisseur in music, yet we have found no difficulty in recognizing any of our known species of birds from their notes. When looking for new or rare birds, during the last forty years, our constant habit was to listen for the notes of the male. It saved us from many a long walk, and enabled us to find what we wanted without undue exertion. We were not a month in Europe before we were able to recognize the different species of their common birds from their distinct notes. Nor do we regard this as a peculiar faculty imparted to one and denied to another. It only requires close attention and a little practice. In directing the attention of collectors of specimens to this subject, we found that they soon fell into the habit of passing by the common birds which were not wanted, and of listening for the notes of new or rare birds. Mr. Audubon, who was our frequent companion in these rambles, at first expressed his doubts whether species could be detected as well by the ear as by the eye, but he soon gave in his adhesion, and was made sensible of the advantage of learning to distinguish the various species of birds by their peculiar notes.

Returning to the thrushes and the black birds, we will barely allude to the awkward and unintelligible designation of the species. The black bird of Europe is a Thrush—(*Turdus merula*.) The black birds of America are in no wise related to the thrushes, but belong to other genera—*Quiscalus*, *Icterus*, &c. Presuming, however, that the comparison was intended to be drawn between the European black bird, and the American thrushes, we will now briefly inquire how far the statements of Agassiz and his reviewer are correct, when they tell us that “their notes are similar?” We do not hazard a mere conjecture, when we affirm that we know no family of birds in America whose notes differ so widely from each other, as those of the thrushes. The notes of all our Atlantic species, with which naturalists are most familiar, are so distinct and peculiar that a child with a little practice would learn to distinguish them from each other. What resemblance is there between the mewing of the cat bird and the polyglott notes of the mocking bird? How does the whistle of the American robin compare with the clear thrilling notes of the wood thrush, the varied warbling of the Hermit thrush, or the quick shrill hollow tones of Wilson’s thrush? The notes of several of these bear a greater

at imitating the kee-o of the red shouldered hawk—and the crested titmouse sometimes vociferates the note of whip-tom kelly, which seems to have been borrowed from the white-eyed flycatcher. But all these species have native notes of their own, by which they are easily distinguished from all others. A single call note of the European black bird bears some resemblance to that of the American robin, whilst his whole song is widely distinct, as may be easily ascertained by listening to them in contiguous cages, as we have often done in Charleston. A single note is often heard from birds that resemble the notes, not only of the species of other and far removed genera, but of those of quadrupeds. The cluck of the chuck wills widow, as well as that of the American cuckoo or rain crow, are very similar to the call note of the ruffed grouse, and this again reminds us of the chipping Squirrel, (*Tamias lysteri*.) Nuttall, in a letter to us, says of the Little Chief Hare, (*Lagomys princeps*)—"I heard a slender but very distinct bleat, so like that of a young kid or goat, that I at first concluded it to be such a call. * * At length I discovered that this little animal was the real author of this unexpected note." We could, if it were necessary, point out many other resemblances between a single note of birds or animals of very distinct families, to prove that they bore a greater resemblance to each other than they do to those of kindred species. As an evidence, however, that "their throats" are not alike, we repeat what we have said before, that they all have other notes by which they may easily be known from each other.

The immense family of warblers in the United States, with bills and toes, and size of body very similar, have in each species so distinct a note, that we are never at a loss to recognize the males of each species without the trouble of searching for them. The same may be said of the family of sparrows; and how different is the note of the Baltimore oriole from that of the orchard oriole of the same genus.

That the notes of animals and birds are natural and not acquired, and that they are connected with the internal organization of their vocal organs, we have satisfactorily ascertained by keeping them in confinement separated from the slightest knowledge of their own species. The red squirrel uttered its querulous chick-a-ree, the grey squirrel its quack-quack-qua, and the ground squirrel its monotonous chip-chip—although neither of them had ever heard the sound of the parent's voice. The common partridge, which was raised from the egg under a hen in the city, where it never heard the call of any of its species, uttered its cheerful notes of Bob White on the following spring as distinctly as any of its tribe in the fields. The same result—

greater difference in the vocal organs between different varieties, than in individuals of the same variety. If the tribes of men are of different species, why do they not exhibit by their organs of sound those differences that pertain to the species in all other animals?

Again it may be inquired wherein do the varieties of men differ in the enunciation of sounds? The varieties the farthest removed from each other learn to speak any language to which they are trained. In our native town in Rensselaer County, New-York, we had in our boyhood a settlement of Germans on the one side and of Hollanders on the other, intermingled with those who spoke the English language. Our slaves who were rather numerous, and were of unmixed blood of the second and third generation from Africa, familiarly spoke the three languages then in use in the neighbourhood. We observed in the market at York, Pennsylvania, that the negroes spoke the German patois, such as is heard among the Pennsylvania farmers. In Surinam and the Cape of Good Hope, the negroes speak Dutch—in Cuba Spanish, and in St. Domingo, and to a considerable extent in New-Orleans, French. We met a young negro in England who had resided from early life in Ireland, and he had as broad a brogue as any Pat in the Island; and a negro of London used the word "at" instead of hat, like the cockney. The negroes among the Cherokees and Seminoles, speak the languages of the tribes among whom they reside. If sent when young to China, they would speak no other language but the Chinese, and the descendants of the blacks sent from America with the English language to Sierra Leon and Liberia, will be very apt to acquire the native tongues of their ancient African forefathers.

Having said thus much on the adaptation of all the races to acquire various languages, let us next inquire how they are constituted in regard to the power of song. Some nations such as the Germans, the Swiss and the Italians, are more generally devoted to music than others, because it is a part of their education and by this means the love of music has been generally diffused. Every nation and every tribe however possess the power of song and evince their love of music in a thousand ways. We have listened to the war and hunting songs of the American Indians—the love ditties and the spiritual hymns of the negroes, and the monotonous jargon of the Chinese mongolian. A band of music in our streets will at any time draw together a crowd of negroes, excited to the highest pitch of enthusiastic joy. Their singing at funerals and at prayer meetings evidences a whole-souled love of the excitement. The airs played at our operas reach the greedy ears of the

the census of 1850, 3,638,808 of the descendants of the African race in the United States, and nine tenths of them are of unmixed African blood. What has become of the language of their forefathers? There are perhaps not a dozen that understand one word of it. They have adopted the language and sing the songs of the people with whom they live. They have also commingled with the whites, and for weal or woe, produced mixed breeds which have multiplied at a rapid rate.

Let us now enquire what the thrushes have done or any other species of bird in this wide earth. Have they multiplied or in a single instance mixed with neighboring species? Above all, have the notes and the songs of one species been converted into those of another? The American thrushes breed in the same vicinity in our temperate climate and retire together to warmer regions during winter. Has one species ever communicated its notes and its song to its neighbour of another tribe? More than three and a half millions of Africans have forgotten their native language and their songs and adopted those of another people speaking another language, singing in a different style and playing on different instruments. They have laid aside their native tom-toms and adopted the drum, the fife and the fiddle. Suppose we were to import from Europe to America three or four millions of young black birds, give them our robin or any of our thrushes as foster mothers and naturalize them in our country, would they lose their native song and adopt that of any other thrush in the land? Would not nature assert its inherent claim? Would not the organization of their throats deny them the power of singing a new "song in a strange land?"

We now submit to enlightened naturalists 1st, whether "the notes of the different species of thrushes inhabiting different parts of the world, bear the closest affinity to one another," and 2nd, whether the difference in the structure of the languages used among the races of men, may be legitimately assimilated with the notes of closely allied thrushes or those of any other species of birds?

On the other hand we advance it as one among the many powerful arguments in favour of the unity of the human race that different species of birds, however closely allied, never associate with each other, or produce intermediate races, whilst every variety of the human species produce fertile progeny; whilst species of birds and animals although belonging to the same genus, never relinquish those notes which are natural to them and which belong to their organization, and never adopt

Having, we think, shown clearly that the notes in each species of wild quadruped or bird are distinct from those of other species, although an individual may sometimes be found with a louder cry or a more melodious song than others of the same species, the question naturally arises, what effect has domestication produced on the voices among the different varieties in the same species ?

Domestication produces not only striking changes in size and colour, but in the organic structure of the limbs and shape of the skull, but how does it effect the voice ? Large animals in any variety have louder voices because their throats are larger, and in this particular their organs of sound may be compared to the pipes in an organ—the smallest pipes sending forth the shrillest and the largest the fullest notes. In the same manner from Seabrights small bantam—the size of a pigeon, through the various sized breeds of fowls, up to the long legged malay and gigantic shanghai, all the gradations of sound may be heard from the shrillest treble notes of the bantam—through the tenor of the common fowl up to the bass of the Shanghai and bahram pooter. Yet the males of every breed of fowls all have the crow of the cock, preceded by the flapping of the wings, differing in these particulars very widely from the wild pheasant, or any other species of the gallinaceous tribe ;—the hens also in all the varieties cackle and cluck in the same manner. The same characteristics may be observed in all the varieties of the common goose up to the large Bremen geese. The voice of the Chinese goose may be easily distinguished by its more musical tones, and the honk of the Canada goose designates the species as its sounds come gliding over the lake at the distance of a mile. So also every breed of English ducks have the quack of the original wild duck and the shrill guttural whistle of the mallard. The tame male turkey, whether he be white, red or tufted, gobbles precisely like his ancestor in the forest, and the call notes of the hens are the same. The hoarse screams of the peacock and the monotonous and never ending *come-back* of the Guinea hen, whether the birds are white or black, are precisely like those of their wild progenitors. We have kept in the same aviary, the American wild pigeon, the Barbary, the turtle dove of Europe and the American species, the white-headed and the blue-headed pigeons of Cuba and the wood pigeon of Europe—their notes all differed so widely from each other that we required no aid of the sight to know the different species—on the other hand all the different varieties of the common pigeon cooed so similar that there was no other difference than that which might be expected from the different sizes of the birds.

Among domesticated quadrupeds, the same similarity exists in the

the hoarse grunt of the water oxen, both of which may be heard on the plantations of Dr. Davis, near Columbia, S. C., and the latter also at Mr. Williams Middleton's, in St. Andrew's Parish.* Every variety of hog has its lazy grunt, in a state of rest or enjoyment, and its angry squeal when in pain.

These similarities, in the voices of every variety of the same species, present additional evidences that they have the same origin.

The voices of hybrids partake of the notes of both their progenitors. The tedious, difficult, and very uncertain process of procuring hybrids, between the canary bird and goldfinch, has sometimes resulted in obtaining a mule bird with sprightly notes—this race of birds could, however, not be kept up, and, with the death of the bird, this new breed of songsters was at an end. In general, however, we have observed that hybrids are as imperfect in voice as they are in sexual organization. The braying of the mule is unlike any thing earthly; the mongrel duck is nearly mute, and the cry of the rare hybrid, between the Guinea hen and common fowl, is a queer, and seldom heard cackle. We preserved one for several years, and scarcely heard the sound of its voice.

Let us now apply these facts to the human species. What kind of a voice would be uttered by the hybrids between two distinct species of men, remains to be determined when two species are found. Thus far the voices of all are similar, and can be modulated to speak every language and sing every song. We trust it will not be necessary to prove that languages are continually changing. The Latin, of the Romans, has ceased to be the spoken language of any one people—it cannot be understood by the Italian or Spaniard. The English is not what it was the days of Chaucer and Shakespeare—the Dutch—the German—the

* An allusion to the water ox reminds us of another fact, which we will notice more in detail, under the head of hybridity.

A pair of Brahmin cattle and two pair of water-oxen, were imported by Dr. Davis, of Columbia, in 1849; the former, as admitted by naturalists, is a variety of our common domesticated cattle—the latter, of the eastern buffalo. The new theory, that our domesticated cattle have proceeded from a commixture of different species, was now to be tested. The male and female of the Brahmin cattle were placed in the same field with many of our common cows. The female Brahmin cow has produced a calf each year—and, at the same time, as we are informed by Colonel Hampton and Dr. Davis, the descendants from this same bull, by our common cows, amount to more than 1,500, having greatly improved the stocks of cattle in Carolina, Georgia, Kentucky, and Louisiana. In the mean time, what has been the result in their efforts to produce hybrids from the male of the water cattle? The female had a calf annually—the male was running in the immense inclosure of Mr. Middleton, with from 1,000 to 1,500 common cattle, for several years past, and not a single hybrid has been produced. The same was the result at Dr. Davis' plantation. The water bulls were always ready for a battle, but for nothing else. There is another experiment going forward, at the latter place, which, we think, is calculated to peril Mr. Brown's new theory of designating species by the hair. Dr. Davis imported the Cashmere, Thibet shawl, the Malta milking, and the large pendulous eared goats of Syria—admitted by all naturalists, Hamilton Smith, and Dr. Morton, included, to be varieties of the common goat. The product is now over 300. By interbreeding, the coarse hair of our common goat has been converted into wool, as fine as that of the cashmere, and by crossing the Thibet with the Cashmere goat, the long coarse hair, which covers the down of the former, has been converted into the soft wool of the cashmere goat.

Swede—and the Dane, no longer understand each others languages—yet they are the descendants of the Saxon—all their dialects can be traced to one original root. Able philologists are now at work in tracing all the languages of the earth to one origin, confident that, by this means, they will be able to work out the problem of the unity of the human species. Certain it is, that every variety of man will, when placed in childhood among any other people, whether it be in Africa, New Holland, the Feges Islands, or any of the Indian tribes of America, learn to speak their language and forget that of their forefathers. The conformation of their organs of sound must therefore be alike. Hence it is not surprising that the recent advocates for the plurality of races, finding such a similarity in all that constitutes a species in every tribe of man, have been driven, as a last resort, to the theory that the same species of man might have been created as varieties in different parts of the world—a theory broached, long since, in Germany, and which, we supposed, had been set at rest half a century ago. Any new theory, moreover, that requires a new nomenclature and a new and unheard-of definition of species, must be looked on with suspicion; especially from such naturalists as Morton and Agassiz, who were content to describe all their species by the established rules which governed the naturalists of the world, until they found themselves hemmed in with difficulties in attempting to establish a new theory.*

We yet crave the indulgence of our Editors and the patience of our readers, whilst we notice the summing up of Prof. Agassiz's article, in page 75-6, and the charge which he has made against the believers in the unity, of arguing in favour of "the Lamarckian development theory." He says "there is no evidence whatever," that mankind originated from a common stock, and that, because there is a diversity among animals, "it follows, that what are called human races, down to their specialization as nations, are distinct primordial forms of the types of man."

Professor Agassiz is fully aware that the doctrine of the unity of the human race was an established belief among men of science before either of us were born. Therefore, the speculators in a new theory, instead of requiring the believers in an adopted theory to prove that there

* Dr. Morton finding that the generally adopted definition of species would overturn his whole theory on hybridity, defines species by a "*primordial organic form*." He admitted "a difficulty presents itself in the outset in determining what forms are primordial." Dr. Nott corrects what is faulty in this definition by giving another "species—a type or organic form that is permanent, or which has remained unchanged under opposite influences for ages." He cites as examples, the Arab, the Egyptian and the negro, the greyhound, the turnspit and the wild dog, on the

was originally no diversity in man, are bound to afford those evidences which would satisfactorily prove that these diversities existed from the beginning. The burden of proof now falls upon them, and, to require us to prove the contrary, is, to quote his own language, "begging the question." It behoves them to prove that the Japetic,* Mongolian, African, Malay, and American stocks are not varieties, but original species. When they are able to detect a single tooth, vertebra, or muscle, in one race, that is not found in every other—when they can clearly prove that the issue between these races is not prolific—when they prove that they differ in conformation of their organs of speech, and that one race is incapable of laying aside its native speech and song, and acquiring those of another, we will regard them as legitimate arguments in favour of their new theory. Conjectures and speculations will not be satisfactory—we call for sound reasoning based on unquestionable facts.

In insisting on the doctrine, that animals and men have run into varieties in their domesticated state, we have asserted no more than Agassiz, himself, has admitted. If the hog, the merino sheep, the pigeon, &c., have produced the breeds that have originated from their several wild stocks, all of which professor Agassiz has published as the result of his investigations, then there is no difficulty in accounting for the origin of the different varieties in the human race.

What reasonable grounds then we ask are there for charging the advocates of the unity with believing in the absurd theory of Lamarck. This idle charge was made against us before in this journal which also contains our answer. We have been so fortunate as never to have been inveigled into a belief of any of the many humbugs of the day. The notions of Lamarck, of Oaken, and of Vivian, the author of the vestiges, and the believers in the development system, have appeared to us too absurd to require a serious refutation. The theory that the surface of the earth was at first covered over with mucous, which by the power of galvanism called forth animalculæ—that these became in successive developments, worms, reptiles, fishes, and warm blooded animals; and, finally, that the monkey was elevated into a man, can dispense with the creative power of God, and is best adapted to the views of an atheist. We believe in species as having originated from the hand of the Creator, and that they continue to the end of time to bear the characteristics of the original creation. That any change which domestication may produce does not destroy the marks of identity. Hence we

views on the characteristics of species, might have been looked for from any other man rather than Agassiz. It is calculated to place his opponents in an unfair position, and from its absurdity to weaken the cause he advocates.

In our leisure moments, which have recently been doled out to us very sparingly, we will give our views on the characters of species and on Agassiz's "Zoological provinces of the animal world and their relation to the different types of man."

In conclusion, we may be allowed once more to express a hope that naturalists and men of science who write on this subject, will in future confine themselves to their legitimate calling, and leave theologians to discuss the scripture question. Formerly the clergy were told by men of science, "leave us alone in our pursuits and we will not meddle with the scriptures." Of late, however, they have struck out into a new path, and commenced a rude and reckless warfare against the Bible and the clergy. When a clergyman comes forward professing to take them at their word, to leave the Bible alone and discuss the whole subject on the principles of science, they immediately commence on the scripture question. This was the case both with Morton and Agassiz. When we were thus compelled in self-defence to quote the opposite scriptural passages, they conceived they had fairly diverted us from the path of discussion which we had proposed to pursue. They arrayed the passages we had quoted as a warrant for them to become expounders of scripture. Naturalists are, at best, unpractised theologians. They may aid in making sceptics in religion, but they are injuring the cause of true science. Then come the subalterns in the "combination," and exhibit to the public specimens of their courtesy, their veneration for the Bible, and their love for its ministers. Have they permitted themselves to be misled by the idea that all clergymen were so timid, so frightened at the rod which was suspended over their heads, that they would sooner surrender their judgments and their consciences, than submit to the bitter personalities of their opponents? We were not without a hope that A. L., in the *Southern Review* for October, would at least have afforded us some evidence that the views of Gliddon were unpalatable to his scientific appetite. That he who professed to give an honest review of a book should not have omitted to stigmatize the rude attack upon us, as unjust as it was uncourteous. We could not but think that the article of Agassiz, couched in the language of science, but erroneous, as we are prepared to prove, was entirely out of place in such a work as

ABSTRACTS

FROM FOREIGN AND DOMESTIC JOURNALS.

PATHOLOGY AND PRACTICAL MEDICINE.

Apoplexy of the Spinal Chord. By M. TRIER. (Dublin Medical Press. From Guntz. Ztschr. and Med. Times and Gazette.)

Ollivier, in his work upon diseases of the spinal cord, based his views, as to the influence of spinal hæmorrhage on sensation and motion, upon the physiological views at that time recently propounded by Bell and Majendie. His observations, however, wanted confirmation, inasmuch as pathology had not recorded a single case of hæmorrhage into the spinal cord, without their coexisting lesion of the brain or of its membranes, so that it was impossible to determine the share which each had in producing the morbid phenomena. Since then, Goultier de Claubry, Cruveilhier, Monod and Grisolle, have related cases; and Trier adds another, which occurred in the Friedrich's Hospital.

The author concludes, from these cases, that the paralysis affects that side of the body which corresponds with the extravasation. There may be loss of motion alone, or of sensation alone, or of both together; the last being the most common. It is not yet proved that hæmorrhage into the posterior part of the spinal cord especially affects sensation, and into the anterior part of the spinal cord motion. From the cases hitherto recorded, it has been remarked, that paralysis is not the first symptom. The disease commences after a shivering fit, with pain along the corresponding part of the vertebral column, with increased sensibility to pressure upon the spinous processes. This pain soon extends from the affected spot to both sides of the body, and seizes on the limbs which soon become paralysed. The pain differs from that of rheumatism, in not being increased by active or passive motion, or by pressure. In paralysis, resulting from cerebral apoplexy, this symptom is wanting. In hemiplegia dependent upon hemorrhage into the cord, there is frequently pain of the opposite side of the body, a symptom not remarked in similar conditions dependent upon cerebral disease. The degree of pain varies from a sense of weight and oppression to the most acute and agonising suffering. Cramps of the toes, so often witnessed in apoplexy of the medulla oblongata, and also in cerebral hæmorrhages, do not occur in apoplexy of the spinal cord. Paralysis of the bladder and of

the rectum occur simultaneously with the muscular paralysis, or soon after; the former producing ischuria, or simple enuresis; the latter constipation, which, once removed, is followed by involuntary discharge of feces. Respiration is affected in those cases only where the upper part of the spinal cord is involved. It is short, broken and hurried, and sometimes purely diaphragmatic, from paralysis of the intercostal muscles. Paralytic aphonia is as rare as paralytic dysphagia from this cause. The pulse at the commencement is often strong, full, and frequent, but subsequently it resumes its usual beat, if nothing accidental occurs to interfere. The intellect is clear, the movements of the muscles of the face and tongue unimpaired. As secondary appearances, the author enumerates passive congestions and hæmorrhages into the abdominal viscera, ulcerations of the urinary mucous membrane, abscesses, and bed-sores. Hæmorrhage frequently takes place in the spinal cord, which has undergone partial softening. It has occurred in persons who have been violently exerting themselves; but, as in cerebral apoplexy, recovery seems possible after the lapse of many years. As regards the echymoses upon the lower segment of the body, as also the dark colour and injection of the mucous membranes and of most of the viscera of the abdomen, both Ollivier and Cruveilhier have pronounced them to be appearances resulting from stoppage of the circulation consequent upon impeded respiration. But, observes the author, this explanation does not suffice for those cases in which respiration becomes free shortly before death; and it is remarkable, that similar morbid appearances do not occur under other conditions leading to difficulty of breathing. He, therefore, dismisses the generally received opinion, and believes, that the cause must be referred to want of power in the sympathetic nerve, which, from its intimate connexion with the spinal nerves, must sympathise with destruction of the spinal cord, and be reduced to a more or less paralysed condition. This influence is manifested by stoppage of the circulation, by transudation of blood, or by ulceration, and the formation of abscesses.

Cholera in Richmond, Va. (From the Stethoscope.)—We would be glad to give our readers a sketch of this disease as it has prevailed in our city during the past few weeks; but in the absence of a board of health and meetings of our city medical society, we find ourselves deprived of the usual sources of information of a general character, with the exception of the records of the superintendant of the public burial

Burials of Persons who had died of Cholera.

WHITE PERSONS.

				Males.	Females.
1854—June 19,	-	-	-	0	1
23,	-	-	-	1	1
July 10,	-	-	-	1	0
11,	-	-	-	1	0
12,	-	-	-	1	0
13,	-	-	-	3	1
14,	-	-	-	0	1
16,	-	-	-	1	0
17,	-	-	-	2	0
18,	-	-	-	2	0
20,	-	-	-	1	1
21,	-	-	-	1	0
22,	-	-	-	1	2
				15	7

COLORLED PERSONS.

				Males.	Females.
1854—July 7,	-	-	-	1	0
8,	-	-	-	3	0
9,	-	-	-	2	0
10,	-	-	-	5	0
11,	-	-	-	6	0
12,	-	-	-	6	0
13,	-	-	-	9	0
14,	-	-	-	1	0
15,	-	-	-	4	1
16,	-	-	-	3	2
17,	-	-	-	4	1
18,	-	-	-	1	1
19,	-	-	-	4	1
20,	-	-	-	1	1
21,	-	-	-	0	2
22,	-	-	-	2	1
				50	10

while among negroes there were *five* times as many males as females.

The greater mortality of males, we think, is chiefly attributable to the greater prevalence of habits of intemperance among them. Our city is completely infested with those pests of society, common dram shops, many of which carry on their abominable, murderous traffic under the authority of our city government. At these places a large amount of villainous stuffs is sold to blacks and whites, which is commonly known by the very appropriate title of *rot-gut whiskey*. A ruinous practice to country negroes prevails here, by which they are furnished with means for procuring this poison. Negroes hired to our factories, instead of being supplied with wholesome food, are generally furnished with board money, as it is called, amounting to about a dollar or a dollar and a quarter per week. With this they are expected to pay for room rent, fuel and food. The consequence is, that many of them live in damp cellars and close garrets, living on the meanest fare, and spending their money at the grog shop. They are then fit subjects of disease; and when epidemics of a typhoid or depressing character prevail, they have no stamina to contend with disease, and sink under the struggle by scores. We have often thought that the owners of negroes in the country would consult their own interests by hiring them out at half price where they were furnished with good fare and comfortable lodgings.

As usual with cholera, it has infested particular localities, while other parts of the city have been nearly exempt. These localities are chiefly a low, damp piece of ground near the state penitentiary; and in that institution a large number of cases of diarrhoea, dysentery and malignant cholera have occurred; the valley between the two principal hills upon which Richmond is built, and the low grounds in the neighborhood of the shipping wharves, known as Rocketts, together with a more elevated strip of land below Rocketts. The last mentioned place forms part of a valley running back from the river, the surface of which being nearly level, and composed of clay soil, contains several filthy, stagnant pools of water.

The first three deaths of white persons which are recorded in the table above, were members of one family by the name of O'Connor, living at Rocketts, and a number of cases which occurred in other parts of Richmond could be traced to these infected districts. It is a remarkable fact, that the place most infected during the former visitation of 1849, is at present entirely or nearly exempt. This is a range of low frame buildings near the large basin of the James river and Kanawba canal, and was familiarly known in 1849 as the infected district. It is, therefore, an interesting subject of enquiry to what cause its present exemption is due. According to our recollection, the places most infected now were also, with this single exception, the most infected in 1849. At that period there was a range of old canal locks filled with rubbish, passing directly through the infected district. The water soaking through this rubbish rose to the surface, making the cellars extremely damp. In addition to this, the streets surrounding this district were unpaved, and the drains were in an excessively filthy condition.

At the present time the appearance of this district is entirely changed. New locks have been cut through, by which the leakage water is carried off, and the streets are all paved and provided with well graded drains. The population of this district is precisely of the same character as that in 1849. Comment on these facts is unnecessary.

Cholera has been so well described that it is unnecessary to present our readers with any details of cases. Suffice it to say, that some of the cases have been of as malignant a character and as rapidly fatal as any we have ever witnessed. A discussion has been going on in our city as to whether cholera is *epidemic* here, and, as usual, the most positive opinions are advanced by those totally ignorant of medicine. While these persons would consider it the highest piece of presumption and folly to pronounce an opinion in regard to a difficult question in botany, geology or astronomy, or any other science of which they may be ignorant, they do not hesitate to express opinions with the utmost confidence on the most abstruse questions in medicine, and to denounce and villify physicians who may differ from them. For these grannies in petticoats, we can feel some pity, but towards those in breeches we feel disgust and contempt.

It is sufficient for us to state here, that without the *epidemic cause* of Asiatic cholera, no case of Asiatic cholera can occur. Exposure and imprudence alone cannot produce it. They may and always have produced diarrhoea, dysentery and cholera morbus, but they do not produce Asiatic cholera, unless the cause of Asiatic cholera be superadded. For ex. $A + I = D$ but $A + I + X = C$. The unknown quantity X is a necessary element for the last equation. But says some one, although the epidemic cause be present, there have not been a sufficient *number of cases* to constitute an epidemic. We would like to be informed how many cases in a population of 80,000 would constitute an epidemic. We presume there have been two or three hundred cases, and seventy-five deaths are reported, besides those which have not been reported, within the period of twelve days.

In addition to this, there has been a general complaint of a feeling of malaise, of cramps in the bowels and in the calves of the legs, shewing the existence of some general morbid cause, operating upon the entire mass of the population.

The present epidemic was ushered in by diarrhoeas and dysenteries, which prevailed to an unusual extent, for three or four weeks. These were generally very manageable under the use of calomel, opium, astringents, demulcents and counterirritation.

At the present time, we think the disease is subsiding; a smaller number of cases are met with, and these being of a more manageable type. B.

Treatment of Continued Fever by Stimulation. By Dr. Brinton. (Ranking's Abstract, from Lancet.) The subjoined quotations, which are taken from a paper on the general treatment of fever, are of great interest and importance as connected with the remarks of Dr. Todd on the same subject.

Dr. Brinton states it to be one of the chief objects of his paper—"to record the successful results of my careful and deliberate trial of a system of stimulation which was proposed some years ago by my friend and former colleague, Dr. Todd. This consists in the frequent exhibition of very small quantities of brandy diluted with water, alternately with beef-tea and other fluid food. The *rationale* of this plan I presume to be that ordinary port wine is a mixed fluid of uncertain strength, which requires digestion, and has the additional disadvantage of sometimes acting upon the bowels; while brandy is taken up at once by the veins of the alimentary canal. The smallness and the frequency of the dose in which the stimulus is administered are equally important; for it makes every difference to the patient whether his exhausted system is sustained by minute doses of alcohol every hour or half-hour, or partially intoxicated three or four times in the day. In the latter case, the reaction between each of the separate doses sometimes places him in a far worse position than if no stimulus whatever had been given.

"But whatever may be thought of this explanation of the above plan—for which it ought to be stated that I alone am responsible—few who had seen the cases under my care in the Royal Free Hospital could have doubted its efficacy. And one point of great importance respecting it is the fact that, at this stage of the disorder, severe febrile symptoms do not contraindicate its use. Repeatedly have I seen all the appearance of fever clear off as though by magic under a quantity of stimulus (sometimes amounting to twenty-four ounces of brandy daily,) which might well alarm any one who had not had practical experience in its effects. Whether its efficacy might not be reduced to some more exact and physiological statement than that of 'supporting the strength,' I hardly dare inquire, lest it should lead me to a topic which, though interesting, would be irrelevant here,—the relation of fever to combustion and animal heat. Thus much, however, may be pointed out, that alcohol is comparatively a very pure carbonaceous diet, and an agent which appears to check the process of waste in all tissues.

with which they may be repeated. The same drug may also be administered by the mouth and rectum. But of the two, the enema appears to be more efficacious than the mixture. And even apart from this fact, since the majority of these cases also require the very frequent administration of brandy and beef-tea, it is preferable in all but extreme cases to leave the stomach free for the latter by giving the enema only. To both enema and mixture I have generally added full doses of the vegetable astringents with great advantage. And it is scarcely necessary to allude to the value of opium, which, unless expressly contraindicated by the cerebral symptoms, should generally be an ingredient of the glyster. The following prescriptions are those which figure most frequently in my notes: Spirits of turpentine, five minutes; tincture of catechu, half a drachm; mix, and afterwards add one ounce of infusion of krameria; make into a draught, to be taken every fourth hour. Spirits of turpentine, thirty minims; tincture of catechu, two drachms; tincture of opium, fifteen minims; mix; to be added to two ounces of decoction of starch, for an enema.

"The small bulk of this enema gives it a better chance of its being retained, which it very often is. Its efficacy in controlling the diarrhoea is remarkable; so much so, that while the febrile symptoms continue very prominent, I have generally suspended its use as soon as it has reduced the evacuations to three or four per day, having reason to doubt the propriety of checking them where they do not threaten any serious drain to the system.

"The efficacy of the treatment just sketched out may be best imagined by an example like the following."

M. W—, æt. 42, was admitted into the hospital, February 14, 1853, at about the tenth day of typhoid fever. When brought in she might almost have been termed moribund. Her skin was of a brick-red hue, and of a pungent, dry, but not excessive heat. Her pulse of about 130 per minute, was feeble, fluttering, and at times scarcely perceptible. She lay supine and unconscious; breathed stertorously; was blind and deaf; in short the only relics of the animal functions were evinced by her still executing the movements of swallowing and moaning when her tympanitic belly was pressed in the region of the cæcum. Her urine and fluid fæces were passed involuntarily, and the latter at very frequent intervals. The integuments over the sacrum had sloughed.

She was ordered half an ounce of brandy, with one ounce of water, every hour, and beef-tea as frequently in the alternate half hours. A turpentine stupe was applied to the belly, and the above enema administered. The two latter remedies were repeated in the course of two or three hours. The distressing state of the bladder and nates were also attended to. But I did *not* apply blisters, as perhaps I ought to have done: for not only did the state of the pulse and bowels render me less desirous of doing so, but, to say the truth, I had grave doubts whether the patient would live till the blister rose, and was also very anxious to concentrate the nurse's whole attention on the sedulous repetition of the stimulants and enemata.

On my next visit the patient was still living, but without any apparent amendment. Finding that she had swallowed better than I had

expected, the quantity of brandy was raised to eighteen ounces, and, after a few hours, to twenty-four ounces per day, to be divided into rather larger and more frequent doses than before.

The next day the stools were no longer passed involuntarily; and the pulse was decidedly improved in both force and rhythm, though nearly as frequent as before. She was sensible of the noise made by shouting loudly in her ear, but seemed not to be capable of appreciating the words used. Her skin was rather paler, and much less pungent to the feel.

From this time she gradually amended in every respect. The diarrhoea soon diminished in frequency, but continued for a fortnight to claim the most constant and watchful attention. But long before this the patient had recovered her senses, her eye had brightened, her skin had cooled down, her pulse had dropped, her tongue had peeled, she had perspired, and was, in fact, convalescent. Her recovery was, however, very much retarded by the presence in her bladder of a quantity of rough precipitated phosphates, which had doubtless accumulated there during the distension and paralysis of this organ, that had preceded her admission. Very little alleviated by treatment, the intense pain and partial hectic produced by this mortar like mass were terminated by its gradual discharge piecemeal through the urethra, and she left the hospital cured.

Another case which may well illustrate the good results of the same plan is that of a woman who had been subjected to an operation for the relief of an ulcerated leg by my colleague, Mr. Gay. She was attacked with typhoid fever, and transferred to my care. The fever was very severe, and all healing action in the leg was already suspended, when another grave disorder supervened, in the shape of erysipelas of the limb. This spread upwards from the ulcer of the leg, and terminated in abscesses of the thigh and groin. In spite of all these serious complications she made a very fair recovery, so as to be discharged from the hospital cured of all her complaints, and with a leg which did great credit to Mr. Gay's new operation, its larger ulcer of many years' standing being covered by a firm cicatrix. I believe my esteemed colleague once or twice felt rather nervous on seeing the large quantity of brandy our patient was taking daily; and, to say nothing of the stimulus and support she received, I am persuaded that, unless the characteristic diarrhoea had been instantly controlled, this case must have been lost.

diarrhoea of typhoid, and remedy its exhaustion, the cerebral symptoms of the second and third weeks of severe typhus will frequently defy all the resources of our art.

"Returning to the general results of the treatment of both kinds of fever, 8 deaths in 77 cases will represent a mortality of about 10.4 per cent. I think this would generally be regarded as a low rate for the class of cases usually received into metropolitan hospitals,—especially if it be remembered that I have excluded those numerous instances of ephemeral fever, or febricula, which are often included under the common name of fever in hospital returns. But it is very difficult to compare the cases of different institutions in all those minute details which are necessary to insure a true parallel. Such circumstances as the population from which they are derived, the amount of selection exercised in admitting them, and the character of the various epidemics themselves—all these are important elements, which we can rarely regard as identical for any two groups of cases. And yet without such an identity it is obvious that their mortality may differ to almost any extent.

"There is, however, one comparison which appears far more valid and significant than any of these could be; since it applies to a large number of patients, derived from the same localities, suffering from the same epidemics, and placed in exactly the same circumstances with those above mentioned in all respects save one—that of the treatment pursued. And I trust that, in adducing such a comparison, the reader will believe me actuated by no motive but that of placing fairly before him the numerical results of a plan which, as it appears likely to be of advantage to human life, it is the duty of any one holding a public and responsible position to promulgate and recommend. My late lamented colleague, Dr. Chambers, was a gentleman whose long experience as an hospital physician, and great humanity and skill, sufficiently vouch for the manner in which his patients were treated. During the period he held my present appointment of senior physician, forty-nine cases of fever came under his care: of these ten died, making a mortality of 20.4 per cent. Far from my having any reason to think these cases more severe than my own, I have reason to suspect that, on the whole, they were less so,—both intrinsically—and in consequence of my excluding *febricula* from my list. Whatever may be thought of this mere numerical contrast, I think those who witnessed the cases themselves found their comparison still more impressive. Among such impartial and competent observers, I may especially name Messrs. Lane and Curgenven, the late and present house-surgeons of the institution; both of whom soon recognized the merits of this plan of treatment, just as I had myself been obliged to confess the efficacy of some of its chief features when house-physician at King's College Hospital seven years ago."

On the Principles which should regulate the use of the Iodide of Potass in the treatment of Chronic Rheumatism. By Dr. Basham. (Abstract from Lancet.) In the following remarks, Dr. Basham enunciates a rule of general practical value,—viz., *that iodide of potassium is only useful in those cases of rheumatism in which the patient has at*

some former time been mercurialized. This rule is the sole result of clinical observation and experience; but it derives collateral support from the recent researches of M. Melsen (*vide Abstract, viii.,*) which show that the action of the iodide under these circumstances is that of a chemical solvent, which dissolves the mercury out of the tissues and forms a suitable compound, which finds its way out of the system through the kidneys.

"From time to time," says Dr. Basham, "cases have come under observation, presenting the usual symptoms of chronic rheumatic pains, gnawing and erratic, with paroxysms aggravated by atmospheric changes; and there has been diffuse tenderness of the periosteal surfaces nearest in the contiguity to the skin, as the scalp, clavicles, ulna, tibia, &c.; sometimes distinct tumefaction with exquisite tenderness, and these nodal elevations in some have been evanescent, in others persistent during the whole course of the malady. It has frequently been noticed, and practitioners of experience cannot have overlooked the fact, that some of these cases materially and rapidly improve under the administration of iodide of potassium, while in others, with symptoms in all respects identical, no benefit has been derived or improvement become apparent till the patient has been put through a course of bichloride of mercury and sarsaparilla.

"I was once inclined to think that these varying results depended on peculiarity of constitution, and that the treatment of such cases by one or other of these remedies, must remain to a certain extent, empirical, and destitute of any settled principle. But a careful examination of all the precedent conditions in the history of such cases exhibited the following facts: That in all cases in which the iodide had been productive of benefit, the patient at some antecedent period had been salivated, in some for syphilis, in others for an inflammatory or other disease; while in those cases in which no benefit was obtained by the iodide, the patient had either never taken mercury to salivation, or had suffered from syphilis or gonorrhoea, which had been either neglected or treated only locally. It appeared then, that there were two predisposing causes to the same form of chronic periosteal rheumatism—the impregnation of the system by mercury, and the lurking and subtle influence of the syphilitic virus.

"In my clinical lectures for some years past, I have directed the attention of students to these facts, and impressed on them that the treatment of these cases of chronic periosteal rheumatism should be based on these principles: the first form of the disease requiring the iodide of potassium; the second form, the agency of alterative doses of some mild preparation of mercury. The following cases illustrate these views:

CASE 1.—Lydia E—, æt. 20, was admitted on the 5th October, 1839, suffering from rheumatic pains in the arms and legs, of nearly two months' duration. There is tumefaction of the periosteum above the olecranon of the left arm, another immediately below the tubercle of the tibia, and a third a little below the tibia—all highly painful and sensitive to pressure; the general health is much deranged; the countenance is anxious, and there is some emaciation; there is a diffuse

eruption of urticari on the inner part of the thighs, very troublesome at night, the itching of which, with the pains in the limbs, has deprived her of rest for some weeks past. She denies that she has ever had any venereal disorder, although she confesses to have led, for the last two years a dissolute life. A cicatrix exists in the left inguinal. She states that a year since she had a swollen knee, which confined her to her room for some weeks, and which, from her description, appears to have been an attack of acute rheumatic arthritis. Heart sounds natural; no pulmonary disorder. She was at first placed on mercurial alteratives—gray powder and Dover's powder, with aperient salines; and on the fourth day from admission, the state of the excretions being improved, she was ordered sarsaparilla, and the liquor of bichloride of mercury in drachm doses. The node on the olecranon was so painful, with some increase of temperature at the spot, that a few leeches were applied, with considerable relief to this local condition. The tibial nodosities were relieved temporarily by saline fomentations with the spongio piline.

On the seventh day from admission, although the periosteal inflammation had everywhere subsided, yet her suffering at night from the gnawing, erratic pains in the limbs continued very distressing.

About the fourteenth day, visible signs of improvement manifested themselves; the local swellings had totally disappeared, and the parts could be touched without pain. The rheumatic nocturnal accessions of pain also became less, and she slept comfortably, and her general appearance indicated approaching convalescence. On the twenty-first day from admission, she was free from all former symptoms, and on the twenty-fifth she was discharged convalescent. No symptoms of pytalism exhibited themselves.

CASE 2.—William C—, set. 49, a footman, was admitted October 3d, 1849, suffering from severe rheumatic pains in all his limbs, of near eight months' duration, increasing slowly in intensity to the present time. The general aspect of the patient is unhealthy; there is no emaciation. The functions of excretion appear pretty regular; tongue not unnatural; pulse moderate in force and frequency; there is anorexia; no thirst; considerable bodily debility and exhaustion; the heart sounds natural, somewhat feeble; no pulmonary disorder; the surface of the skin is everywhere natural; but on both tibia there are periosteal enlargements and irregularities, causing extreme pain on pressure; the fauces are quite free from any vestige of ulceration. He states he never had a sore throat in his life, and most earnestly declares that he never had any venereal disease; that he married early in life, and has always been a steady man; but that about this time last year, he suffered an attack of pleurisy and inflammation of the liver, for which he was salivated, and was thirty-six days under the influence of mercury, for several days excreting pints of saliva.

For the first two days he took some saline medicine with opiates at night; on the third day he commenced the iodide of potassium in ten-grain doses four times a day. A few leeches were applied to the right shin, and relieved the acute suffering of which he complained as becoming aggravated at night, and more particularly on attempting to walk or even place the foot on the ground. On the fifth day the ward-book

states that the local pains in the tibia had been relieved, and that the patient slept better, but that the tongue was coated and the bowels sluggish; the pulse 72, small and weak. On the seventh day the report states the nodosities to have disappeared, and that he bore the pressure of the finger without pain. The urine has been daily examined, and iodine detected in it. On the tenth day the general appearance of the patient had much improved; he complained of pain only in the ankles and shoulders; slept well; appetite natural. Between this day and the twentieth, a relapse of general rheumatic pains in all his limbs, with a sleepless night or two, occurred, but this passed away, and on the twenty first he is reported as nearly convalescent, his only complaint being debility. The iodide was discontinued, and he took quinine with much advantage, and was discharged convalescent on the twenty-eighth day.

"These cases have been selected out of thirty-one patients of chronic rheumatism, eight of whom had suffered syphilis, but had never taken mercury or been salivated, eighteen had been salivated for venereal or other diseases; and five had suffered neither syphilis or salivation, but presented strong, marked characteristics of the scrofulous diathesis. In the first class no benefit was apparent while the iodide was administered, but a rapid improvement became visible under small alterative doses of the bichloride of mercury. In the second class, which exhibited a previous impregnation of the system with mercury, the remedial influence of the iodide was in almost every instance well marked. In one or two of the third class, the effects were doubtful, and the greatest benefit was derived from chalybeates and cod-liver oil. The cases that have been selected for illustration, it is admitted, present the treatment in the most favorable light: in other of the cases similar results were obtained after a more protracted course of treatment: and some were complicated with disease of other organs; one or two with lung disease, which, while the relief obtained for the rheumatic disorder was sufficiently apparent, would not so forcibly illustrate the treatment as the cases just recorded."

Alkalies in the Treatment of Acidity of the Urine. By Dr. BENICE JONES. (*Medical Times and Gazette*.) Regarding the use of alkalies (we quote from a recent clinical lecture by Dr. Bence Jones,) two kinds of action may be distinguished.

The first is curative; the second palliative.

The curative action consists in the promotion of oxidation. in causing

gravel; the excessive acidity was checked; to remove it I advised him to go to Vichy. He went. For six weeks he took an alkaline hot bath for one hour each day. He drank from five to twelve half-pints of Vichy water, by which the urine was kept alkaline from fixed alkali, as the test paper which he sent me showed; and the day after he landed in England, on his return, the uric acid reappeared in the urine. He continued the alkaline treatment some time longer, and for months no red sand appeared.

You may perhaps say, Which of the alkalies is best, and to what extent should it be given? In consequence of the constant supply of soda in the common salt of food, I think potassa is most likely to be needed and to be beneficial. In the *Philosophical Transactions* for 1849, you will see a paper in which the action of potassa and tartrate of potassa on the urine is traced. The tartrate of potassa has a much more immediately sensible action on the urine than caustic potassa. The caustic potassa had, however, a decided action in neutralizing the acidity of the urine.

In the *British and Foreign Medical Review*, January, 1853, there is a paper by Dr. Parkes, on the action of potassa. He speaks of taking a drachm of liquor potassæ in two ounces of water, and two drachms in three or four ounces of water. If taken on an empty stomach, it increased the flow of urine,—urine which was faintly acid soon became alkaline. He says two drachms in only four ounces of water caused me epigastric pain and uneasiness, although it produced considerable temporary scalding of the mouth and throat, and without apparently producing any local effects on the stomach. This degree of dilution is not sufficient in most cases. There is a patient now in the Queen's ward, who complains of pain in the epigastrium and of great soreness of the mouth, caused by half a drachm in an ounce and a half of water; moreover, this quantity I have known cause blood to be vomited.

Of the carbonate of potassa, larger quantities may be taken without injury to the stomach. Induced by the promise of a certain crew, patients will take much stronger doses of a quack medicine than of a *Pharmacopœia* preparation of the same substance, recommended with an accurate statement of the probable benefit to be derived from its use. Thus, the so-called "constitution water" is only a strong solution of carbonated alkali, which patients will take for weeks. It is with great difficulty that they can be persuaded to take an equivalent quantity of bicarbonate of potassa for a few days.

That large quantities of alkali may be taken without any injury for long periods is proved by cases on record. Among others, you will see an account in vol. v. of the *Medico-Chirurgical Transactions*, p. 80, of a young lady who began with half an ounce of subcarbonate of soda daily. She progressively increased it to an ounce and a half, two ounces, two ounces and a half, and finally to three ounces. This last quantity caused vomiting, and in a few days had to be lessened. She continued two ounces and a half daily for many months: the urine was alkaline; specific gravity, 1016; the blood drawn during the treatment coagulated firmly; there was no appearance of any delinquency or impoverishment of it.

A very convenient form for taking the carbonate of soda exists in these soda lozenges, which consist of compressed carbonate of soda only.

Regarding the action of ammonia, I must refer you to experiments on the carbonate and tartrate given in the Philosophical Transactions for 1815. The chief result is, that ammonia does not act as an alkali on the urine: it may neutralize acidity in the stomach, but it does not affect the urine like fixed alkali.

To neutralize acidity alkalies should be given when the stomach is full, that is, from one to three hours after breakfast, and from one to six hours after dinner. The amount of acid in the stomach during healthy digestion appears from my experiments considerably more than would be neutralized by the usual doses of alkali. At least in some healthy persons half an ounce of carbonate of potassa will not neutralize the gastric juice, and not unfrequently twice as much alkali would be required.

You may ask, How is the quantity of alkaline medicine to be determined in these cases of excessive acidity, and how long is the alkaline treatment to be continued? The rule is this. The urine must be passed into a clean phial, and the sooner the red crystals appear on the glass, or can be detected by the microscope, the more alkali must be taken, and it must be continued as long as the uric acid crystals are formed. If no crystalization occurs in twenty-four hours the alkali may be stopped.

If, then, you find red sand in the urine, determine how soon the crystals form. The sooner the crystals occur the greater the acidity. The more alkali must be given, and the stricter must the dietetic rule be. If the red sand forms in the bladder, much larger quantities of alkali should be given than when it only forms after the urine has been passed; for each grain of red sand which forms in the bladder and kidneys may become the nucleus of a stone.

Singular Complication in Yellow Fever. By H. Foerster, M.D. (N. O. Med. News and Hosp. Gazette.) In the beginning of this year I saw for the first time a remarkable case of febris flava, which was the more interesting as I had previously heard of other similar cases from Drs. S. and B., head physicians of the San Carlos Hospital of this place. Mr. C., Captain of an American vessel, entered the Casa de Salud for

to it which render this case, as well as others mentioned to me by the gentlemen I have above referred to, so interesting and singular. In almost every case where this symptom has been observed death has ensued. Post mortem examination shows that the muscles, nerves, blood vessels and periosteum of the affected region have been destroyed by gangrena siccæ. What the cause of this singular lesion may be; why such destruction of one part should take place when the patient's general symptoms appear to be tolerably favourable, is a question for future investigation. There is an evident tendency to dissolution of the blood in yellow fever, and the gangrene may be owing to the septic condition of this fluid. The treatment of the symptoms of this singular affection, in the case to which I have reference, was entirely empirical, and based upon the results of the only two previously successful cases on record. Leeches were freely applied to the seat of pain and the bleeding encouraged by warm fomentations; afterwards the limb was enveloped in large cataplasms. Internally strong decoctions of Peruvian bark with muriatic acid, were administered. The pain in the limb ceased after a duration of two days, and the prognosis was pronounced favourable by physicians who had previously seen this affection. On the 29th, the patient began to take nourishment, and on the 7th of April was discharged cured.

Treatment of Rheumatism. By Dr. Chapin. (New-York Medical Times.) The treatment of rheumatism has of late years excited especial interest on account of the unsatisfactory results of the different modes heretofore pursued, and the dangerous complications attending the disease. It has been found that its duration has not been materially affected by the means used for its relief; and it has been a question with some whether or not complications of the disease have not been promoted by the treatment pursued.

Dr. Fuller, in his recent work upon rheumatism, regards the pathology of the disease as depending upon the presence of a "*materia morbi*," which he supposes to be an excess of lactic acid, owing to defective assimilation or suppressed perspiration. With the view to the neutralisation and elimination of this acid, he recommends the free use of alkalies, by which means he "hopes the average duration of an attack may be reduced from a month or six weeks, to ten days or a fortnight." Statistical tables must, in the present state of our knowledge, be relied upon to determine this result. During the attendance of Dr. Swett at the New-York Hospital, the present year, all the patients admitted with acute articular rheumatism were put upon the "alkaline treatment." The salt chosen for administration was the tartrate of soda and potassa, a neutral salt, possessing the property of rendering the urine alkaline.

The plan of treatment usually pursued was—if the patient presented himself with unusual excitement of the skin and pulse, to administer a mixture of sulphate of magnesia and tartarized antimony until the skin was relaxed and the pulse reduced to a more natural standard. The Rochelle salt was then directed in drachm doses, every two or three hours during the day time, till the urine was rendered alkaline, when it was gradually suspended. A lotion of carb. potass. ʒj. with opium

The average date of commencing improvement was seven days after commencement of treatment, coinciding, in the large majority of the cases, with the commencing alkalinity of the urine. The improvement was invariably permanent, and after the urine was rendered alkaline, *no new articulations were affected*, as a general rule.

The average period of convalescence was twelve days after admission, and the whole duration of the disease, including the period previous to admission, was twenty-two days. Of thirty cases treated by Dr. Swett, during April and May, 1853, during which time no uniform course of treatment was pursued, the average duration was five and a half weeks. One of the most gratifying results of the alkaline treatment was the diminished frequency of cardiac complications. Twenty-one of the twenty-five were free from any complication, three were admitted with aortic obstruction, and one with mitral regurgitation. Not one patient was attacked with any heart complication during the treatment of the disease. Comparing this result with the practice last year, it was found that four had mitral regurgitation, six aortic complication, and three suffered from pericarditis; thirteen in all, out of thirty.

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SURGERY AND SURGICAL PATHOLOGY.

On the Differential Diagnosis of Hydrocele and the diseases with which it may be confounded. By R. G. H. Butcher, Esq. (Dublin Journal of Med. Science.)

HERNIA.

Begins above.
Changeable in bulk.
Engages ring.
Feeling of weakness.
Can often feel intestines, or omentum.
Testicle at the bottom.
Opaque: in child sometimes transparent.
Base of tumor above.
Flatulence, dyspepsia.

VARICOCELE.

Soft, like earth-worms.
Changeable, like hernia.
Ring dilated often.
Testicle distinct.
Testicle wasted.
Tumor whole length of chore.
Tumor light.

VENEREAL TESTICLE.

Both engaged generally.
Tumor very heavy.
Hard all over.
Size moderate.
No fluctuation; sometimes small quantity of fluid.
Tumor slanting.
Painful to handling.
Solid contents.
Eruption or sore throat.

SCROFULOUS TESTICLE.

Round in form.
Never very large.

HYDROCELE.

Begins below.
Unchangeable.
Ring free.
Feeling of weight.
Can feel nothing.
Testicle at back part.
Often transparent.
Base of tumor below.
Bowels not deranged.

HYDROCELE.

Tense, elastic.
Unchangeable.
Ring closed.
Testicle indistinct.
Enlarged, if distinguishable.
Tumor at bottom.
Tumor heavy.

HYDROCELE.

One tunica vaginalis generally.
Tumor not so heavy.
Hard only at back part.
Often very large.
Fluctuation.
Tumor perpendicular.
Not painful.
Fluid contents.
None such necessarily.

HYDROCELE.

Oval in form.
Often very large.

FUNGUS HÆMATODES.

Tumor irregularly hard and soft, hardness predominating in early stages.

Shape, globular generally.

Rapid in growth.

Painful.

Opaque.

Elastic.

Chord becomes hard and knobby.

Pains up loins.

Health impaired.

Fungates.

HYDROCELE.

Uniformly smooth.

Oval generally.

Slow in formation.

Free from pain.

Transparent.

Fluctuating.

Chord sound.

No such pains.

Not so.

Never.

CANCER OF THE TESTICLE.

Hard, knobbed.

Small.

Round.

Painful on handling.

No fluctuation.

Chord knobby.

Adheres to scrotum.

Glands in groin enlarged.

Shooting pains.

Fever peculiar.

Fungates.

Death.

HYDROCELE.

Soft, smooth.

Large.

Oval.

Not so.

Fluctuation.

Chord soft.

Never.

Never engaged.

Never (in loins.)

No fever.

Never.

Never.

In hydro-sarcocele the testicle will be found hard, painful, irregular, large at the back part, with some fluctuation in front. Testicle distinguished in hydro-sarcocele, not so in hydrocele generally. Shooting pains on handling the former, not so in the latter. If obscure, the tumor may be tapped, and then the enlargement of the testis will be discovered, and the water small in proportion to the size of the tumor.

Radical Cure of Hydrocele. (Dublin Medical Press. From New York Journal of Medicine.) Prof. Willard Parker, of New York, recommends for the radical cure of hydrocele, the local application of the solid nitrate of silver; and he states that this possesses the following advantages over other operations: 1. The ease and safety with which it may be performed. 2. The less liability to severe inflammation. 3. The certainty of success. The following case illustrates the mode of operating, and the result of treatment:—"Mr. J., aged about 60, an Irishman, waiter by occupation, unmarried, had always enjoyed good health until April last, when he discovered an enlargement of the left scrotum. It had never previously been the seat of any difficulty. The tumor increased so rapidly that, within three weeks, it had become a great annoyance, and prevented him, simply from its size, from continuing at his business. At this time I first saw him, and such had been the rapidity of the growth of the tumor, that it had been mistaken for

hernia, and he was wearing a truss. On examination, however, its true character, that of hydrocele, was made out without difficulty ; a trocar and canula were accordingly introduced ; and a large quantity of water withdrawn ; and the patient dismissed. In about three weeks, he again applied for relief, and I proceeded to operate for his radical cure in the following manner : After drawing off the fluid contents of the tumour in the ordinary way, I introduced through the canula a common probe, the end of which was coated, for half an inch or more, with nitrate of silver. This extremity, thus charged with caustic, was carried lightly over the serous surface of the tunica vaginalis, in various directions, and then removed. The patient complained of some pain during this part of the operation. He was directed to keep quiet, for the pain and swelling consequent on the application of the caustic, and apply cooling lotions, should the inflammation be at all severe. He returned home ; but, as he suffered but little pain, and the swelling was slight, and as his services could not well be spared, he continued about his business without any interruption. The pain lasted three or four days, when it ceased altogether, leaving the scrotum of its natural size. In this condition, it has since remained, with no symptoms of a return of the hydrocele, the cure having been complete."

A New Operation for Ununited Fractures. By Mr. JORDAN. (Med. Times and Gaz.) The method of producing union of an ununited fracture, by the operation of resection, has been lately modified by Mr. Jordan, who founds his method upon the following experiments of Syme.

The first experiment consisted in exposing the radius of a dog and removing an inch and three quarters of it, along *with* the periosteum, and in the other leg, removing a corresponding portion without the periosteum. In six weeks, the cut extremities of the bone which had been deprived of periosteum had only united by a small band of ligamentous texture ; whereas, in the other, which had retained the periosteum, the bony union was complete.

In the case of George Dickens, admitted into the Manchester Infirmary, with ununited fracture of the humerus, Mr. Jordan performed the following operation. He first made an incision down to the fractured part, then separated the periosteum from the extremities of the bone ; he proceeded to saw off the ends of the bone, leaving the periosteum, and lastly, put the ends of the bone in apposition and replaced the periosteum. This case was then treated as one of compound fracture.

This man had been operated on before, by a surgeon who attempted Diefenbach's method, by pegs, but union of the fractured bones did not ensue.

New Mode of Treatment for Delayed or Non-Union of a Fractured Humerus. By Dr. F. H. HAMILTON. (Buffalo Med. Jour.) It has been observed by surgeons that non-union results more frequently after fractures of the shaft of the humerus, than after fractures of

the shaft of any other bone. This observation is confirmed by my own researches.

Comparing the humerus with the femur, between which, above all others, the circumstances of form, situation, &c., are most nearly parallel, and in both of which non-union is said to be relatively frequent, I find that of forty-nine fractures of the humerus, four occurred through the surgical neck, twelve through the condyles and twenty-nine through the shaft. In one of the twenty-nine, the patient survived the accident only a few days. In four of the remaining twenty-eight, union had not occurred after the lapse of six months, and in many more was it delayed considerably beyond the usual time. Two of the four were simple fractures, and occurred near the middle of the humerus; the third was compound, and occurred near the middle also; the fourth was compound, and occurred near the condyles.

This analysis supplies us, therefore, with four cases of non-union, from a table of twenty-eight cases of fractures through the shaft.

Of eighty-seven fractures of the femur, twenty occurred through the neck, one through the trochanter major, and one through the condyles. The remaining sixty-five occurred through the shaft and generally near the middle, and in not one case was the union delayed beyond six months.

To make the comparison more complete, I must add that of the twenty-eight fractures of the shaft of the humerus, six were compound; and of the sixty-five fractures of the shaft of the femur, six were either compound, comminuted, or both compound and comminuted. The six compound fractures of the shaft of the humerus, furnished two cases of non-union. The six cases of either compound or comminuted, or compound and comminuted fractures of the femur, furnished no case of non-union.

I beg to suggest to the Society what seems to me to be the true explanation of these facts.

It is the universal practice, so far as I know, in dressing fractures of the humerus, to place the forearm at right angles with the arm. Within a few days, and generally, I think, within a few hours, after the arm and forearm are placed in this position, a rigidity of the muscles and other structures has ensued, and to such a degree, that if the splints and sling are completely removed, the elbow will remain flexed and firm; nor will it be easy to straighten it. A temporary false ankylosis has occurred, and instead of motion at the elbow joint, when the forearm is attempted to be straightened upon the arm, there is only motion at the seat of fracture. It will thus happen that every upward and downward movement of the forearm will inflict motion upon the fracture, and inasmuch as the elbow has become the pivot, the motion at the upper end of the

when placed at right angles with the arm and encumbered with splints and bandages, that even when supported by a sling, it settles heavily forwards, and compels the arm dressings to loosen themselves from the arm in front of the point of fracture, and to indent themselves in the skin and flesh behind. By these means the upper end of the lower fragment is tilted forward. If the forearm should continue to drag upon the sling, nothing but a permanent forward displacement would probably result. The bones might unite, yet with a deformity.

But the weight of the forearm under these circumstances is not uniform, nor do I see how it can be made so. It is to the sling, that we must trust mainly to accomplish this important indication. But you have all noticed that the tension or relaxation of the sling depends upon the attitude of the body, whether standing or sitting—upon the erection or inclination of the head—upon the motions of the shoulders, and in no inconsiderable degree upon the actions of respiration. Nor does the patient himself cease to add to these conditions by lifting the forearm with his opposite hand whenever provoked to it by a sense of fatigue.

This difficulty of maintaining quiet apposition of the fragments while the arm is in this position, at whatever point the arm may be broken, becomes more and more serious as we depart from the elbow joint, and would be at its maximum at the extreme upper end of the humerus, were it not that here a mass of muscles, investing and adhering to the bone, in some measure obviates the difficulty. Its true maximum is therefore near the middle, where there is less muscular investment, and where, on the one hand, the fracture is sufficiently remote from the pivot or fulcrum to have the motion of the upper end of the lower fragment multiplied through a long arm, while on the other hand it is sufficiently near to the armpit and shoulder to prevent the upper portion of the splint and arm dressings from obtaining a secure grasp upon the lower end of the upper fragment.

It must not be overlooked that the motion of which we speak belongs exclusively to the lower fragment, and that it is always in the same place, forwards and backwards; but especially that it is not a motion upon the fracture as upon a pivot, but a motion of one fragment to and from its fellow. This circumstance I regard as important to a right appreciation of the difficulty. Motion, alone, I am fully convinced, does not so often prevent union as surgeons have generally believed. It is exceedingly rare to see a case of non-union of the clavicle. Of forty-seven cases of fracture of the clavicle which have come under my observation, and in by far the greater majority of which considerable overlapping and consequent deformity has resulted—of this number only one has resulted in non-union, and in this instance no treatment whatever was practised, but from the time of the accident the patient continued to labor in the fields and hold the plow as if nothing had occurred. I have therefore seen no case of non-union of the clavicle where a surgeon has treated the accident. Indeed, what is most remarkable, its union is more speedy, usually, than that of any other bone in the body, of the same size. Yet to prevent motion of the fragments in a case of fractured clavicle with complete separation and displacement, except where

the fracture is near one of the extremities of the bone, I have always found wholly impracticable. Whatever bandages or apparatus I have applied, I have still seen always that the fragments would move freely upon each other at each act of inspiration and expiration, and at almost every motion of the head, body or upper extremities. It is probable, gentlemen, that you have made the same observation.

From this and many similar facts I have been led to suspect, for a long time, that motion has had less to do with non-union than was generally believed.

I find, however, no difficulty in reconciling this suspicion with my doctrine in reference to the case in question; and it is precisely because, as I have already explained, the motion, in case of a fractured humerus, dressed in the usual manner, is peculiar. In a fracture of the clavicle through its middle third, (its usual situation,) the motion is upon the point of fracture as upon a pivot; although, therefore, the motion is almost incessant, it does not essentially, if at all, disturb the adhesive process. The same is true in nearly all other fractures. The fragments move only upon themselves, and not to and from each other.—I know of no complete exception but in the case now under consideration.

Aside of any speculation, the facts are easily verified by a personal examination of the patients during the first or second week of treatment, or at any time before union has occurred, both in fractures of the humerus and clavicle. The latter is always sufficiently exposed to permit you to see what occurs, and as soon as the swelling has a little subsided in the former case, you will have no difficulty in feeling the motion outside of the dressing, or perhaps in introducing the finger under the dressings sufficiently far to reach the point of fracture. I believe you will not fail to recognize the difference in the motion between the two cases.

Such, gentlemen, is the explanation which I wish to offer for the relative frequency of this very serious accident—non-union of the humerus.

I know of no other circumstance or condition in which this bone is peculiar, and which therefore might be invoked as an explanation. Overlapping of the bones, the reason assigned by some writers, is not sufficient, since it is not peculiar. The same occurs much oftener, and to a much greater extent, in fractures of the femur, and equally as often in fractures of the clavicle; yet in neither case are these results so frequent. Nor can it be due to the action of the deltoid or any other particular muscles about the arm, whether the fracture be below or above their insertions, since similar muscles, with similar attachments on the femur and on the clavicle, tending always powerfully to the separation of the fragments, occasion only deformity, but not non-union.

If I am correct in my views, we shall be able sometimes to consummate union of a fractured humerus where it is delayed, by straightening the forearm upon the arm, and confining them to this position. A straight splint, extending from the top of the shoulder to the hand, made of some firm but moulding material, and made fast with rollers,

will secure the requisite immobility to the fracture. The weight of the forearm and hand will only tend to keep the fragments in place, and if the split and bandages are sufficiently tight, the motion occasioned by swinging the hand and forearm will be conveyed almost entirely to the shoulder joint. Very little motion indeed, can in this posture be communicated to the fragments, and what little is thus communicated, is a motion which experience has elsewhere shown not disturbing or pernicious, but a motion only upon the ends of the fragments as upon a pivot.

I do not fail to notice that this position has serious objections, and that it is liable to inconveniences which must always, probably, prevent its being adopted as the usual plan of treatment for fractured arms. It is more inconvenient to get up and lie down, or even to sit down, in this position of the arm; and the hand is liable to swell. But I shall not be surprised to learn that experience will prove these objections to have less weight than we are now disposed to give them. Remember, the practice is yet untried—if I except one case, and in which case, I am frank to say, these objections scarcely existed. The swelling of the hand was trivial, and only continued through the first fortnight, and the patient never spoke of the inconvenience of getting up or sitting down, or even of laying down.

On Cleft Palate and Hare Lip. By Mr. JOHN FERGUSSON. (Dub. Med. Press.) Mr. Fergusson has lately given a very interesting clinical lecture on the subject of "Cleft Palate," more immediately in its several relations with hare-lip, which he seems to think connected, in no indirect mode, with this obstinate but not less curable congenital deformity. After some preliminary observations, as to the immense number of these cases, with the crowds of the lame, the halt, and the blind, now brought to King's College Hospital, the lecturer alluded to the curious fact, that the congenital defect in cases of hare-lip is generally on the left side of the body. "One cannot well tell," said Mr. Fergusson, "why or how it should be so. There is another curious point, too, you never see hare-lip in the mesial line. I have conversed with many intelligent surgeons as to this peculiarity. You may have the cicatrix of the old hare-lip in the mesial line, but when I have pressed the matter on their notice, they have each individually admitted it. Now why should we have this curious malformation or arrest of development, at the left side! I think, also, I have noticed, that the 'hare-lip' is, in some, not in any indirect way connected with the intra-uterine formation, and closing up of the nostril

because the cicatrix of hare-lip is in the centre; *ergo*, as Dogberry would say, the hare-lip is in the centre. You all recollect the old legend of Joe Miller, of the gentleman who ate the horse and saddle! A patient, it seems, was once in a bad fever, and rigidly interdicted everything but water-gruel. The doctor and his pupil coming one day, found him, however, much worse. "Oh, ho!" said the doctor, "you have been eating oysters;" the poor patient admitted the fact. "How on earth did you diagnose that?" said the pupil, when they left the house. "You fool, didn't you see the shells under the bed." This is our cicatrix. A few days after the pupil went alone to pay an evening visit, the fever was at its height, the poor man raving awfully, there was a saddle, somehow, flung accidentally under the bed, our pupil thought he would improve his former clinical instruction. "The man is very bad," he said, with a patronising grace, "but he must have eaten part of a horse and some stirrup irons!" There is a good deal of this horse-and-stirrup irons sort of arguing, I am sorry to tell you, in our modern surgery. I believe it is sometimes called "post hoc, ergo propter hoc," but you must be on your guard against it. I have seen a great number of curious cases of hare-lip, but I cannot call to recollection any immediately in the mesial line; you will have a hideous deformity of even both sides, with a sort of velum or nipple between. We saw, lately, a case of very marked or exaggerated hare-lip, in which the rent went back by the nostril to the edge of the masseter muscle. In this case there is an appearance as if the lip was torn open at one side, in the womb, before the child was born, the cheek and mouth put in the wrong place. Some ingenious person thought that the poor mother was over fond of cats, and that the child should have a cat's mouth and whiskers. Another instance of loose reasoning.

We need not go into the various details of the operation for hare-lip only, as they are connected, very often with further deformity of the palate, of which I am more anxious to speak. Hare-lip may be produced by a wound, but is generally, it need scarcely be remarked, a congenital malformation; sometimes we have one fissure to deal with, at other times two. In some cases the fissure extends right through the nostril, which is widened and much deformed; in more formidable cases still, as too often met, the cleft in the upper maxilla and in the palate bones make, apparently, one large cavity. We have seen all these cases here this session. Double hare-lip is mostly always accompanied by this fissure in the palate bones. Mr. Fergusson's mode of operation is very simple and complete; it is very much like the plan adopted by Liston; and

very moveable, and may disturb the needles. To prevent this, he used also, a sort of pad or truss, made of ordinary elastic materials, which, coming round the head, presses on the lip immediately external to the fissure. The utmost exactness is used in the use of the pins, and after a week, we have seen most remarkable and perfect cures.

If the case, as very often happens, is one of double hare-lip, and the intervening portion of lip is of any extent, it should be saved; we have then two operations at different periods. Sometimes a large tooth will stick out and add to the deformity (this, in a late instance of formidable hare-lip in a man, cured in a week at King's College Hospital, presented quite a tusk like a wild boar, but was at once extracted under chloroform, the man appearing quite another looking individual altogether on leaving the hospital.) The complication of hare-lip with cleft palate is not easy to understand in a physiological point of view; it is useful to recollect, however, that where the latter is confined to the maxillary bone alone, it often closes in after the operation of hare-lip; it would seem, like the fontanelle, perhaps, as if the parts only required a little time to complete the process of ossification. If the fissure extend along the palate bones and velum, such an occurrence is less likely to happen.

Here we have CLEFT PALATE to deal with, quite a different state of things, but one which Mr. Fergusson has paid particular attention to, and described lately in full detail to his class. The point on which he laid greatest emphasis, "a full and free division of the levator palati, together with the palato-pharyngeus muscles, by a knife of his own peculiar construction, an operation which he thinks not at all impossible, as represented by some unsatisfied writers. In the case lately operated on, the operation has been completely successful, but instead of erring with Joe Miller on the side of credulity as to stirrup irons, and Joe Miller is buried in King's College Hospital, rather a presiding genius here, some men in London would doubt the existence of the cross on St. Paul's, if it suited private journal motives, or sold half a dozen more cheap diplomas. Mr. Syme is free from this, but it is in London practical surgery meets the largest amount of stumbling blocks in its way.

Mr. Fergusson first recommends the levator palati and palato-pharyngeus should be divided by cutting on each side deeply into the pterygoid fossa with a peculiar triangular knife, which he uses, not unlike a dentist's scalpel, completing the operation, if necessary, with a pair of scissors. Mr. Syme objects to the bleeding state of the parts; but this Mr. Fergusson obviates by the patient, as in the similar process of drawing a tooth and lancing the gums, washing the parts out with a little ordinary lukewarm water. The patient's throat, as remarked by Syme,

either; and here we might observe they are got in very easily, and Mr. Fergusson uses only two. Nor does there seem any greater difficulty in this portion of the operation than placing ligatures on internal cavities of any sort, ligatures in vesico-vaginal fistulae, &c., as we see them now and then applied. Mr. Fergusson's long experience of these cases is, no doubt, of vast advantage; and in explaining it he used a very happy figure, that as in a dream all a lifetime occurs in a second, according to Lord Brougham, so in an operation all a lifetime of surgical shifts and expedients occurred to the mind in an instant of time.

Mr. Syme thinks the alleged advantage of dividing the muscles of the palate to promote union, is an "entire delusion of those who believe in it." A partial division producing no good effect, while a complete one he fancies is not only impracticable, but if accomplished would be useless. This, from the remarks made by Mr. Fergusson, as well as from watching the steps of the operation lately, which Mr. Cusack also witnessed in London, we think is "coming it too strong." Mr. Fergusson was obliged to make several incisions, but the girl bore it all very well. "The palato-pharyngeus and levator palati are not slender thread," Mr. Syme says. It scarcely required Mr. Syme to tell us this; yet one could not help thinking the operator had thoroughly separated their attachments; and after all, it is perhaps splitting a hair, as to whether we are to put off operating at all, or do the best we can for the patient. Mr. Fergusson, in operating, remarked the knife he uses is not at all that represented by Mr. Syme; it is neither clumsy nor imperfect for its purpose, and seems completely to divide the "lax and yielding bellies of the palatal muscles." But Mr. Syme appears to us to halt a little in his logic, or puts one in mind of the soldier of the triangles who was not satisfied if the cat o' nine tails was applied either high or low. "There can be no doubt that if the palate admitted of being rendered perfectly lax and flaccid, it would then become very unfavorably situated (he says,) either for the performance of the operation or for a satisfactory result, since there is nothing so opposed to sound union between the surfaces of a wound as the soft and flabby state," &c. &c. So that whether the muscles are tense or flaccid, it is all the same to the Highland Chief of the Edinburgh Infirmary.

The questionable propriety of Trephining for Intra-Cranial Suppuration. By Mr. Hewitt. (Rankin's Abstract.) The following important remarks occur in an "Analysis of the cases of injuries of the head, examined after death in St. George's Hospital, between January, 1841, and January, 1851:"

Suppuration between the bone and the dura-mater appears not only to have been much more frequent, but also very much more commonly confined altogether to this situation formerly than it is at the present day.

In the second section of "Injuries of the Head," Pott gives twelve cases of scalp-wound followed by subsequent mischief beneath the bone, in seven of which the suppuration was altogether confined to the outer surface of the dura-mater. In the various cases which I have noticed of this affection, I have never seen a single instance in which the suppura-

tive inflammation was thus limited; in every case which I have examined, inflammation has at the same time existed beneath the dura mater. It cannot be said that this very marked difference depends upon the amount of inflammation, or the quantity of matter thrown out, for in several of Pott's cases not only was the quantity of matter thus situated very large, so large indeed that it was thought impossible for the patients to recover, but the inflammatory process had been going on for some days, as shown by the symptoms and the quantity of matter evacuated from the spot where the trephine was applied; whereas, in several of the cases which I have noticed, although the extent of the mischief between the bone and the dura-mater has been comparatively trifling, still the diffuse inflammation beneath the membrane has been most extensive. In every one of the eight cases of suppuration between the bone and the dura-mater occurring in this decennium, puriform effusion was found within the cavity of the arachnoid, and appeared to have spread from the external to the internal parts. That this difference does not depend upon locality, is evinced by the results of similar cases in different places and in different countries.

Suppuration between the bone and the dura-mater naturally leads to a practical question of the utmost importance, I mean the application of the trephine. Formerly, and especially in Pott's hands, it appears in such cases to have been one of the most successful operations. Mischief was suspected under the bone, the trephine was applied, matter was found, in some cases in large quantities, and evacuated, and the patients recovered without any further difficulty. Such is the history of several of Pott's cases; but, unfortunately, we have met with no such success. It has never yet fallen to my lot to see a single instance in which the application of the trephine has, under such circumstances, had a successful issue. In every case in which I have seen the operation performed, the patient, notwithstanding the evacuation of the matter, has died of diffuse inflammation of the membranes. Still, notwithstanding that our efforts have been so unsuccessful, we do not hesitate, whenever the various symptoms lead to the diagnosis of matter between the bone and the dura-mater, to apply the trephine, and we give the patient the only chance, however small that may be, of recovery; but, on the other hand, we consider the application of the trephine altogether useless in cases of intra-cranial suppuration, where the symptoms are those of acute inflammation of a diffuse kind, in which the signs of compression are generally but slightly marked.

Of the eight cases of suppuration between the bone and the dura-mater without fracture, noted in this decennium, five were not operated upon, and three were trephined. In the first class, in which no operation was performed, the symptoms of compression were either but very slightly marked, or altogether absent.

* * * * *

Purulent infection was observed in fourteen out of the twenty-three fatal cases of scalp-wound, and, although developed in other injuries, in none is it more frequently so than in those of the head, and that, too, in cases where the injury has apparently been of a trivial nature. The well-known fact that this disease is found especially in injuries involving

the osseous system, will serve to explain the frequency of the development of this most formidable complication in accidents about the head, where the bones are not only abundantly supplied with cancellous tissue, but where are also found venous canals much larger and much more numerous than in any other part of the skeleton. That no cases of this kind should have fallen under Pott's notice is surprising, and the more so, as Desault and others were at about the same period directing the attention of surgeons to this affection, as one of the most common consequences of injuries of the head.

It has been thought by some foreign pathologists, that the early application of the trephine made by Pott and other surgeons, in cases of exposed and contused bone, might, in some measure, serve to explain why these practitioners had not met with more cases of purulent infection in injuries of the head. M. Chassaignac especially thinks, that the removal by the trephine of the contused bone before suppuration has taken place in its diploë, destroys the source from whence the secondary mischief is for the most part derived; but such an explanation can scarcely be admitted as a valid one; for in how many cases of purulent infection after amputation do we not find extensive suppuration in the cancellous tissue of the bone?—cases in which there had been no injury, and in which suppurative inflammation did not exist in the bone previous to the removal of the limb.

MATERIA MEDICA AND THERAPEUTICS.

On the Use of the Solution of Magnesia (Bi Carbonate of Magnesia.) By M. C. Delacre, Pharmaceutist at Brussels. (Translated from the *Revue Medical-Chirurgicale de Paris*.) The action of medicines depends most generally on the form under which they are administered. So that the practitioner ought always, not only to remember under what forms the active principles exhibit themselves, but further, how it is best to prescribe them under particular circumstances.

The carbonate of magnesia (*magnesei calcini*) if employed as an absorbent of acids which are in excess in the digestive viscera, is of the number of medicines which are misapplied.

This powder is too light, and has too great a volume, not to disgust patients sooner or later.

Furthermore, it cannot form concretions in the intestines, as is done by magnesia in powder. It acts in a more prompt, more efficacious manner, and in doses infinitely smaller.

The solution of bicarbonate of magnesia, possesses all the advantages of the calcined, without partaking of any of the disadvantages which characterize the latter. It subdues the acidity of the stomach, purges without producing colic, by producing an alkaline effect upon acids formed by impaired digestion, and prevents milk from coagulating too promptly in the stomach of infants at the breast.

A wine-glassful is the dose for an adult, for a child from two to six years of age, a tablespoonful, and for a young child, from one to two tablespoonfuls.

Taken in this way the magnesia acts as an absorbent.

If the purgative effect of this medicine alone is desired, it should be taken with lemon juice. A purgative lemonade of citrate of magnesia will thus be formed, but it must not be forgotten, that taken in this way, we are acted upon by a salt which has not the power of saturating the acids produced from the stomach, as would be done by the solution without this addition.

This medicine, it will be seen, is destined to enjoy an extensive reputation. It should not fail to be known by the practitioners of the continent, as it is to the English physicians, so that calcined magnesia may be completely abandoned.

On the Internal Administration of Chloroform in Fever. By Dr. Gordon, physician to the Hardwicke Fever Hospital. (Dublin Hospital Gazette.) The subjoined cases are related for the purpose of showing the beneficial action of chloroform in allaying the insomnia and nervous irritation of fever.

CASE 1.—Patrick Dempsey, aged 25, was sent from Santry to the Hardwicke Hospital, on the 8th of December; he was then eleven days ill of fever; his body was covered with dark-colored maculæ; his pulse was 110, and very weak, his speech muttering and indistinct; he had subsultus in both upper and lower extremities. His head was shaved; he was ordered the bark mixture of the Hospital, and half a pint of wine. Late in the evening he began to rave violently, and could not be induced to remain in bed; he was ordered large doses of hyoscyamus and the back of his head was blistered; he was so violent as to require, the use of a straight waistcoat all night.

December 9th. Has not slept since admission. Pulse, 132; very weak. He continues constantly muttering and raving. Tongue dry and brown; eyes slightly suffused; head not very hot; respiration short

continued for nine hours. He awoke perfectly sensible; the subsultus had ceased, and his pulse had fallen to 111. He continued to improve, and in a few days was convalescent.

In this, and other similar cases, chloroform acted by producing *anæsthesia* of the sensory nerves, and exerting a paralyzing influence on the muscular fibre; and this it appears to effect without depressing or deranging the nervous force, as is the case with sedatives in general, while it is altogether free from the objection of causing depression of the action of the heart, as is the case with some special sedatives. My colleague, Dr. Corrigan, has just treated a somewhat similar case by the internal administration of chloroform. I had an opportunity of daily witnessing the progress of the case; and, by his permission I here append it, as—

CASE 2.—Denis Beahan, aged 20, a porter from High Street, was admitted into the Hardwicke Hospital, January 4th, 1854, the fifth day of his illness.

On the sixth day he was thickly covered with bright maculæ. His tongue was loaded, but moist; his pulse 112; respiration 22; no abnormal sound in the lungs; no tenderness of abdomen. He is reported not to have slept for two nights. His eyes are red and injected, and his head hot.

His head was shaved and cold lotion applied.

Seventh day. Pulse 116; respiration 28; slept but little.

Eighth day. Pulse 120; very feeble; respiration 32; head hot; constantly raving, and getting out of bed; no sleep; subsultus of hands; tongue dry; great difficulty of utterance.

Vesicatorium nuchæ;

Eight ounces of wine.

Tenth day. Pulse 130, weak; raving continually; difficult to restrain; requiring the straight waistcoat; constant talking, no sleep; tongue brown and dry in centre; thirsty; eyes very congested, pupils dilated.

Chloroform was now administered by inhalation, without any other effect than the pulse being slightly reduced in number. The patient was in no way quieted by it. Four leeches were now applied to the temples without any good effect. At 5 p. m. he took 3ss. chloroform by the mouth, and continued it every second hour till 11 p. m., when, as he did not sleep, and the delirium continued, he got the same dose of chloroform every hour through the night. At 3 a. m. he was somewhat quieter, but the same dose was continued every hour till 8 a. m.

Eleventh day. 10 a. m. Much quieter, but has not slept. Pulse

"In the above case," writes Dr. Gordon, "the chloroform was longer in producing its effects than in any instance in which I have as yet used it. We learn from it, however, that we are not to be discouraged by the apparent failure in the first dose or two in procuring sleep, for as in the present case, although actual sleep may not be at once procured, we may expect that a state of calm and quietness will be induced, which will soon be followed by 'nature's sweet restorer, balmy sleep.' We learn also from this case that the inhalation of chloroform is, to say the least, useless in procuring sleep in cases of cerebral excitement by fever. I had, on one occasion before, in the Hardwicke Hospital, fully tried this mode of administering it; its inhalation was followed by general convulsive movements, very similar to an epileptic seizure, and I have not since administered it by inhalation in any similar case. Dr. Corrigan carefully tried the effect of inhalation three times, in the above case, each time without any good effect."

On the Use of Tincture of Iodine to prevent pitting in Small-Pox. By Dr. Crawford, Physician to General Hospital at Montreal. (Montreal Medical Chronicle.) For upwards of nine years Dr. Crawford has been in the habit of using a saturated alcoholic tincture of iodine as a local means of allaying irritation and of preventing pitting, and he now adduces considerable evidence in support of this practice. Once or twice a day he paints the face, or any other part where it may be necessary, with this tincture, and this he continues to do from the first appearance of the eruption until the pustules are matured. The pain, which is the immediate effect of the application, is said to subside speedily, and to abate in severity after the first few trials. But this is rarely heeded; on the contrary, the relief to the itching is so gratifying to the patient that he frequently requests the extension of the application to other parts.

Dr. Crawford relates several cases out of his own practice, of which the two subjoined are specimens; and he also appends the testimony of Dr. Bérnin, of Cornwall, (U. S.,) which testimony is of very considerable value.

On the Culture of the Opium Poppy and the Preparation of Opium in the Benares Opium Agency. By W. C. B. EATWELL, M.D. (Extracted from the selection of Records of the Bengal Government.) We note a few interesting statements from the above paper, which appears in an article in the late number of Hooker's Journal of Botany, "on the contents of the Museum of Economic Botany attached to the Royal Gardens at Kew," by Sir W. H. Hooker. (H. W. R. For Charleston Med. Journal and Review)

the Benares agency was 107,822* beegahs, equal to about 66,561 acres. The lands selected for this cultivation are usually in the vicinity of villages, where facilities for manuring and irrigating are greatest. The time for sowing the seed is between the first and fifteenth November, and the plant takes about three months and a half to reach maturity. The usual amount of production varies from twelve to fifteen pounds per beegah, or about twenty pounds per acre, though under favourable circumstances of soil and season, this amount is sometimes doubled. As soon as the capsules reach their utmost state of developement, about the size of a duck's egg, the process of collection commences, which extends from about the middle of February to the middle of March.

At about three or four o'clock in the afternoon the operatives repair to the fields and scarify the capsules with a sharp instrument called a *nushtur*. The scarifications are very superficial and only traverse the thin pericarp of the capsule. Each capsule may be scarified from two to eight times, according to its size, an interval of a day or two being allowed after each operation.

The capsules having been scarified in this manner, the collection of the juice is made early the next morning, by means of a small iron instrument, resembling a concave trowel, by which the juice is scraped from the surface of the scarifications.

The juice when brought home is placed in a shallow earthen vessel, tilted to such a degree that the fluid portion may drain off. After separating all this fluid, or *pussewah* as it is called, the opium now requires frequent attendance on the part of the operator. It is daily exposed to the air, but never to the sun, and is regularly turned over every few days in order to insure a uniform dryness in the mass, and this process is persevered in for a month or more, or until it has nearly reached to standard consistence.

Standard Opium, according to the Benares regulations, is opium which, on being subjected to a temperature of 200° fahrenheit, until every thing volatile is driven off, shall have a residue of 70 per cent.

The opium on its arrival at the factory is then subjected to the native opium examiner or *Purkhea*, who very readily detects whatever impurities or adulterations there may be. The colour of well prepared opium is a deep dull brown when viewed in mass, which becomes a bright chestnut brown when a small portion of the drug is spread in a thin layer upon a white surface. It adheres to the fingers and draws out to a moderate extent, breaking with a ragged fracture.

The last operation of bringing the opium to a marketable state is the

The following are the results which Dr. Eatwell obtained from the analysis of fresh juice collected in February, 1850 :

2000 grains of freshly collected juice, subjected to experiment on the day of collection, yielded,		grs.
Morphia,	- - - - -	11.1
Narcotine,	- - - - -	32.7
Other matters soluble in alcohol, codeia, meconic acid, &c.		54.0
Dry mucr insoluble in alcohol, lignin, caoutchouc, &c.		225.2
Water and volatile matter separable at a heat of 200° fahrenheit,	- - - - -	1210.0
Total,	- - - - -	2000.0

It is a curious fact "that among the thousands of individuals, cultivators and employes, with whom the factory is filled during the receiving and manufacturing season, no complaints are ever heard of any injurious effects resulting from the influence of the drug, whilst they all remain quite as free from general sickness as persons unconnected with the general establishment; in fact, if any thing, more so. It occasionally happens that a casual visitor complains of giddiness or headache, but the European officers employed in the department, who pass the greater part of the day with the thermometer between 95° and 105° farh. amongst tons of the drug, never experience any bad effects from it. The native purkhea (examiner) sits usually from 6 A. M. to 3 P. M., daily, with his hand and arm immersed nearly the whole time in the drug, which he is constantly smelling, and yet he feels no inconvenience from it." "In the large caking vats, men are employed to wade knee deep through the drug for several hours during the morning, and they remain standing in it during the greater part of the rest of the day, serving out the opium by *armfuls*, their bodies being naked with the exception of a cloth about the loins. These men complain of drowsiness towards the end of their daily labours, and declare they are overpowered early in the evening by sleep, but they do not complain of the effects as being either unpleasant or injurious."

"Infants a few months old may be frequently seen lying on the opium-besmeared floor under the vats, in which dangerous position they are left by their thoughtless mothers; but, strange to say, without any accident ever occurring. Here are abundant facts to show that the health of those employed in the opium factory and in the manipulation of the drug, is not exposed to any risk whatever, whilst the impunity with which it is handled, by hundreds of individuals for hours together, proves that it has no endermic action."

Hydro-Ferro Cyanate of Potassa as a febrifuge. By Dr. JAUSSAIL. (From the Journal de Med. of Toulouse. Translated from the *Gazetta Medica Italiana Toscana*, by Eds. of Charleston Med. Journal and Review.) Clinical observations have not yet fully pronounced upon this medicine introduced into therapeutics by M. Baud. Chemistry pronounced it unsuitable, but what is this to the testimony afforded by experience. M. Gaussail has experimented with it without regard to its chemical re-agency, and the consciencious report which we have under our eyes

furnish us with the results. The yellow hydro-ferro cyanite of potassa was employed in twelve cases of intermittent fever of various types and eight cures were effected, one being doubtful. This medicine whether employed during the paroxysm or apyrexia were well tolerated and produced no appreciable inconvenience, except in three cases slight abdominal pain and diarrhoea. It appears that its action is exerted specially upon the nervous phenomena, the element of pain above all having been advantageously modified. This action upon innervation was exhibited as well in those cured as in those in which the paroxysms could not be completely. The ordinary dose is from one gramme to one gramme and fifty centigrammes daily taken in pills of ten centigrammes every half hour. The conclusions of the author are the following: 1st. The febrifugal influence determined by M. Baud seems to owe its action solely to the hydro-ferro cyanate of potassa. 2d. That this saline compound, perfectly definite in its chemical composition, and though only borrowed from the arts and existing as a chemical reagent, can be administered as a medicine and furnish advantageous results, specially in the intermittent pyrexias *not paludal*; and the modifications which it is susceptible of producing in these last also can favour their definitive cure in doses less elevated than the sulphate of quinine. 3d. Taking into account the sedative action produced upon the circulation by this medicine, we are induced to think that it will furnish resources to the therapists in other morbid affections than intermittent fever.

Bromuret (Bromide) of Potassium—Its sedative action upon the generative organs. By Dr. THIELMANN. (Translated from the *Gazetta Medica Italiana Toscana*. For *Charleston Medical Journal and Review*.) It is stated in the *Gazetta Hebdomadaire* that Dr. Thielman uses the bromuret (bromide?) of potassium with great advantage in the priapism which accompanies certain forms of blenorragia. Besides the internal use of this means he advises the local application to the penis of compresses soaked in tepid water covered with taffetas of gum to prevent evaporation. This means seems to have succeeded in the hands of the Russian Practitioner against satyriasis and nocturnal pollutions. The formula is the following:

Bromuret (bromide) of potassium 1 gr. 25—2 gr.

Pulverised sugar, 6 gr.

Mix and divide into 12 equal parts of which take one every two hours.

New Method of curing Gonorrhoea Disease By Dr. LANGELOTT

hæmorrhage. The febrile reaction, if we may express it, is nothing; but the local irritation and inflammation is very often active and painful, often depending in great part upon individual susceptibility. To provoke the gangrenous state of the cancerous part Landolfi uses a paste composed of chloruret of bromine, chloruret of gold, and chloruret of antimony, equal parts; mixed up with common farrina and a little water.

This paste is applied immediately to the cancerous part; if the epidermis is still untouched it is first necessary to destroy it with the azotate of silver, particularly when it is thin. The action of the caustic depends upon the thickness of the paste applied. It has been observed that if the statum of paste has a thickness of two millimeters the caustic operates to a depth of a centimetre and a half. When the part which surrounds the cancer is much inflamed, Landolfi advises the application of antiphlogistics before the caustic. Eight to fifteen days are sufficient for the paste to fall together with the cancerous part eliminated. If there should remain some hardened point, a second application proportioned to the circumstances, should be employed; and if it is wished to temper the corroding action of the caustic, you can incorporate with it grease in the form of unguent or pomatum. In the first days the bandaging is simple and it should be renewed once or twice a day. To establish suppuration emollient cataplasms are applied, and also basilicon ointment, until the cancerous escar falls. In case it should happen that the wound resulting has a bad appearance and the pus emits a bad odour, Landolfi uses the following ointment:

R. Terebinth 6 grammes; ol. olivæ xxx. do.; ceræ flavæ xxiv. do.; spermaceti vi. do.; pulv. pterocrrp santolini iv. do.; camphoræ ii. do.; mix and apply gentle heat in a vase constantly agitated until liquifaction takes place.

By this treatment the healing and cure of cancerous tumours of limited extension and confined to the skin, is secured in from three to four weeks; those which are planted among the ganglia require two to four months. Internal medicines are excluded during this local treatment; and a good nourishment is recommended but with abstinence from all sorts of stimulating drinks. Even when the cancer is not simply local but is accompanied by a general diathesis, Landolfi adds to the external treatment the following, prescribing morning and evening a pill composed in the following way:

R. Brom. O gr., 0 2 pulv. sem. phelland. aquat. ext. conii, each O.gr., 50. Make of this one pill.

This method is not applicable except in those cases of cancer in which there is only slight extension of the disease limited to the skin or to certain lymphatic ganglia and accessible to the action of the caustic. Landolfi indeed never wishes to attempt the cure of vast cancerous diseases, or which are complicated with considerable waste of the organs. It remains to be seen if with this method we will have returns, and if the paste is preferable to the much vaunted one of *Canquoin* for the cure of carcinoma, which is nothing else than a mixture of the chloruret of zinc with butter of antimony. If the paste of Ludolfi has veritably a superior virtue to that of the other caustics already known and experi-

mented with in the cure of cancer, it seems that it will owe its influence to the combination of the two chorurets (chlorides) of gold and bromine.

MIDWIFERY.

On the danger of Jaundice in Pregnant Women. By M. Carpertier, Rouboix. (Translated for Charleston Journal from *Revue Medico-Chirurgicale de Paris*.) For several years numerous cases of Jaundice have been noticed in our country; but in most of these cases, this disease was without importance, and they recovered in the course of twelve weeks, but it is not the same in pregnant women. I have noticed that all those who are delivered during the progress of this disease, sink in one or two days after, with the gravest of cerebral phenomena. Eleven cases of this kind have come under my notice; and I will relate the most interesting.

CASE 1.—On the 9th of May, 1852, Mary Veroeghe, enjoying good health, and in the seventh month of her pregnancy, was walking in the square of Rouboix with her child, 4 years of age. This was a festal day; there was a vast concourse of people. All of a sudden she perceived that her son had disappeared in the crowd. This woman, a prey to the liveliest emotions, commenced searching for her child, which she did not find for three hours after. The following day Mary V. was troubled with a general uneasiness, which obliged her to keep her bed. Two days after, it was noticed that the conjunctivas and the face presented the appearance of well marked jaundice. The relations of the patient, far from being alarmed, were, on the contrary, perfectly satisfied with her condition, and did not even think of sending for a physician. It was only on the 12th, toward evening, that I was called to this woman (for the first time) who had been for several hours in the pain of labor. On arriving, I found the labor far advanced, and I thought it my duty to warn her husband of the serious fears which I entertained with regard to the issue of this disease. (I ought here to remark that I had already been witness of several similar cases which had terminated fatally.) My worst predictions, unfortunately, were soon realized. Mary V. passed a part of the night in drowsiness. In the morning, when I arrived, she was in a delirium, and had lost consciousness. All these symptoms were soon aggravated, and in the evening, towards 10 o'clock, this woman expired in a profound coma.

CASE 2.—Julia Deltour, pregnant for eight months, had experienced some domestic sorrows. This woman was taken with jaundice, which, however, did not alarm her relations. The pain of labor came on, and they obtained the assistance of M^{me}. Boyaval, an intelligent midwife, who recognized a case of jaundice well marked, but attached to it very little importance. The labor was soon terminated, and they rejoiced at this happy delivery. The following morning there was no change in Julia, who had slept all night. It was only towards evening, at eight o'clock, that they perceived that what had been taken for a calm sleep, was a state of drowsiness. This woman was delirious, and had lost con-

sciousness. They sent for the midwife, and it was then that I was first called. On my arrival, I discovered that cerebral symptoms of the gravest nature, already existed. The jaws were closed tightly, the eyes strongly convulsed, the breathing slightly stertorous. I announced immediately that there was no hope, and remarked to the midwife, that I had met several cases of this kind, and that I had always had to deplore the death of the patient. She died in the night, with all the symptoms of encephalite.

CASE 3.—The 18th Sept., 1853, Mdme. Boyaval, midwife, was called to Mary Dathoit, to assist in her delivery. On the road, she learned from the husband, who accompanied her, that this woman had had the jaundice for five days, and that she was only seven months in pregnancy. She remembered immediately the unfortunate case of which she had just been witness, and those of which I had spoken to her. She told the husband of the danger his wife would encounter, and requested immediately the aid of an accoucheur. They applied to Dr. G., who went immediately. By the time of his arrival, Mary D. had already been delivered, and the Doctor appeared to be very much astonished that they should trouble him for such a natural case. Mdme. B. explained to him her motive, and made known to him her fear. She observed to him that for some months past, several women attacked with jaundice had died after delivery. Dr. G. could not keep from laughing. He persuaded her that her fears were groundless, and they retired perfectly satisfied with the condition of Mary D. The following morning, at 10 o'clock, they returned. They were told that the patient had slept profoundly all night. This was judged a favorable omen by the doctor, who prescribed a panado, and who again quieted the midwife's fears of the evening previous. Twelve hours after their departure, it was perceived that this apparent sleep was only drowsiness. Mary D. recognized no one, and replied to no questions that were asked her. They again called in Dr. G., who now no longer wore a smile on his countenance, but appeared to be deeply impressed by such a sudden change. He had recourse immediately to an energetic treatment, but despite all his efforts, the symptoms continued to be aggravated, and the poor woman succumbed in the night to the most alarming cerebral phenomena.

CASE 4.—On the 8th January, 1854, Celestine Sogordo, a *primaissare*, in the eighth month of her pregnancy, applied to Mdme. Boyaval, midwife, to be bled. She said that she had enjoyed good health during her pregnancy, and attributed this derangement of it to a violent passion in which she had indulged 15 days previously. Mdme. Boyaval was certain that it was a well marked case of jaundice, and noticed that this woman had a tottering step like an inebriate. She determined upon

dency to sleep, and it was even necessary to urge the patient to obtain answers to questions. I practised manipulation, and observed by and by the commencement of dilation of the neck, that the accouchment would terminate during the day. She was bled the second time, and I warned her relations of the fears I entertained with regard to the termination of this case. In the evening, at nine o'clock, Celestine S. was delivered, in the absence of the midwife, after several expulsive pains. I was again called in, and remained with her several hours. I soon perceived that her sleep became more and more profound. I ordered an application of leeches to the mastoid processes, and revulsives to her inferior extremities. All these means effected no change. In the morning I found the patient without consciousness; and the following night, she expired, like the others, in terrible agony.

However, can we assert that jaundice was of itself the cause of this fatal issue in the cases of the women who were the subjects of these observations? or ought we not to suppose that the cause under the influence of which the jaundice was developed, had profoundly and mortally affected the nervous system? I am inclined to the latter hypothesis; although in several cases, it was impossible for me to attribute this disease to any violent moral shock. Whatever it may be, I regard jaundice as one of the gravest symptoms in pregnant women; and I am certain that the greater part of my confreres who will read my treatise, will remember of having met with more than one case of this kind, and will agree in opinion with me.

Researches on the sudden deaths which occur in the Puerperal State. By Dr. A. H. McClintock. (Dublin Medical Press.) Sudden death, either immediately or a few days after delivery, is not so rare as the silence of physicians on the subject might seem to indicate. As early as 1814, Ramsbotham called the attention of the profession to sudden deaths (*Medical Repository*) and on cases of syncope after delivery. M. McClintock, last year, in two memoirs, brought forward the facts cited by English authors, and cases which had been under his own observation. Of these cases, which we cannot reproduce or even analyze, he concludes that sudden death might be attributed, in the puerperal state, to the following causes: 1st. Idiopathic asphyxia, a disease mentioned by Mr. Chevalier, (first volume of *Med. Chir. Transactions*), who reports the case of a woman dying suddenly three hours after being delivered of twins, and in whom no other alteration was found than the flaccidity of the heart and the emptiness of the cavities, lesions of idiopathic asphyxia. 2d. Shock of the nervous system. It is known that great pain, by itself, causes death. It happens sometimes after very tedious labors. 3d. Syncope. 4th. Strong mental emotions. Many remarkable cases are reported in which the death could be attributed to the great fears which the patients felt for the issue of the delivery. 5th. The presence of air in the uterine veins and in the heart, facts already mentioned by Legallois and confirmed by the recent researches of McCormack. Of the seven cases, reported by this author, air was detected in the veins six times, and without any other sign which could account for the death. 6th. The formation of a clot in the heart. It is

the opinion of Professor Meigs, of Philadelphia, that a clot is formed in the heart under the influence of syncope, much more readily in a woman who has lost much blood during labor, than in any other. He substantiates it with facts. 7th. Obstruction of the Pulmonary vein, a frequent cause of sudden death, according to Mr. Paget. A woman aged 34, delivered naturally of her second child, was seized with palpitations, and died suddenly on the twelfth day, whilst perfectly well to all appearances. A post mortem showed that the pulmonary arteries were completely obliterated by clots, which were solid, adherent at certain points, and which were an inch and a quarter long. 8th. Phlegmasia. 9th. Diseases of the heart.

In conclusion, we will cite a fact which does not enter into the preceding classification, and which shows that after a long and difficult labor an inconsiderable loss of blood is sufficient sometimes to produce death. In the case of a woman already the mother of five children, the labor lasted three days and three nights, and produced very great debility; the delivery had to be accomplished with the forceps. The uterus did not contract well and there was a slight flow of blood, which seemed to ease the patient; but twenty minutes afterwards she began to complain and was restless. The uterus was again pressed upon, and clots of blood were again expelled; prostration and extreme agitation soon succeeded and carried off the patient at the end of an hour and a half, in spite of the employment of stimulants. Death in this case had been attributed to the small quantity of blood lost, which, under ordinary circumstances, would have been altogether insufficient to produce so terrible an effect.

New Operation for Vesico-Vaginal Fistula. By Dr. Wagner. (Medical Times & Gazette.) Dr. Wagner has operated upon two cases of vesico-vaginal fistula since we have been in Dantzic; one healed after the first operation, but an incontinence of urine remained. The patient was not to be prevailed upon to remain in the hospital, so that no further treatment could be resorted to. Langenbeck and Wagner both treat vesico-vaginal fistula on a principle first introduced into plastic surgery by Langenbeck himself. Its application to vesico-vaginal fistula will, however, scarcely be intelligible without a detailed explanation, which I find it rather difficult to offer.

The margins of the fistula are left untouched; the operation is commenced by performing two semilunar incisions meeting at both extremities, so as to surround the whole periphery of the defect. These incisions are made about a line distant from the edge of the fistula, (or even further, according to the extent of the cicatrix tissue,) and penetrate through the whole depth of the mucous membrane of the vagina. The operator then separates the mucous membrane from the submucous fibro areolar tissue to the extent of a line or more all around the fistula, commencing from the incisions above mentioned, so that two flaps of mucous membrane result, one on each side.

It will be easily understood that two flaps are now formed, one on each side of the fistula: 1st. The two last mentioned, formed of mucous membrane, the free margins of which, meeting at both extremities, correspond to the incisions with which the operation commenced. I shall

call these the external or inferior flaps. 2dly. Two flaps, consisting of the other tissues of the vagina, the layer of fibro-areolar tissue with organic muscular fibres, and the external layer of fibrous tissue with numerous elastic fibres, and very vascular. The free margin of these flaps is formed by the surrounding margins of the fistula; these internal, or according to the position of the parts and the anterior wall of vagina, superior flaps, are broader than the external ones by the distance in which the first incisions were made from the margin of the fistula. It is important to remember this fact for the third stage of the operation. The sutures are then applied as follows.

The sutures at both extremities are merely passed through the flaps of mucous membrane; all the other sutures are first passed through the external flap on one side from above downwards, then through the internal flap in the same direction; the needle is then passed through the same parts on the opposite side in inverse order, from below upwards; all four flaps being thus taken up by each suture. The sutures being drawn tight, the double closure is obvious. The parts brought into close apposition are the raw margins, and raw submucous surfaces of the external flaps, and the inferior raw surface of the interior flaps, the margins of which (they being broader than the external flaps) are turned inwards towards the cavity of the bladder, and, being closely approximated, effectually prevent the urine from coming into contact with the parts below,

I will leave you to judge of the merits of this proceeding, and only add that it is not well adapted for large fistulæ, and quite impracticable in cases where the tissues surrounding the fistula are indurated to a great extent, as the first incisions must be performed on healthy mucous membrane. I do not think that a great number of cases have been operated on in this manner as yet, so that I can offer no judgment as to its comparative merits, except theoretical deductions, which certainly speak in its favor. I think that this description is the first hitherto attempted.

MICELLANEOUS.

The Legal Relations of Physicians to their Patients. By Joel Par-

answering the call and proceeding to take charge of the case?—I have found no case where this question has been litigated and settled.

Upon principle, there seems to be no good reason for holding, from the mere fact that the physician has been called and has prescribed, that there is a binding contract on his part to attend the patient until the termination of the disease. There is no contract upon the part of the patient to continue the employment until that period. He is at liberty to dismiss the physician at any time—and this without rendering a reason. He may so far recover, as not to desire the farther attendance of his physician whenever he chooses. And so it seems that the physician may, in like manner, dismiss the patient when he elects so to do, with this qualification, arising from the nature of the case,—that he gives reasonable notice of his intention to abandon the employment, so that the patient may have a proper opportunity to employ another person in his stead.

By accepting the call and undertaking the care of the case, the physician undertakes generally for the performance of a service in a reasonable manner, but with no stipulation for the continuance of his attendance for any particular period. The contract of the patient is to pay for such service as shall be rendered.

It has been decided, that an attorney having engaged in a cause, if desirous of giving up the conduct of it must give reasonable notice of his intention so to do.

And I perceive no reason why the same principle may not hold good in relation to physicians.

There seems to be a sufficient reason why the physician, after commencing an attendance generally, should not desert the patient without notice, if the case requires further treatment.

This responsibility on the part of medical men will not be a source of much difficulty.

For malpractice, a physician may in a certain case be exposed to a criminal prosecution.

There are several cases of this character against quacks and pretenders, but they have usually arisen from an attempt to commit some unlawful act, or from gross ignorance or negligence; and courts have been ready to excuse those who have erred with good intentions.

The danger of the educated physician does not lie here.

A physician is liable also, to a civil action, for not exercising sufficient skill and care in the performance of his duty to his patients.

This is a principle fraught with danger to all medical men, and one requiring their profound consideration; not that they are subjected to a greater responsibility than that imposed upon other professions and occupations, nor that they are more likely to be heedless in the performance of their duties, but because of the peculiar means which the patient has above all other litigants, of misrepresenting his case, and of moving the sympathies of the jurors who pass upon it.

The rules which regulate this liability are deduced from the general principles of the law, that where any person undertakes for a reward, to

perform a work for another, which requires skill for its accomplishment, he holds himself out as possessing the requisite knowledge and skill, and engages that he will exert and apply that skill in the performance of the work he undertakes. And if he does not possess the skill which is requisite, and the employer, by reason of the want of it, suffers an injury; or if possessing it he does not use it, either from wilfulness or negligence, and damage is thereby sustained by the employer,—the party who undertakes the performance of the service is liable to a suit for the recovery of the damages sustained by his incapacity, wilfulness, or negligence.

Thus, the blacksmith who undertakes to shoe a horse, and ignorantly or negligently lames him; and the mechanic who, undertaking to repair a piece of machinery, fails, from incapacity or carelessly, to perform what he agrees to do, becomes answerable for the injury occasioned by his breach of duty. And so with the attorney-at-law who, from want of a competent knowledge of his profession, or from neglect in applying his professional skill to the case committed to him, is liable at the suit of the client for the damage thereby sustained.

In like manner and upon the same principle, a person who holds himself out as a physician and assumes the care of a patient, undertakes for the necessary skill to treat the disease in a proper manner, and to prescribe the medicines suited to counteract and cure, if perchance it may be cured, or to alleviate the pain of it so far as that may reasonably be done; and if he fail in the performance of the duty thus devolved upon him by his employment, whether the failure be the result of ignorance of his profession or want of due care in his prescriptions, if injury to the system of the patient, delay in the cure, or unnecessary pain, can be shown to have resulted from his want of skill or care, the patient may have an action to recover his damages. The rule applies with full force in surgical practice; and the great danger of the medical man lies here. Actions are rare, except in cases of surgery; and in cases of that character, the plaintiff generally comes into court with some visible deformity, which, however it may have been occasioned—whether by his negligence, restlessness, or wilfulness—excites sympathy; and being attributed to the mismanagement of the defendant, he is in great danger even when he has been in no fault.

Several cases of this character have fallen under my own observation.

The inquiry naturally arises, How may the medical man best avoid the danger of loss—loss of money, and perhaps to some extent loss of reputation—through such prosecutions. It has been suggested, that before acting in any case, he should require a bond or writing exonerating him from all responsibility, or perhaps containing a direct stipulation not to prosecute a case of failure to effect a cure.

Possibly, such a document may have a tendency to restrain the party who has executed it from instituting a suit; but it admits of very grave doubt whether it could have any legal operation to exempt the physician from any responsibility.

Theoretically, he is liable only for want of skill, or negligence; practically, he is in danger from an unreasonable claim, and from the sym-

pathy of the jury. The document, to be of any avail as a legal bar to a right of action, would be in effect a contract that the physician should not be liable, even if he lacked skill or was guilty of negligence. But it has been held that it was against the policy of the law to permit a party to contract for exemption from the legal consequences of his own fraud; and gross negligence has been held equivalent to fraud.

Whether the principle would be extended farther, and held to embrace such a contract with a physician, remains to be seen.

Such a precaution may do no harm; but from the nature of the case it must be impracticable in some instances to procure such writing from the patient himself.

Another precaution in cases where danger of a suit may be apprehended, is to secure sufficient evidence of the state and progress of the case, from time to time, by an examination by other physicians and by intelligent men not of the profession, and therefore not liable to a suspicion that their testimony is influenced by an *esprit du corps*.

Another, when the patient is refractory or unreasonable, is to decline further attendance, assigning the reason, and giving reasonable time to procure another medical attendant. But whenever this course is resorted to, there should be plenary evidence respecting the state in which the case was left.

Other measures will suggest themselves as adapted, more or less, to each case as it arises. Perhaps none of them can give a perfect assurance of exemption from unreasonable prosecutions.

But something may yet be hoped from an intelligent public opinion, which shall rebuke this sponging of the profession to indemnify the patient, in many instances, for the consequences of his own folly or negligence.

Extraordinary Case of Pin-Swallowing. (London Lancet.) In September last, a girl at Vienna, laboring under an aberration of intellect, attempted to destroy her life by swallowing a quantity of pins.—The first dose consisted of 70 which she took one after the other, each pin being enveloped in a wafer; but in consequence of their smallness, they passed away without doing any mischief. Subsequently she again took to swallowing pins of a larger size, some of them two inches long. She was then seized with such severe illness that she was taken to the hospital of the town, when it was instantly detected what she was suffering from, and she was placed under a course of treatment, which had the effect of dislodging the whole of the pins in succession from the bowels. There were no less than 242 pins passed, all of them of a black color. The girl is now in a fair way of recovery.

Medical Items from Nashville Journal of Medicine & Surgery.

COPPER AND LEAD IN SODA WATER.—Prof. Doremus, of N. York, as may be seen in another department of this number of the Journal, has detected large quantities of both *copper* and *lead* in our ordinary soda, or more properly, carbonic acid waters of the shop.

LODEMENT OF MEAT IN THE ESOPHAGUS.—Dr. Thayer having succeeded, after failure, to dislodge a piece of meat arrested in the attempt,

to swallow it, by the probang, suggests a trial of the method which proved successful to him, viz: Kneading the œsophagus with the ends of the fingers, while the trachea is so secured that pressure cannot be exercised upon it. In this way the morsel of meat was made to rise into the patient's mouth. This is condensed from the Boston Medical and Surgical Journal.

PILLOW DRINKING TUBE.—We learn from our cotemporary of Boston, that a neat little article has been invented and called by the above title, for patients confined to beds on their back, by means of which they can drink without rising. It is manufactured by Mr. Burt, of Manchester, Connecticut, and is on sale at Mr. Burton's, 39 Tremont street, Boston.

DOCTORS, GOVERNORS OF OUR STATES.—Dr. Hubbard, of Hallowell, is Governor of Maine; and Dr. Martin, of Dover, is Governor of New Hampshire.

OPHTHALMIA CURED BY KEEPING THE EYELIDS CLOSED.—M. Bonnafont has found that both acute and chronic conjunctivitis, inflammation of the cornea, &c., have been relieved by closing the eyelids with adhesive plaster, filling up the orbital depression, and retaining this dressing by a black bandage. These applications are renewed every four or five days.

OXIDE OF ZINC AND HYOSCYAMUS FOR ARRESTING COLLIQUATIVE SWEATING IN PHTHISIS.—Dr. Theophilus Thompson recommends (Lectures on Phthisis) oxide of zinc and extract of hyoscyamus, of each four grains, made into two pills, to be taken at bedtime, for arresting the colliquative sweating in Phthisis.—*Am. Jour. Med. Sciences.*

EDITORIAL AND MISCELLANEOUS.

Yellow Fever.

In our September issue we announced the existence of yellow fever in Charleston, and expressed our fears that it would become in a short time epidemic. Our apprehensions have been fully realized. The universal has been its diffusion as to almost entitle it to the appellation of *Pandemic*. Nor condition, nor sex, nor age, nor race, nor geograph-

show, reached its culminating point about the 20th to the 25th of September, and declined rapidly to the end of the first week in October; when there was a perceptible increase in the number of cases as well as of deaths. It then again declined and may be said at this date (Nov. 1st) to have ceased, only a few sporadic cases occurring.

There were:—

For the week ending August 19th,	-	-	-	4 deaths.
" " " " 26th,	-	-	-	20 "
" " " " September 2d,	-	-	-	26 "
" " " " 9th,	-	-	-	78 "
" " " " 16th,	-	-	-	127 "
" " " " 23d,	-	-	-	118 "
" " " " 30th,	-	-	-	72 "
" " " " October, 7th,	-	-	-	58 "
" " " " 15th,	-	-	-	48 "
" " " " 21st,	-	-	-	31 "
" " " " 28th,	-	-	-	23 "

Total deaths to 1st November, - - - 600

The above Editorial, in the opinion of many of our physicians, should require some modification. Many believe, that old residents were either entirely, or almost without a single exception, exempt from *yellow fever*; though some, even of those, suffered from mild attacks of some form of fever. It is also the opinion of a large number, that the proportion of deaths among natives, was certainly not more than one third, or even one fourth, that of foreigners; making, of the 600, 200 natives;—a large proportion of the latter being children. The numbers, when published, will however, conclusively settle these points. Not to include the Hospitals, it has been generally conceded by Physicians, that during few preceding seasons, have a larger share of recoveries rewarded their attention, than during the past summer in this city. In private practice, so mild did many cases of fever prove under nursing and good management, that very few comparatively, of these, died. Though fever was

cently occupied the chair of the Theory and Practice of Medicine, in the University of the City of New York. Dr. Swett was a patient observer, and was recognized as one of the most experienced auscultators in this country.

In the October issue of the New York Medical Gazette, edited by Dr. D. M. Reese, it is announced that Prof. S. H. Dickson, of this city, is a candidate for the vacant chair. We take this occasion to correct the statement referred to, by denying that Dr. Dickson is a candidate for the vacant Professorship.

Mortality of the White and Black Races in the United States.

WE have been politely furnished by Mr. J. D. B. De Bow, chief of the Census Bureau at Washington, with the subjoined table exhibiting the comparative mortality of the white and black races in the United States, from the proof sheets in advance of the publication of the vital statistics in connexion with the census.

On an examination of this table one fact stands prominently forth; it is that of the greater longevity, both general and extreme, of the black than the white in the slaveholding States. This is a fact pregnant with significance; proving conclusively, as it does, that the almost complete freedom from care, the lightness of his labour, and the physical comforts generally enjoyed by the slave, combine to increase the duration of his life, not only beyond that of the *labouring white* class, but even beyond the average of the white class of *all* conditions.

But the difference between the two races, in point of longevity, would be still greater in favour of the black than is represented in the table, if we deduct a certain proportion of early deaths from the latter, occurring among the mulattoes who exist in large numbers in the cities of the slave States.

NATIVITIES OF THE INHABITANTS OF THE LEADING
CITIES OF THE UNITED STATES IN 1850.

Born in the United States.

Cities :	In city or port of arrival.	Alabama.	Delaware, District of Columbia.	Connecticut.	Delaware.	Florida.	Georgia.	Illinois.	Indiana.	Kentucky.	Louisiana.
Albany,	28738	1	2	558	13	00	3	9	4	6	5
Baltimore,	113583	26	1170	326	756	23	61	17	43	85	96
Boston,	68687	17	47	575	29	9	82	17	3	12	32
Charleston,	16066	18	9	58	1	22	182	00	4	10	11
Chicago,	5831	21	7	506	29	4	8	00	89	97	24
Cincinnati,	39322	52	127	527	215	1	105	147	1185	2096	378
Detroit,	6323	6	15	224	9	2	13	25	10	98	6
Hartford,	8923	9	4	00	7	5	14	9	4	1	4
Louisville,	16285	71	53	116	59	3	51	99	1255	00	195
Manchester,	9555	00	00	11	00	1	1	2	13	00	00
Memphis,	2134	187	18	36	4	10	96	67	94	360	56
Milwaukie,	2641	1	1	263	14	00	1	91	33	19	5
Mobile,	5507	00	18	139	5	133	355	1	14	91	228
Nashville,	4883	78	2	31	6	2	26	12	16	382	30
Newark,	21477	4	12	533	31	1	16	12	2	7	16
New-Haven,	13775	21	9	00	13	15	92	6	6	15	34
New-Orleans,	34101	620	92	228	44	139	249	147	118	1038	00
New-York,	234843	90	261	7784	393	54	277	72	41	122	303
Philadelphia,	242681	55	394	829	8678	15	123	52	62	197	163
Portland,	15110	5	15	43	8	1	6	7	1	00	8
Portsmouth,	7088	00	5	30	3	00	2	00	06	00	2
Providence,	24368	6	35	989	22	9	29	5	3	9	6
Richmond,	14138	8	40	72	14	5	8	00	00	24	4
St. Augustine,	1100	1	00	33	00	00	25	1	00	3	1
Savannah,	4774	14	8	84	9	121	00	00	2	5	6
St. Louis,	60221	69	109	325	79	10	73	1210	438	1846	360

Born in the United States.

<i>Cities :</i>	<i>Maine.</i>	<i>Maryland.</i>	<i>Massachusetts.</i>	<i>Michigan.</i>	<i>Minnesota.</i>	<i>Missouri.</i>	<i>New-Hampshire.</i>	<i>New-Jersey.</i>	<i>New-York.</i>	<i>N. Carolina.</i>
Albany, -	43	49	736	7	00	1	111	218	00	10
Baltimore, -	396	00	1192	11	121	51	215	723	2037	126
Boston, -	7689	191	00	15	5	6	6628	118	1594	47
Charleston, -	19	119	227	00	2	00	6	58	545	109
Chicago, -	203	69	480	249	28	71	170	239	3870	24
Cincinnati, -	220	1643	1079	87	189	126	225	1417	3142	169
Detroit, -	70	34	296	00	4	19	80	83	2620	15
Hartford, -	46	40	983	6	1	2	110	67	636	6
Louisville, -	45	742	219	8	69	148	39	206	777	109
Manchester, -	197	1	1012	00	00	1	00	00	82	1
Memphis, -	21	100	56	4	164	79	16	20	160	351
Milwaukie, -	156	30	350	122	5	26	83	80	2281	3
Mobile, -	100	154	289	00	230	16	39	73	701	216
Nashville, -	4	91	54	1	36	3	00	30	283	243
Newark, -	46	111	277	9	2	1	45	00	3239	20
New-Haven, -	79	57	551	5	12	8	35	270	1167	20
N. Orleans, -	620	733	1178	52	842	431	155	274	4086	214
New-York, -	1432	1852	5587	86	83	56	826	13255	00	284
Philadelphia, -	333	5760	1858	18	24	60	288	15570	4858	198
Portland, -	00	35	1208	1	00	00	474	9	142	3
Portsmouth, -	866	7	415	00	00	00	00	1	51	1
Providence, -	326	218	4003	5	4	00	321	86	760	58
Richmond, -	22	268	161	1	1	2	37	77	254	102
St. Augustine, -	6	9	8	00	00	00	3	3	5	7
Savannah, -	25	49	100	00	3	1	24	75	305	69
St. Louis, -	112	841	603	94	234	00	137	270	2470	126
Washington, -	79	7017	284	26	39	25	77	139	723	86
Wilmington, Del., -	21	765	50	00	1	2	10	525	100	10
Wilmington, N. C., -	16	26	42	00	1	1	2	8	97	00

Mortality of Black and White Races in U. States. 855

<i>Cities:</i>	<i>Born in the United States.</i>							<i>Foreign born.</i>	
	Ohio.	Pennsylvania.	R. Island.	S. Carolina.	Tennessee.	Vermont.	Virginia.	England & Wales.	Ireland.
Albany, - -	26	154	92	21	1	322	31	2082	13079
Baltimore, - -	276	4986	181	122	26	211	3605	2133	12057
Boston, - -	69	393	584	94	6	1744	251	3213	35287
Charleston, - -	2	165	38	00	9	13	115	546	2369
Chicago, - -	390	545	46	5	18	456	119	1883	6096
Cincinnati, - -	00	5112	143	112	245	250	2178	4135	14393
Detroit, - -	305	276	21	12	24	281	161	1245	3289
Hartford, - -	25	66	67	7	3	114	12	235	2188
Louisville, - -	1090	1365	22	72	306	42	1582	720	3105
Manchester, - -	00	1	15	00	00	1343	8	182	1193
Memphis, - -	150	194	5	131	00	7	419	133	704
Milwaukie, - -	334	814	35	16	4	225	42	1212	2816
Mobile, - -	86	237	24	459	75	32	330	547	2009
Nashville, - -	106	178	4	61	00	8	608	137	421
Newark, - -	41	504	38	25	1	45	43	2124	5564
New-Haven, - -	57	212	47	21	4	63	39	371	2772
New-Orleans, - -	781	1515	174	502	484	133	1232	2670	20200
New-York, - -	499	5283	961	535	26	953	1702	23671	133730
Philadelphia, - -	505	00	288	470	85	157	2602	17500	72312
Portland, - -	1	49	32	6	00	78	23	156	2301
Portsmouth, - -	6	15	9	3	1	27	7	343	523
Providence, - -	30	170	00	20	1	134	99	1119	7635
Richmond, - -	12	206	9	43	11	16	00	268	685
St. Augustine, - -	00	3	9	19	1	1	6	7	11
Savannah, - -	6	87	26	720	6	9	60	227	1555
St. Louis, - -	1638	2684	45	73	380	176	1630	2957	9719
Washington, - -	114	1000	19	71	54	40	4046	585	2023
Wilmington, Del., - -	15	1908	2	1	1	2	56	240	1215
Wilmington, N. C.,	1	34	15	57	00	1	89	33	63

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Cities :	Foreign Born.							Aggregate.	
	Scotland.	Germany.	Prussia.	Austria.	France.	Spain.	Italy.	Native.*	Foreign.*
Albany, -	540	2875	1	00	97	2	1	31162	16591
Baltimore, -	525	19274	164	16	346	16	67	130491	35492
Boston, -	897	1777	39	00	225	67	134	88948	46677
Charleston, -	323	1789	27	1	187	23	56	17809	4643
Chicago, -	610	5035	38	21	234	2	4	13693	15682
Cincinnati, -	718	33374	148	18	797	4	152	60558	54541
Detroit, -	474	2838	18	7	282	4	4	11055	9927
Hartford, -	58	271	12	13	27	00	4	10551	2915
Louisville, -	162	7357	145	24	422	4	112	25079	12461
Manchester, -	31	00	00	00	1	00	00	12244	1688
Memphis, -	20	341	8	1	69	1	44	5026	1401
Milwaukie, -	245	6028	1243	16	129	00	00	7180	12782
Mobile, -	205	513	22	17	303	144	65	9565	4086
Nashville, -	70	193	15	00	36	1	3	7185	948
Newark, -	265	3818	4	6	240	8	00	26561	12322
New-Haven, -	107	273	11	00	54	00	3	16641	3697
New-Orleans, -	854	11220	205	129	7522	1150	658	50470	48601
New-York, -	7660	55476	665	109	4990	803	708	277752	235733
Philadelphia, -	3291	22750	270	84	1981	291	236	286346	121699
Portland, -	50	22	14	2	14	00	6	17265	3512
Portsmouth, -	62	25	1	00	6	00	00	8540	1179
Providence, -	322	87	2	1	40	10	19	31755	9679
Richmond, -	183	740	18	2	68	5	34	15541	2102
St. Augustine, -	4	5	00	00	6	3	1	1244	56
Savannah, -	60	383	3	7	37	13	15	6590	2434
St. Louis, -	550	22340	231	13	682	36	101	36529	38397
Washington, -	136	1246	10	1	69	22	49	33530	4282
Wilmington, Del., -	24	157	26	00	16	1	00	12198	1763
Wilmington, N.C., -	14	72	00	1	9	00	00	4025	208

NOTE.—Exclusive of 5 Arkansians in Baltimore, 25 in Cincinnati, 26 in Louisville, 80 in Memphis, 97 in New-Orleans and 39 in St. Louis, &c.; 4 Californians in New-York; 19 Iowans in Chicago, 24 in Cincinnati, 10 in Louisville, 5 in Memphis, 6 in Milwaukie; 21 in New-Orleans, 4 in New-York, 7 in Philadelphia, and 77 in St. Louis; 19 Texans in Baltimore, 9 in Cincinnati, 8 in Hartford, 9 in Louis-

Mortality of Black and White Races in U. States. 857

AGES OF PERSONS WHO DIED BETWEEN JUNE 1, 1849,
AND JUNE 1, 1850.

States and Territories.	Under 90																	
	Under 1.	1 and under 5.	5 and under 10.	10 and under 15.	15 and under 20.	20 and under 30.	30 and under 40.	40 and under 50.	50 and under 60.	60 and under 70.	70 and under 80.	Under 90.						
Alabama,	W 839	811	394	271	245	482	402	285	241	191	138		W	1184	1279	343	202	184
Arkansas,	W 390	453	212	106	134	264	224	168	96	67	30		W	184	206	83	39	134
California,	W 36	38	10	13	26	125	71	60	30	34	13		W	1	00	00	00	00
Colum, Dis. of	W 110	148	42	22	28	51	55	40	33	24	26		W	44	89	21	10	110
Connecticut,	W 684	905	282	188	228	591	487	423	406	479	490	3	W	21	22	6	5	21
Delaware,	W 186	184	60	37	32	74	91	69	77	61	53		W	53	63	12	12	186
Florida,	W 62	120	41	25	28	55	51	34	21	22	8		W	85	128	34	17	62
Georgia,	W 890	830	230	230	270	490	380	292	289	260	217	1	W	1393	1330	296	238	890
Illinois,	W 2261	2630	753	492	617	1416	1138	931	630	430	239		W	9	10	4	3	2261
Indiana,	W 2247	2748	968	823	862	1483	1029	818	31	477	341	1	W	22	21	14	6	2247
Iowa,	W 446	539	141	81	81	235	195	129	82	66	27		W	00	3	00	00	446
Kentucky,	W 1743	2074	655	456	618	1359	1006	748	623	540	445	2	W	967	906	286	257	1743

States and Territories.	Under 1.													
	Under 1.	Under 5.	1 and under 10.	5 and under 15.	10 and under 20.	15 and under 30.	20 and under 40.	30 and under 50.	40 and under 60.	50 and under 70.	60 and under 80.	70 and under 90.	80 and under 100.	
Louisiana,	w	538	762	342	176	286	1324	1156	667	322	160	83	29	
	c	737	1302	435	279	826	851	778	535	320	237	90	62	
Maine,	w	910	1584	519	260	454	921	631	497	403	469	462	350	
	c	9	8	2	00	3	5	3	4	1	1	2	1	
Maryland,	w	1536	1555	454	475	00	579	473	492	412	383	352	151	
	c	554	575	209	277	00	253	197	172	143	127	100	73	
Massachusetts,	w	2833	4380	1064	684	725	2203	1801	1309	1070	1138	1078	793	
	c	9	25	6	6	9	23	11	15	17	9	5	5	
Michigan,	w	851	995	293	177	198	493	408	312	280	227	165	61	
	c	5	13	2	3	2	5	3	1	3	00	1	00	
Mississippi,	w	569	746	288	188	170	419	283	233	157	145	80	53	
	c	1270	1502	375	248	256	543	408	296	164	152	62	36	
Missouri,	w	1632	2179	750	465	564	1726	1366	891	562	363	187	53	
	c	322	309	100	92	91	188	120	75	51	39	29	8	
N. Hampshire,	w	450	760	240	146	202	466	333	282	275	299	389	318	
	c	1	1	1	1	00	00	00	1	00	00	00	00	
New-Jersey,	w	1040	1282	344	185	232	632	472	469	421	438	366	245	
	c	41	47	17	14	21	33	34	21	24	18	18	15	
New-York,	w	6628	9758	2889	1387	1834	5404	4369	3584	2608	2351	2098	1341	
	c	80	141	53	30	49	133	102	99	62	65	44	30	
N. Carolina,	w	853	748	366	276	309	636	502	513	326	417	354	206	
	c	1059	1147	264	217	272	415	294	216	243	234	192	140	
Ohio,	w	4363	6553	2252	1098	1327	3464	2753	2115	1523	1369	967	52	
	c	57	97	27	19	33	65	32	29	24	13	15	1	

[illegible]

NOTE.—Average age of white deceased 25.45 years, colored 21.39, supposing those under 10 to have lived 6 months, and those between 1 and 5, 3 years, &c., which is not true in point of fact, but sufficiently so for the comparison.

Free colored in Alabama 28, District of Columbia 204, Delaware 241, Florida 8, Georgia 46, Kentucky 184, Louisiana 165, Maryland 1220, Mississippi 15, Missouri 83, New-Jersey 304, North-Carolina 463, South-Carolina 81, Tennessee 125, Virginia 801. In Alabama 5 slaves died aged 105, 2 120; Arkansas 1 white 105, and 1 slave 110; District of Columbia 1 slave 103; Delaware 1 free colored 100, and 1 107; Georgia 1 white 105, 1 107, 3 slaves 103, 2 105, 1 120, 1 121; Michigan 1 white 103, 1 110; New-Jersey 1 colored 114, 1 109; New-York 1 white 102, 1 103, 1 free colored 104, 1 106, 2 110, 1 113; North-Carolina 1 Indian female 140, 1 slave 120; Texas 1 slave 115.

Meteorological Observations made at Aiken, in South-Carolina,
for the month of August, 1854, by H. W. RAVENEL.

Days.	Rain and Melted Snow.			Psychrometer.						Daily Remarks on the Weather.
				Dry bulb.			Wet bulb.			
	Hour began	Hour ended.	Am't inches.	T A. M.	2 P. M.	9 P. M.	T A. M.	2 P. M.	9 P. M.	
1				83	95	88	77	80	80	clear.
2				83	94	90	78	82	80	clear.
3				84	96	91	79	81	82	clear--thermometer at 5 P. M. 99°. [af.
4			0.061	80	91	83	70	81	74	light show. before daylight; cl'r m'g. eldy.
5			0.060	76	88	86	73	76	78	light shower before daylight; clear day.
6				80	90	86	76	73	77	clear.
7				79	91	85	75	75	75	clear.
8				75	78	77	70	71	72	cloudy all day.
9				71	81	79	67	70	71	variable morning; clears off.
10				74	85	78	70	74	73	variable; clears off.
11				76	88	83	73	75	75	clear.
12			0.258	77	81	78	74	74	75	rain about mid-day, cloudy afternoon.
13				76	90	84	73	77	75	clear.
14			0.145	79	94	81	74	78	75	clear morning, light rain in afternoon.
15			0.215	78	92	82	74	79	77	do. do. do. do.
16			0.140	78	89	77	75	76	74	do. do. do. do.
17				75	89	83	72	73	76	clear morning, variable in afternoon.
18	P. M. 2.00		0.550	77	82	76	74	76	74	cloudy, rain in afternoon.
19				71	73	72	69	69	69	cloudy all day.
20				68	79	78	66	73	74	clear.
21				72	84	79	69	72	73	clear.
22				74	84	80	71	72	75	clear.
23				74	86	84	70	74	77	clear.
24				77	89	83	74	76	77	clear.
25				78	91	85	73	74	77	clear.
26				78	94	86	74	77	76	clear.
27				80	94	84	74	76	76	clear, distant thunder in afternoon.
28					89	77		75	78	variable, distant thunder and light shower.
29				76	78	76	72	72	70	cloudy morning; clears off,
30				70	80	73	67	72	66	clear.
31				70	83	77	67	73	71	clear.
1.429										

Meteorological Observations made at Aiken, in South-Carolina, for the month of September, 1854, by H. W. RAVENEL.

Days.	Rain and Melted Snow.	Psychrometer.						Daily Remarks on the Weather.
		Dry bulb.		Wet bulb.				
		7 A. M.	9 P. M.	7 A. M.	9 P. M.	9 P. M.		
1		76	84	81	74	76	74	morning variable; light shower; clears off,
2		77	86	75	74	77	72	clear.
3		74	89	83	71	75	71	clear.
4		78	94	85	73	75	77	clear, with floating clouds; very dry.
5		80	95	84	73	76	76	do. do. do. do. do.
6		80	92	81	75	78	72	do. do. do. do. do.
7		76	90	81	71	76	76	do. do. do. do. do. [stormy.
8	0.815	77	80	76	71	74	74	Overcast with fly'g cl'ds all day; w. high and
9	1.967	74	77	74	72	75	73	r. all day; w. high fm. N. E. sh'g rd. to S. W.
10		78	90	87	75	82	82	clear. [at night.
11	0.575	77	88	75	74	81	73	clear morning, thunder storm in af. with rain
12		75	85	80	73	78	76	clear.
13		80	88	80	77	78	75	clear.
14		76	88	81	74	75	75	clear. [with light shower.
15	0.137	78	87	76	75	77	73	clear morning, thunder cloud in afternoon
16	0.183	75	76	75	72	72	70	variable, light shower about mid-day.
17		68	77	69	64	67	60	clear and cool.
18		64	79	73	60	70	69	clear, becomes cloudy in afternoon.
19	0.430	70	83	77	67	77	75	morning variable, showery in afternoon.
20	0.790	73	79	74	72	74	71	variable, light showers, rain at night.
21	0.137	70	68	62	68	63	60	overcast with rain most of the day.
22		60	71	64	57	64	59	clear.
23		58	72	64	54	61	56	clear.
24		59	73	71	55	67	68	clear morning, becomes cloudy in afternoon.
25	0.018	67	76	72	65	72	68	cloudy, with light rain in morning; clears off.
26	1.120	70	73	70	69	71	68	rain all the morning.
27		67	78	75	66	72	73	cloudy and variable, light rain at night.
28		71	80	74	69	74	70	clear.
29	0.087	71	84	75	69	73	71	clear morning, shower in afternoon.
30		67	75	65	61	63	58	clear.
6.259								

Remarks on the storm of the 8th and 9th.—The day of the 7th was clear, with a few floating clouds and a brisk, pleasant breeze from N. E. At night, light hazy clouds began to cover the sky, and the ring around the moon was very distinct, announcing a change. After midnight the wind increased, and about 3 A. M. of the morning of the 8th, it began to blow in puffs with showers of rain; the appearance of the weather throughout the 8th was threatening, and announced an approaching gale, wind increasing and coming in puffs, accompanied with mists, rain or spray at intervals—this continued through the night. On the 9th the wind increased in violence and blew more steadily, attended with constant rain until the afternoon—wind veering round in that time from N. E. to S. E., S. and S. W. Towards night the wind abated and the rain ceased, and by 9 P. M. the stars began to be visible. Next morning the sun rose clear.

COMMUNICATION FROM DR. NOTT,

OF MOBILE, ALA.

Messrs. Editors.—As Dr. Bachman's "*Notice of the Types of Mankind*," or "*Types of Infidelity*," as he is pleased to term this work, was, in my estimation, calculated to harm no one but himself, I had fully made up my mind to pass it with the silence which it deserves. Those who have, during the last few years, perused the writings of the parties alluded to in this "*Notice*," cannot be misguided by the garbling and special pleading of the Doctor, and I have no disposition to follow him in his "warfare" of personalities. "Filthy trash, I'll touch it not."

A letter, just received, from Mr. Gliddon, who is on the eve of departure for Europe, has changed my determination. He informs me, that he deems it due to the memories of Patterson and Morton, that he should reply to Dr. Bachman, at his earliest leisure, and that he has written you a note to that effect. It becomes proper, therefore, that I should say a few words, in explanation of my own connexion with this affair, and thus leave Mr. Gliddon free to manage the rest as may seem fit to him.

The reader need not be reminded that, in 1850, Dr. B. published a book, which he entitled, "*The Doctrine of the Unity of the Human Race, examined on the principles of Science*," but which is, perhaps, more strongly tinged with the "principles" of bigotry and fanaticism. Outside of a labored, I may say interesting, resumé of the facts connected with Hybridity, which can never materially effect the great point at issue, this volume has very little to recommend it. It is full of bold assertions, palpable contradictions, false reasoning, which deserved exposure. It, moreover, contained the following unprovoked and insulting language towards me. I say unprovoked, for, up to the time of the publication of his book, Dr. Bachman was unknown in this discussion. I had never alluded to him, directly or indirectly, in any thing I had written, and knew nothing more of him than, that there was a Clergyman of the name of Bachman, in Charleston. On page 36, of his book, he says, after enumerating authorities on his side of the question:

"And who are the opposite side. Virey, who pronounces the negro, 'undoubtedly a distinct species from the beginning of the world,' and divides mankind into two species, the white and the black, and suspected a certain fraternity between the Hottentot and the Baboon—Desmoulins divided men into 11 species—Borey into 15, and Broc into several sub-genera, and an infinite number of species—and finally, an American, (Dr. Nott,) who can see no reason why we should not make an hundred species of men. There are, no doubt, others, who have more recently entered into the field, whose works we have not seen. The Frenchmen belonged to a school of skeptics, of which Voltaire was at the head, and we may be allowed to suspect, that their minds have been biassed by the cheerless doctrine which they had imbibed."

"The American," (Dr. Nott,) "who seems recently to have labored, rather to deny the historical veracity of the Scriptures, than to prove the diversity of the

acquainted with those characteristics on which genera and species are formed—hence, they all disagree among themselves.”*

In his “Notice,” just published, the Doctor says: “As soon as our work on the ‘Unity of the Races’ appeared, he (Dr. Nott,) showed his hand, and commenced a warfare which, to all appearances, will not be brought to a close during his life-time. His *first attack*, as far as we know, was commenced in De Bow’s Review, February, 1851.”

The reader can now see who “*commenced the warfare.*” I reviewed his book, in the Journal alluded to, and, if the reader will refer back to it, he will see an exposure of some of the absurdities of the Doctor, which has not served much to improve his Christian spirit.

There, in reality, should end my connexion with Dr. Bachman, and with this controversy, which *he* has waged; but he not only holds me responsible for Patterson’s Memoir of Morton, but charges me with being, at least, in part the author. It is this memoir which now so stirs up his ire, and against which he mainly directs his attack. This memoir requires no defence, nor will I attempt one—it can stand on its own merits. I shall merely explain the extent of my connexion with it, to show the injustice and folly of Dr. Bachman’s attack on me. The following is the history of the memoir, and, I may say in advance, that I never saw one line of it before it was sent to me in the print, and that I never exchanged a line with Dr. Patterson in my life, nor did I ever see him.

After I had completed my part of the work on the “Types of Mankind,” Mr. Gliddon took the MSS. on to Philadelphia, to finish them, and look for a publisher. We thought it would be proper to prefix to the volume, a short memoir of Dr. Morton, and hoping to find some one more competent, and of more leisure than ourselves, he carried on, with him, our private correspondence with Morton, and what other materials we had at command. Dr. Patterson consented to write the memoir, and we were much gratified that he did so—he was a man of known talent and attainment, an active member of the academy to which Morton was attached,—withal, his fellow-townsmen and intimate friend, and it was generally conceded, in Philadelphia, that we could not have made a more fortunate selection. Our materials were placed in his hands, and the memoir was written in Philadelphia, while I was busily engaged in my profession in Mobile.

Dr. Patterson published some of my letters, which I should not have had the egotism to print, nor, had I written the memoir, should I have alluded to Dr. Bachman at all. So much for my agency in this matter.

* A reference to the following title pages of some of the works of Virey—Borey de St. Vincent—Broc—and Desmoulins, whom Dr. B. so complacently assures us, were utterly “unknown as Naturalists,” will show how they ranked in France.

“*Historie Naturelle du Genre Humain.* par J. J. Virey, Docteur en Médecine de

It is but justice to myself to state that, in every line I have written, in "Types of Mankind," I avoided all personalities, and I attempted to make it strictly Scientific. Mr. Gliddon thought proper to pursue a different course—he had submitted, quietly, to insults for years, from sectarians, or, as Mrs. Partington would say, from those whose business it was to "dispense with the Gospel," and the time had come for him to pay off old scores—he has done it, it must be confessed, *con amore*, and like John Randolph, it is clear he would go, any time, "a quarter of a mile to kick a sheep," with the bare hope of finding a wolf concealed under his skin.

Dr. Bachman complains that I, in one place, misquote him by substituting the word "may" for "might"—this was unintentional—that I abbreviated one of his titles, by which he is made an "American Ass"—this, I assure him, was a mistake of the printer, which I never saw until it was pointed out to me. And again, that I accuse him of "seeing through coloured glasses." In making this charge, I had no idea of accusing him of falsehood, but simply of being blinded by his unfortunate temper and prejudices, and his "Notice" makes good this charge.

When my offence against Dr. Bachman is narrowed down to the simple truth, it is this—I have published scientific opinions, which conflict with his sectarian dogmas. In 1849, I published my "*Biblical and Physical history of Man*," in which I advocated Plurality of Origins for the human Races; and although I disclaimed, honestly, any desire to attack the Bible, this could not save me from his bigotry and venom. Here is a paragraph from my Introduction.

"As the ground we are compelled to travel over, in this discussion, brings us directly in collision with the religious prejudices of a very large portion of the community, for whose opinions we have the highest respect, and as we do not wish to be placed in a false position, we beg leave, at the outset, to disclaim any intention, or feeling, which would prompt us to call in question the well-authenticated and important parts of the Scriptures, or to deny their inestimable value to man. We beg leave further to state, that so far from appealing to the authority of implied writers, we shall advance no opinion in relation to the *texts*, or interpretations of the Old and New Testaments, which is not based on the researches of the most learned and authoritative theologians of the present day."

Dr. Bachman charges me with being "a wrangler,"—but I challenge him to point to one instance where I have been the aggressor. His attacks upon Dr. Morton and myself, have certainly been unprovoked and unwarrantable. He has even so far forgotten himself, as to charge me with *falsehood* and *forgery*! I will only reply, that I hope the utterance has afforded to him infinitely more pleasure, than it has pain to me.

Very respectfully, yours, &c.,

J. C. NOTT, M.D.

Mobile, Alabama.

TO READERS, PUBLISHERS, &c.

THE following books and pamphlets have been received :

Manual of Human Microscopical Anatomy. By A. Kölliker, Prof. of Anat. and Physiology in Wurzburg. Translated by G. Busk, F.R.S. and Thomas Huxley, F.R.S. Edited with notes and additions, by J. De Costa, M.D. Illustrated by 313 engravings on wood. Philadelphia: Lippincott, Grambo & Co. 1864. (From publishers. For sale by C. S. Burdett, No. 5 Haver street.)

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